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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

ONE of the most remarkable things about the last number published of the *Revue semestrielle des publications mathématiques* (1917, **25** [2]) is the exceedingly small space devoted to publications from Germany. In fact, though the papers in mathematics from October 1916 to April 1917 are supposed to be indexed in the number referred to, only just over three pages out of the sixty-four are devoted to an account of recent German work, and some of this was published in 1915. The case is apparently better as regards Austria-Hungary, for about ten pages are devoted to mathematical literature from that Empire,—but most of these papers were published in 1915. It is probably unsafe to conclude from this fact that the actual production of mathematical work in the Central Empires has been very much smaller of late than in the rest of the world, but still the fact is remarkable.

Educational Note.—A. F. Frumveller (*Amer. Math. Monthly*, 1917, **24**, 409–20) gives a full development, with many illustrations, of the making of graphs of $f(x)$ for complex numbers, which is of interest to teachers when they have to teach the theory of functions.

History.—K. Sethe (*Schr. der wiss. Ges. zu Strassburg*, 1916, **25**) studies the art of calculation with the ancient Egyptians.

A. C. Bose (*Bull. Calcutta Math. Soc.* 1916, **6**, 13–31) continues an account of John Napier's life and work.

G. Milhaud (*Scientia*, 1918, **23**, 1–8, 77–90) gives a very valuable sketch of the work of Descartes during the memorable winter of 1619–20, which is based on both the known documents and some very likely conjectures. The first part is on the discovery of Descartes' Method and his ideal of a "Mathesis," and the second part is on his first work in analysis and geometry. Cf. also the papers of Milhaud in *Scientia* (1916,

19 ; 1917, **21**, 185–98), *Rev. de Métaphys. et de Morale* (1916, **23**, 607–21), *Rev. gén. des Sci.* (1916, **27**, 502–10), and those referred to in SCIENCE PROGRESS (1918, **12**, 362–3).

The thirteenth volume of Christiaan Huygens's *Œuvres Complètes*, containing his work on dioptrics of 1653, 1666, and 1685–92, was published at The Hague in 1916.

F. Cajori (*Colorado College Publication, Engineering Series*, 1917, **1**, 245–53) has discovered an account of Newton's use of logarithmic slide rules in securing rough approximations to the roots of numerical equations in James Wilson's *Mathematical Tracts of the late Benjamin Robins* of 1761, and reproduces, with some useful comment, this account as well as the two other known ones (in Oldenburg's letter of June 24, 1675, to Leibniz, and E. Stone's article in his *New Mathematical Dictionary* of 1743). Cajori remarks in passing that there are still manuscripts of Newton which have never been minutely examined and have not been published. Surely there is some reason to hope that this may be rectified after the war.

G. A. Johnston (*Monist*, 1918, **28**, 25–45) makes a most valuable contribution to the history of mathematical philosophy by bringing forward the doctrines stated in Berkeley's early "Commonplace Book." His *Analyst* (1734) is also considered in a very illuminating way. (Cf. also the note on some work by Cajori in SCIENCE PROGRESS, 1917, **12**, 191.)

A. C. Bose (*Bull. Calcutta Math. Soc.* 1917, **7**, 33–48) writes on Fourier's life and work, and Philip E. B. Jourdain (*Scientia*, 1917, **22**, 245–54) considers the influence of Fourier's work on the formation of conceptions in pure mathematics during the nineteenth century.

L. Mongardon (*L'Interméd. des Math.* 1916, **23**, 180–82, 199) gives notes for a bibliography of the works of Wronski.

H. P. Banerji (*Bull. Calcutta Math. Soc.* 1917, **8**, 53–6) writes on Sophie Kowalewski.

J. H. Graf (*Mitt. der Naturf. Ges. in Bern*, 1915, 50–69) gives the correspondence of Ludwig Schläfli and C. W. Borchardt from 1856–77.

Logic, Principles, and Theory of Aggregates.—W. E. Johnson (*Mind*, 1918, **27**, 1–21, 133–51) puts forward the view that the preliminary treatment of thinking should be exactly the same in both psychology and logic. This has of course an extremely important bearing on the principles of mathematics.

R. Demos (*ibid.* 77–85), following up his reduction of negative propositions to descriptions of certain true propositions in terms of their opposition to some other proposition (cf. SCIENCE PROGRESS, 1917, **12**, 192), applies an analogous reduction to modal propositions and propositions of practice. Demos's investigations are of great interest to those concerned with the principles of mathematics, because they give examples of an extension to certain propositions of B. Russell's theory of descriptions.

H. Lanz (*Monist*, 1918, **28**, 46–67) maintains that the "actuality" which some mathematicians attribute to infinity belongs "rather to the methodological than to the quantitative character of infinite aggregates."

W. Sierpinski (*Compt. Rend.* 1916, **163**, 688–91) has a note on the part played in modern analysis by the axiom of Zermelo.

L. E. J. Brouwer (*Vers. Kon. Akad. van Wet.* Amsterdam, **25**, 1418–23) gives some addenda and corrections to his work of 1907 on the principles of mathematics. Brouwer (*ibid.* 1424–6) gives two theorems in the theory of linear aggregates.

C. Burstin (*Sitzungsber. der K. Akad. der Wiss. in Wien*, 1915, **124** [IIa], 1187–202) considers the problem of splitting up the number-continuum into an aggregate (which is of the cardinal number of Cantor's second number-class) of everywhere dense aggregates, under the assumption of the existence of a well-ordered aggregate of the cardinal number referred to. Burstin also (*ibid.* 1916, **125**, 209–17), supposing that the continuum can be well-ordered, shows that it can be spilt up into an aggregate of *c* aggregates which are not measurable in Lebesgue's sense. Here *c* is the cardinal number of the continuum.

M. Souslin (*Compt. Rend.* 1917, **164**, 88–91) has a note on a definition of measurable (Baire) aggregates without transfinite numbers (cf. also N. Lusin, *ibid.* 91–6).

G. Pál (*Math. és phys. lapok*, Budapest, 1915, **24**, 236–42) proves that every continuous plane curve, considered as a point-aggregate, is the projection of a spatial Jordan's curve.

D. König (*Math. és term. ért.* Budapest, 1916, **34**, 104–19) reproduces the article on graphs and their application to the theories of aggregates and determinants which appeared in *Math. Ann.* (1916, **77**, 455–65 ; *Rev. sem.* **25** [1], 278).

W. H. Young and (Mrs.) G. C. Young (*Compt. Rend.* 1916, **163**, 509–11) consider the "normal" frontier of a region or of a set of points.

G. Hamel, it may be remembered, decided (1905), on the supposition that the continuum can be well-ordered, that there are discontinuous solutions of the functional equation $f(x+y)=f(x)+f(y)$. In 1916 H. Blumberg showed that such solutions are non-measurable, and in 1917 obtained a generalised result (*Bull. Amer. Math. Soc.* 1918, **24**, 220).

An account of A. Palatini's paper (*Nuovo Cimento*, 1917) on Einstein's theory of relativity and the motion of the perihelion of Mercury's orbit is given in *Nature*, 1918, **100**, 492). Other recent noteworthy papers on the theory of relativity are by A. Einstein (*Sitzungsber. der K. Preuss. Akad. der Wiss. zu Berlin*, 1916, 1111–16, B. Cabrera (*Rev. de la R. Acad. de Cienc. ex. de Madrid*, **11** [8 articles], **12**, 546–70, 738–52), and F. Kottler (*Sitzungsber. der K. Akad. der Wiss. in Wien*, 1916, **125** [IIa], 899–919). Other information as to the principle of relativity is given under "Astronomy" in these "Recent Advances."

Arithmetic, Theory of Numbers, and Algebra.—É. Borel (*Compt. Rend.* 1916, **163**, 596–8) has a note on the approximation of incommensurable numbers by rational numbers, and O. Nicoletti (*Rend. del Circ. mat. di Palermo*, 1917, **42**, 73–9) translates some geometrical results into analytical language, and thus obtains a class of iterations operating on a complex variable, by which we can approximate to the roots of a quadratic equation.

G. Rados (*Math. és term. ért.* Budapest, 1915, **33**, 702–10) treats a question from the theory of congruences of higher degrees, gives (*ibid.* 758–62) a new derivation of the well-known criterion of the solvability of quadratic binominal congruences, and (*ibid.* 1916, **34**, 62–70) gives an analogue of Wilson's theorem. Rados also (*ibid.* 641–55) gives a new exposition of the theory of binominal congruences.

V. Amato (*Rend. del Circ. mat. di Palermo*, 1917, **42**, 48–60) gives a resolution, in a quadratic corpus, of binominal congruences in which the modulus is a prime ideal number of the second degree.

L. Grosschmid (*Math. és term. ért.* Budapest, 1916, **34**, 236–52) has a paper on the distribution of quadratic residues.

M. Bauer (*ibid.* 90–103) gives his researches on the theory of alegbraic corpora which appeared also in *Math. Ann.* (**77**, 353–61; *Rev. sem.* **25** [1], 26).

G. H. Hardy and S. Ramanujan (*Compt. Rend.* 1917, **164**, 35–8) give an asymptotic formula for the number of partitions of n.

H. H. Mitchell (*Amer. Journ. Math.* 1917, **39**, 425–9) obtains certain limits for the sum of all the positive integers less than a given prime m, the indices of which, with respect to a primitive root of m, have the same residue, mod $2n$, where $2n$ is a divisor of $m-1$. Perhaps the most interesting results concerning the sums of the type considered were established by Dirichlet in connection with his investigation of the class number of quadratic forms, and Mitchell's results are obtained by use of a certain type of Dirichlet's series.

S. Composto (*Giorn. di Mat.* 1916, **54**, 290–93) has a note on a determinant whose elements are functions of factorial powers ("factorial Wronskian determinants").

C. E. Cullis (*Bull. Calcutta Math. Soc.* 1916, **6**, 33–48; 1917, **8**, 1–32) gives two parts of some researches on primitive matrices and the primitive degrees of a matrix.

J. B. Pomey (*Nouv. Ann. de Math.* 1916, **16**, 501–5), byanalogy with the consideration (Cauchy, Kronecker) of imaginaries as the residues of whole functions with respect to the modulus $1+x^2$, calls by the name of "imaginary quantity" any whole function of the undetermined x, if we consider it as "equivalent" to the remainder after division by $(x-w_1)(x-w_2)(x-w_3)$.

G. A. Miller (*Amer. Journ. Math.* 1917, **39**, 404–6) proves that it is possible to construct a non-abelian group which has for its characteristic operators (those operators which correspond to themselves in every possible automorphism of the group) all the operators of an arbitrary abelian group. Miller (*Bull. Amer. Math. Soc.* 1918, **24**, 203–6) exhibits the equivalence of the theory of imprimitive substitution groups and the theory of multiplication of another type of special matrices.

Analysis.—O. Szász (*Math. és term. ért.* Budapest, 1915, **33**, 654–83) has a paper on the convergence of continued fractions of real elements.

H. A. Kempner (*Annals of Math.* 1917, **19**, 127–36) investigates a problem on "lattice-points" by very elementary methods; its complete solution is given by a pointwise dis-

continuous function of a well-known type. Systems of lattice-points are of importance in the theories of numbers, binary quadratic forms, algebraic numbers, elliptic functions, modular functions, and crystallography.

D. Pompeiu (*Compt. Rend.* 1916, **163**, 430–32) gives a necessary and sufficient condition that a given discontinuous function may be a derived function.

L. Galvani (*Rend. del Circ. mat. di Palermo*, 1916, **41**, 103–34) considers "convex" functions of one and of two real variables which are defined in any aggregate.

G. H. Hardy (*Mess. of Math.* 1917, **46**, 175–82) considers Stieltjes's "problem of moments." It has been suggested by Lebesgue (1909) that it should be possible to prove some at any rate of Stieltjes's results, and in particular the theorem concerning the uniqueness of the solution of the problem, by methods independent of the theory of continued fractions. Hardy gives two different proofs of the uniqueness which fulfil this condition.

T. H. Hildebrandt (*Bull. Amer. Math. Soc.* 1917–18, **24**, 113–44, 177–202) gives a paper which is very closely related to the recent paper by G. A. Bliss (see SCIENCE PROGRESS, 1918, **12**, 544). Many definitions of integration, which are more or less related to the Lebesgue definition, have recently been given, and Hildebrandt discusses some of these definitions and considers their relation to the Lebesgue integral. In the first section he discusses the types of definition of integration which are extensions of the Darboux upper and lower integral method of defining a Riemann integral: W. H. Young's definition is shown to be equivalent to the Lebesgue definition; and Pierpont's definition is also considered. The second section is devoted to a consideration of the definitions of the integrals of functions which are not integrable according to Lebesgue's definition, because functions integrable are always absolutely integrable (Jordan, Harnack, Moore, Borel, Denjoy). The third section is devoted to the Stieltjes integral, which has recently come into the foreground on account of the part which it plays in the theory of linear functional operations on continuous functions. Hildebrandt points out that a Stieltjes integral is expressible in terms of a Lebesgue integral of another function and conversely, but that in spite of this the Stieltjes integral seems to be applicable where the

Lebesgue integral is not. He gives an extension of the Stieltjes integral modelled on the Lebesgue extension of the Riemann integral, as well as the Fréchet generalisation of the Lebesgue and Stieltjes integral so as to apply to a class of general elements. The last section gives the definition of the Hellinger integral and also the generalisations of this due to Radon and E. H. Moore.

W. H. Young (*Proc. Roy. Soc.* A, 1916, **92**, 353–6) proves that, in a large class of cases, an oscillating succession of functions can be shown to contain converging sequences of functions.

L. Tonelli (*Ann. di Mat.* 1916, **25**, 275–316) extends results obtained by him in 1910 on polynomials approximating to functions of many real variables (*Rev. sem.* **18** [2], 95).

G. Pál (*Math. és phys. lapok*, Budapest, 1915, **24**, 243–7) gives certain generalisations of the theorem of Weierstrass on the representation of continuous functions by series of polynomials.

O. Szász (*ibid.* 1916, **25**, 157–77) seeks conditions that certain linear combinations of given functions may serve as bases for approximation to any continuous function.

L. Fejér (*Math. és term. ért.* Budapest, 1916, **34**, 209–29) occupies himself with the approximation to a function that we obtain by drawing through n points of its curve a parabola of the $(2n-1)$th degree in which the tangent at each one of the points spoken of is parallel to the axis of abscissæ.

In the short account in Science Progress (1918, **12**, 367) of a paper by R. L. Borger, it was mentioned that the fundamental theorems of Cauchy's theory of functions were deduced from a theorem proved by G. Kowalewski in his book *Die komplexen Veränderlichen und ihre Funktionen* (Leipzig and Berlin, 1911, p. 187). This theorem is, that if the real functions of two variables, u and v, are "properly" differentiable in a rectangle in the xy-plane, and also $\delta u/\delta y = \delta v/\delta y$, they are derivatives of a function w with respect to x and y. But this theorem is proved by Kowalewski by integration: in fact w is $\int(udx + vdy)$ taken along a path in the rectangle up to a point in the rectangle, and of course that means that we have to prove a theorem exactly analogous to Cauchy's and by the same method. Further, "proper" differentiability is a simple translation of the existence of the derivative of $f(z)$, where z is a complex variable and $f = u + iv$ (Kowalewski, p. 199).

According to the *Rev. sem.* (1917, **25** [2], 46) B. Hostinský (*Casopis pro pešt. math. a fys.* Prague, 1915, **44**, 28–30) finds that the function determined by Cauchy's theorem is the *geometrical* mean of its values at the points of the given contour.

W. Sierpinski (*Rend. del. Circ. mat. di Palermo,* 1916, **41**, 187–90) has a note on a potential series which is convergent at every point of its circle of convergence and represents on this circle a discontinuous function.

A. Signorini (*Ann. di Mat.* 1916, **25**, 253–74) writes on the conditions under which a function exists which is regular in a simply connected region S of the complex plane and of which the real and imaginary parts reduce, on certain arcs of the contour of S, to functions of the arc of this contour.

M. Kössler (*Rozp. České Akad.* Prague, 1915, **24**, No. 41), starting from the integral of Cauchy, studies developments in series of analytic functions which hold in a given domain.

J. L. W. V. Jensen (*Mém. de l'Acad. Roy. de Danemark,* Copenhagen, 1916, **2**, 200–28) investigates a class of fundamental inequalities for the absolute amounts of analytic functions under certain conditions. There is a historical sketch and an account of the new researches of the author.

H. Bohr (*Nyt Tidsskr. för Mat.* 1916, **27**, 73–8) proves an inequality concerning the manner of increase of the values of analytic functions.

M. Beke (*Math. és term. ért.* Budapest, 1916, **34**, 1–61) proves and applies the theorems of Hadamard and Hurwitz on certain series compounded out of two given power series.

É. Borel's important *Leçons sur les Fonctions Monogènes* is reviewed at length in the present number of SCIENCE PROGRESS; it bears a close relation to the two papers by Borel noticed in the last number of SCIENCE PROGRESS (1918, **12**, 544; cf. the review of the *Book of the Rice Institute* in the present number).

On Dirichlet's series, we may refer to P. Nalli (*Rend. del Circ. mat. di Palermo,* 1917, **42**, 61–72), and A. Arwin (*Mém. de l'Acad. Roy. de Danemark,* Copenhagen, 1916, **2**, 79–85), and, on the Zeta-function, to C. de la Vallée Poussin (*Compt. Rend.* 1916, **163**, 418–21, 471–3).

R. D. Carmichael (*Amer. Journ. Math.* 1917, **39**, 385–403) gives a sequel to his memoir of 1916 (SCIENCE PROGRESS, 1917, **11**, 456–7; cf. 1918, **12**, 370) in which he laid the foundations

of a general theory of series of two forms of which the first is $\Sigma c_n g(x+n)$. In this sequel he determines the asymptotic character of a function defined by a series of that first form.

G. Pólya (*Math. és term. ért.* Budapest, 1916, **34**, 754–8) proves a generalisation of the theorem of Eisenstein on the representation of an algebraic function by a power series with rational coefficients.

M. Riesz (*Arkiv för Mat.* 1916, **11**, No. 12) gives new proofs and a generalisation of two theorems of Fatou on the coefficients of power series.

S. Wigert (*ibid.* No. 5) proves theorems on the convergence of series associated with the coefficients of a power series. Wigert also (*ibid.* No. 21) gives a theorem on whole functions, and M. Ålander (*ibid.* No. 15) continues his work of 1914 on the zeros of the derivatives of whole real functions.

F. R. Berwald (*ibid.* No. 23) generalises certain identities due to Euler.

The main theorems on factorisation of analytic functions of several variables were given by Weierstrass (cf. his *Abhandlungen über Functionentheorie*, 1886, p. 107), but his presentation does not make clear what is essential, and so W. F. Osgood (*Annals of Math.* 1917, **19**, 77–95) gives a systematic presentation of the theory as a whole. Dunham Jackson (*ibid.* 142–51) points out some of the cases in which theorems about the behaviour of analytic functions of several complex variables in the neighbourhood of a point have analogous theorems corresponding to them for the case of a function which is analytic with respect to a particular one of its arguments, but merely continuous with regard to the whole set of variables; Bôcher (*ibid.* 1910–11, **12**, 18–26) called such functions "semi-analytic." Jackson also points out a simple case in which the above analogy is not preserved unimpaired. His method of treatment is closely related to that of an earlier paper of his (*ibid.* 1915–16, **17**, 172–9).

Paul du Bois-Reymond (1876) gave the first example of a continuous function whose Fourier's development diverges at one or more points; Haar (1909) showed how to construct a continuous function whose Sturm-Liouville development diverges and one whose development in Legendre's functions is divergent, and Gronwall (1914) gave a function whose development in Legendre's functions is not summable at a certain

point. C. N. Moore (*Bull. Amer. Math. Soc.* 1917, **24**, 145–9) gives a continuous function whose development in Bessel's functions of order zero is not summable at a certain point: this function is analogous to a function given by Fejér (1910), whose Fourier development diverges at a point, but the proof of the non-summability follows different lines from Fejér's proof of the divergence for his example.

W. Gross (*Sitzungsber. der K. Akad. der Wiss. in Wien*, 1915, **124** (IIa), 1017–37) applies the well-known process of "Poisson's summation" to Fourier's series, Cesàro's mean, and Riemann's summation.

W. H. Young (*Compt. Rend.* 1916, **163**, 427–30, 975–8; 1917, **164**, 82–5, 267–70) continues his work on the theory of the convergence of Fourier's series. On the subject of Fourier's series M. Fekete (*Math. és term. ért.* Budapest, 1916, **34**, 759–86), and I. Priwaloff (*Rend. del Circ. mat. di Palermo*, 1916, **41**, 202–6) also write.

D. F. Barrow (*Annals of Math.* 1917, **19**, 96–105) gives a simple method of compounding frequency functions when they can be represented by Fourier's series. It is applicable over a wide range of problems, yields formulæ which represent exactly the frequencies and from which numerical results can be obtained with relative ease, and is most useful where a few frequencies are to be compounded—that is, where the asymptotic solution is not very exact.

P. R. Rider (*Amer. Math. Monthly*, 1917, **24**, 420–22) gives a short intrinsic equation solution of a problem proposed and solved by Euler in his book of 1744, which is usually solved by the methods of the calculus of variations.

J. Radon (*Sitzungsber. der K. Akad. der Wiss. in Wien*, 1916, **125** (IIa], 221–43, 241–58) has papers on (1) the application of the calculus of variations to the general problem of the catenary, and (2) an extension of the concept of "convex functions" in this calculus.

Expositions and extensions of Volterra's theory of "functions of lines" are given by E. Pascal (*Rend. dell' Accad. di Napoli*, 1914, **20**, 40–48, 68–77, 85–91, 104–11) and P. Dienes (*Math. és term. ért.* Budapest, 1916, **34**, 154–94, 656–92), and H. B. A. Bockwinkel (*Versl. Kon. Akad. van Wet.* Amsterdam, **25**) has several articles on the "complete transmutations" important in the functional calculus.

H. F. Baker (*Phil. Trans.* A, 1916, **216**, 130–86) considers certain linear differential equations of astronomical interest.

N. Kuylenstierna (*Arkiv för Mat.* 1916, **11**, No. 10) continues his researches on the analytic solutions of two simultaneous linear finite-difference equations of the first order.

E. del Vecchio (*ibid.* No. 11) integrates two parabolic equations.

L. Schlesinger (*Jahresber. der D.M.V.* **24**, 84–123; *Math. és term. ért.* Budapest, 1916, **34**, 129–53, 316–36) writes on the theory of linear integro-differential equations.

W. L. Hart (*Amer. Journ. Math.* 1917, **39**, 407–24) discusses certain properties of infinite linear systems of ordinary differential equations in infinitely many variables, which are analogous to some connected with the notion of fundamental sets of solutions, of those of finite linear systems with a finite number of variables. It may be remarked that Ritt (1917; SCIENCE PROGRESS, 1917, **12**, 12) considered a problem of a different type; the results of E. H. Moore (1908) are not of the nature of those obtained in the present paper; the general systems of von Koch (1899), F. R. Moulton (1915), and Hart (1917; see SCIENCE PROGRESS, 1918, **12**, 547) do not include the system of the present paper; while the results of Hildebrandt (1917; SCIENCE PROGRESS, 1917, **12**, 12) generalise the theorems on finite systems in such a way that the notion of the determinant of the fundamental sets of solutions is retained, whereas in the present paper the matrices of the fundamental sets need not possess determinants.

J. A. Bullard (*Amer. Journ. Math.* 1917, **39**, 430–50) shows how certain properties of a group of linear homogeneous transformations can be obtained at once from the charateristic equation of the general infinitesimal transformation of the group, and thus how the type of structure is in part determined immediately from the infinitesimal transformations of such a group without the determination of the adjoint group.

On various points of potential theory, see A. Wangerin (*Nova Acta der K. Leop. Carol. Akad.* 1915, 1–80) and G. Prasad (*Bull. Calcutta Math. Soc.* 1916, **6**, 3–11; 1917, **8**, 33–40).

Geometry.—B. P. Haalmeijer (*Nieuw Archief voor Wiskunde*, 1917, **12**, 152–60) discusses "convex regions" in the theory of sets of points.

There is an excellent and detailed review by R. C. Archibald

(*Bull. Amer. Math. Soc.* 1918, **24**, 207–10) of F. Gomes Teixeira's *Problèmes célèbres de la Géométrie élémentaire* (see SCIENCE PROGRESS, 1917, **12**, 160).

H. L. Smith (*Annals of Math.* 1917, **19**, 137–41) proves in a simple manner, by using a theorem of Schoenflies, the fundamental lemma of H. Tietze in his proof (1914) that every one-to-one continuous representation of a square upon itself which preserves sense is a deformation; that is, it may be regarded as a member of a continuous one-parameter family of such representations which contains the identity.

Mary Gertrude Haseman (*Trans. Roy. Soc. Edinburgh*, 1918, **52**, 235–55), continuing the work of Listing (1874) and Tait (1876–86), studies those knots in particular which exhibit a special kind of symmetry—the "amphicheiral" knots—and gives a census of the amphicheirals with twelve crossings.

F. Irwin and H. N. Wright (*Annals of Math.* 1917, **19**, 152–8) study what they call "polynomial" curves, $y = F(x)$, where $F(x)$ is a polynomial in x alone, which do not seem to have been thoroughly treated up to the present time. Some applications of the theory are also given.

Algebraic curves form the subject of work by G. Rosati (*Ann. di Mat.* 1916, **25**, 1–32), L. Brusotti (*ibid.* 99–128), A. M. Harding (*Giorn. di Mat.* 1916, **54**, 185–222), C. Burali-Forti (*ibid.* 249–78), A. L. Nelson (*Rend. del Circ. mat. di Palermo*, 1916, **41**, 238–62), and J. de Vries (*Versl. Kon. Akad. van Wet.* Amsterdam, **25**, 954–60); and curves of the fourth order in space by L. Vietoris (*Sitzungsber. der K. Akad. der Wiss. in Wien*, 1916, **125** [IIa], 259–83).

J. M. Stetson (*Annals of Math.* 1917, **19**, 106–26) studies the relations between the radial transformation and some of the other simple transformations of general conjugate systems of curves on a surface, and these results are applied to the surfaces both of whose Laplace transforms are lines of curvature.

On solid geometry in general we may notice the late G. Darboux's *Principes de géométrie analytique* (Paris, 1917; *Rev. sem.* 1917, **25** [2], 58), and C. Cailler's three articles (*Arch. de Genève*, 1916, **42**). Geodesics on quadrics are discussed by A. Arwin (*Arkiv för Mat.* 1916, **11**, No. 9). On algebraic surfaces see K. W. Rutgers (*Nieuw Archief voor Wiskunde*, 1917, **12**, 109–34), C. H. van Os (*ibid.* 169–87, and *Versl. Kon. Akad.*

van Wet. Amsterdam, **25,** 963–71), J. de Vries (*ibid.* 1414–18) G. Grimaldi (*Rend. del Circ. mat. di Palermo,* 1917, **42,** 80–84), and G. Marletta (*ibid.* 1916, **41,** 180–86); on complexes of straight lines see C. Segre (*ibid.* 1917, **42,** 85–93); on algebraic hypersurfaces of *n* dimensions see E. Brambilla (*Ann. di Mat.* 1916, **25,** 317–42); on infinitesimal geometry of space see P. Tortorici (*ibid.* 205–28), C. H. Yeaton (*ibid.* **26,** 1–33), H. Weyl (*Zürich Vjs.* 1916, **61,** 40–72), G. Fubini (*Palermo Rend.* 1916, **41,** 135–62), Z. Geöcze (*Math. és term. ért.* Budapest, 1915, **33,** 730–48; 1916, **34,** 337–54; *Math. és phys. lapok,* Budapest, 1918, **25,** 61–81), L. Bianchi (*Ann. di Mat.* 1916, **25,** 129–204), A. V. Bäcklund (*K. Sv. Vet. Handl.* 1916, **55,** No. 2), and V. Strazzeri (*Palermo Rend.* 1917, **42,** 1–45); and on invariant transformations of curves on a surface, see G. Fubini (*Ann. di Mat.* 1916, **25,** 229–52).

ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

Stellar Parallaxes.—The first of this series of notes on recent progress in astronomy (SCIENCE PROGRESS, **10,** 120, 1915) was devoted entirely to an account of the modern developments in the determination of stellar parallaxes; a rapid expansion of our knowledge of accurately determined parallaxes, consequent upon the successful employment of photographic methods, was then foreshadowed. The developments since have been even more rapid than seemed possible at that time.

One of the principal requirements for accurate parallax work is a telescope with a long focal length, which gives a large scale on the photographic plate. Good definition is not so essential, as the exposures are short. European observatories can therefore co-operate in this work effectively, whereas for observations requiring good definition they are generally handicapped through being in unsuitable sites. Most of the parallax work at present, however, is being done in America. The Committee of the American Astronomical Society which was formed to co-ordinate the work and to prevent unnecessary duplication presented a report at the last meeting of the Society, which gives a good idea of the rapidity with which results are now being obtained.

At the Allegheny Observatory in one year, 2,500 plates

were secured, 2,100 were measured, and more than 200 parallaxes were determined. Results for 81 stars are in course of publication. At the Dearborn Observatory, 800 plates were obtained and 2,600 plates are awaiting measurement, which was delayed pending delivery of a new measuring machine. Results for 17 stars have already been received. At the Leander McCormick Observatory, 4,200 plates have been taken. Preliminary results for 83 stars have been published (*Pop. Astr.* **25**, 23, 1917) and a complete account of the work is being prepared. At Mount Wilson the 60-inch reflector is being used for parallax work: this is the only observatory not using a refractor. The telescope has the long focal length of 80 feet, and this has enabled results of a high accuracy to be obtained, the probable errors averaging only about $0''\cdot006$. The programme includes many stars of small proper motion or of advanced types and a number of planetary and other nebulæ. Two series of results have been published, of 20 stars in *Mt. Wilson Contr.* No. 3, and of 30 in *Mt. Wilson Contr.* No. 136. The programme at the Sproul Observatory comprises mostly visual and spectroscopic binaries. A first list of 50 parallaxes was published recently in *Sproul Observatory Publications*, No. 4, and a number of other results will shortly be available. At the Yerkes Observatory, the 40-inch refractor is being used regularly for parallax determinations, and the results obtained to date are given in the *Publications of the Yerkes Observatory*, vol. iv. Pt. i. Some of the results had previously been published elsewhere, but all are collected for convenience of reference in this volume, which contains results for 131 stars. At Greenwich, the parallax observations have had to be temporarily suspended, but after the war probably at least 40 results yearly will be obtained. The programmes at the various observatories overlap by design to a certain extent. This is necessary in order that the results may be intercompared and a check obtained on the existence of systematic errors, which are more to be feared than accidental errors. The scheme of co-operation eliminates, however, unnecessary overlapping.

Reference must also be made to the spectroscopic method of determining parallaxes developed by Adams. The principle of this method has been explained in these notes (SCIENCE PROGRESS, **11**, 97, 1916). The first-fruits of it are now being

gathered. A first list of parallaxes of 500 stars has been published in the *Astroph. Journ.* **46**, 313, 1917. The stars chosen were for the most part those whose parallaxes had been determined directly in order to provide a check on the results. The agreement in general is surprisingly good and it is evident that the new method—which at present is limited in application to certain types of stars—is capable of turning out results by the hundred, of a high degree of accuracy and invaluable for statistical researches.

We are now within sight of the time when accurate parallaxes of several thousand stars will be available. In 1915 the number of reliable determinations was only about 200, and in 1900 only about 60.

Einstein's Theory of Gravitation.—The astronomical consequences of the new generalised relativity theory of gravitation advanced by Einstein were referred to recently in these notes (SCIENCE PROGRESS, **11**, 623, 1917). It was mentioned that the theory required the spectral lines of the sun to be displaced to the red as compared with the positions of the corresponding lines obtained from a terrestrial source. The displacement is equivalent to the Döppler displacement produced by a radial velocity of 0·63 km. per sec. The existence of this displacement has been carefully investigated by St. John at the Mount Wilson Observatory (*Astroph. Journ.* **46**, 249, 1917): the amount of the displacement in Ångstrom units is only 0·008 Å, but so accurate are the Mount Wilson measurements that the existence of systematic displacements of this amount can be detected. St. John attacked the problem by choosing lines in the band spectrum of cyanogen which show no pressure shift; displacements in the line of sight were eliminated by observations of the same lines at the Sun's polar limb. Wave-lengths of these lines were measured at the centre and at the limb and also in the carbon arc in terms of identical iron standards; the solar wave-lengths were redetermined in the Rowland system. The displacements sun *minus* arc were determined at the centre directly and by three indirect methods. The mean displacement was not in any case greater than 0·002 Å. The results of the investigation were that, although there existed a limb effect not due to motion, in which pressure, level, and line intensity appeared to be involved in varying degrees, there was no evidence of a

displaçement of the order required by the relativity principle. One feels reluctant to abandon Einstein's theory after its success in explaining the discordance in the motion of the perihelion of Mercury and yet it does not seem possible to doubt the accuracy of the Mount Wilson measures. It is greatly to be hoped that their result will be checked by Mr. Evershed at the Kodaikanal Observatory. Meanwhile, it is to be regretted that the present indications are that the splendid opportunity provided by next year's total solar eclipse of testing the third prediction of the theory will not be utilised. If St. John's result is confirmed, Einstein's theory will probably be one more to add to the long list of exploded gravitational theories.

W. de Sitter in the *M.N., R.A.S.*, **78**, 3, 1917, has a third paper dealing with the astronomical consequences of Einstein's theory. In that theory, as originally advanced, there was a difficulty in connection with the boundary conditions at infinity. To overcome this the notion of a " curved " space was introduced ; in this paper the consequences of that conception are worked out. The ideas are very metaphysical and one cannot but feel in reading the paper that physical conceptions have been rather lost sight of, and that there is an artificiality in the new extension. The matter has been more simply and more generally treated by L. Silberstein in a paper on " Planetary motion in space-time of any constant curvature according to the generalised principle of relativity," which was read before the Royal Astronomical Society at its March meeting. (This paper has not appeared in print at the time of writing these notes, but a summary appeared in *The Observatory*, **41**, 162, 1918.) In this paper, instead of treating of a space-time with a preconceived curvature, the sign and amount of the curvature is left entirely free and the equations of planetary motion are developed in a purely relativistic manner. It is shown that there are no reasons *a priori* for discarding a Euclidean or hyperbolic world in favour of a spherical or an elliptic one, and that the "world-curvature," if such exists, is determinate. A method by which the order of magnitude of this curvature can be assigned when the excess of Mercury's perihelion motion is accurately known is indicated.

A useful account in plain language of the nature of the principle of relativity and of the meaning of gravitation on

that theory was given by Prof. A. S. Eddington at the week evening meeting of the Royal Institution on February 1st, 1918. This lecture was reprinted in *Nature*.

The Origin of the Moon.—Sir George Darwin showed, on his theory of tidal friction, that the earth and the moon were once close together, revolving so that each kept the same face towards the other. He supposed that the two had originally formed one body, although the cause of the separation was not traced, and that one of the free periods of vibration of a liquid mass of the dimensions of the earth would not differ greatly from half the initial period of rotation required by tidal theory; the semi-diurnal solar tide would then be magnified by resonance and might produce a rapidly increasing deformation which might easily lead to a rupture of the mass into two parts.

Bryan showed that this resonance theory of the origin of the moon was not tenable if the earth was assumed homogeneous. H. Jeffreys has investigated the matter more generally in a paper entitled "The Resonance Theory of the Origin of the Moon," *M.N., R.A.S.*, **78**, 116, 1917. He shows that homogeneity gives the most favourable case for instability and the least favourable case for resonance, and that the period of rotation necessary to give resonance for the semi-diurnal tide raised by a fixed body can be made as long as one wishes by suitable choice of the density distribution. He then examines what takes place when resonance has been established. The general problem is too difficult to attack, but special cases are considered. It is shown that a deformation in the earth comparable with its size would be formed, and that if the deformation became great enough, a satellite would be formed. As to whether the deformation could be great enough cannot at present be answered. The theory therefore remains open and Bryan's result cannot be regarded as disproving it.

The following is a selection from amongst the more important papers recently published:

Theory of Errors.—LIAPIN, N., On a Fundamental Property of Accidental Errors, *Observatory*, **41**, 172, 1918.

PLUMMER, H. C., On the Errors in a Series of Tabular Quantities, *M.N., R.A.S.*, **78**, 147, 1917.

Gravitational Astronomy.—SMART, W. M., Libration of the Trojan Planets, Part I., *Mem., R.A.S.*, **62**, pt. iii. 1918.

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Lagrange discussed the stability of three bodies whose relative positions are the vertices of an equilateral triangle and showed that the motion was stable when the orbits were coplanar. The problem remained of theoretical interest until the discovery of certain asteroids which with the sun and Jupiter formed an approximately equilateral triangle. Such asteroids are known as the Trojan planets. In this paper the theory of their librations is worked out.

Petter, G. B., The Determination of a Cometary Orbit, *Mem., B.A.A.*, **21**, pt. ii. 1918. This paper gives a demonstration of Leuschner's short method of determining orbits from three observations, with examples, suited to the reader with but slight mathematical knowledge.

Glancy, A. Estelle, On the Orbit of (132) Æthra, *Ast. Journ.* **31**, No. 3, 1918.

Neubauer, F. T., Orbit and Perturbations of (716) Berkeley, *Lick Obs. Bull.* 301, 1917.

Mundt, C. S., On the Orbit of (718) Erida, *Lick Obs. Bull.* 302, 1917.

Pickering, E. C., Asteroids bright in 1918, *Harv. Coll. Obs. Circular*, No. 203, 1918. The Harvard Observatory publishes every year a useful abstract of ephemeroids of asteroids attaining a magnitude of 10 m. and brighter during the year.

Fischer-Petersen, J., Die auf der Kopenhagener Sternwarte ausgeführten numerischen Untersuchungen über das Dreikörperproblem (Problème restreint), *Pub. o. g. Medd. fra Kobenhavns Obs.* No. 27. This is a useful and fairly elementary account of the valuable work done at Copenhagen in investigating periodic and retrograde orbits in the special case of the problem of three bodies in which one has a very small mass in comparison with the other two.

Variable and Binary Stars.—Turner, H. H., On the Classification of Long-period Variable Stars, *M.N., R.A.S.*, **78**, 92, 1917.

Phillips, Rev. T. E. R., Note on the Light Curves of Short-period Variables, *M.N., R.A.S.*, **78**, 185, 1918.

Wilson, R. E. (*a*) The Orbit of the Spectroscopic Binary γ Phœnicis, (*b*) The Orbit of the Spectroscopic Binary σ Puppis, *Lick Obs. Bull.*, No. 303, 1917. γ Phœnicis is of special interest as being the only late type star at present known to be moving in an essentially circular orbit.

HENROTEAU, F., A Spectrographic Study of κ Pegasi, *Lick Obs. Bull.*, No. 304, 1918. κ Pegasi is a visual binary of short period (11–37 years), and magnitudes $4^{m}\cdot8$, $5^{m}\cdot3$. One of the components is a spectroscopic binary. The elements of both systems are deduced.

COMSTOCK, G. C., The Orbit of $\Sigma 2026 = B 7561$, *Ast. Journ.* **31**, No. 5, 1917.

CHANT, C. A., The Variable Star W Virginis, *Journ. R.A.S.C.*, **12**, 47, 1918.

SHAPLEY, H., and V. DER BILT, J., Notes on the Colour-curve and Light Elements of W Ursæ Majoris, *Astroph. Journ.* **46**, 281, 1917.

MARTIN, C., and PLUMMER, H. C., The short-period Variable RZ Cephei, *M.N., R.A.S.*, **78**, 156, 1917.

PICKERING, E. C., 21 new Variable Stars, *Harv. Coll. Circ.* No. 201; Maxima in 1918 of Variable Stars of Long Period, *ditto*, No. 202. With long-period variables the light changes are not regular. This yearly list contains maxima computed from recent maxima.

Stellar Distribution, etc.—JEANS, J. H., The Equations of Radiative Transfer of Energy, *M.N., R.A.S.*, **78**, 28, 1917; The Evolution and Radiation of Gaseous Stars, *M.N., R.A.S.*, **78**, 36, 1917.

EDDINGTON, A. S., The Radiative Equilibrium of the Stars. Reply to Mr. Jeans' Criticisms, *M.N., R.A.S.*, **78**, 113, 1918.

STRÖMBERG, G., A determination of the Solar Motion and the Stream Motion from Radial Velocities and Absolute Magnitudes of Stars of late Spectral Types, *Astroph. Journ.* **48**, 7, 1918.

PERRINE, C. D., Relation to Proper Motion of Preferential Motion and of the progression of Spectral Class and Magnitude Velocity, *Astroph. Journ.* **46**, 266, 1917.

HALM, J., On the Relation between Star Counts and Apparent Magnitude, *M.N., R.A.S.*, **78**, 312, 1918.

CROMMELIN, A. C. D., Are the Spiral Nebulæ External Galaxies? *Scientia*, May 1917, and *Journ., R.A.S.C.*, **12**, 33, 1918.

EDDINGTON, A. S., Researches on Globular Clusters, *Observatory*, **40**, 394, 1917; The Dynamical Problems of the Stellar System, *Observatory*, **41**, 132, 1918.

PHYSICS. By James Rice, M.A., University, Liverpool.

The *Phys. Rev.* of December 1917 contains a paper by A. W. Hull on a new method of X-ray crystal analysis. A brief description had been published in the *Phys. Rev.* for January 1917. The methods already developed by Prof. Laue, and by Prof. Bragg in co-operation with his son are applicable only to individual crystals of appreciable size reasonably free from twinning and distortion and sufficiently developed to allow the determination of the direction of their axes. Prof. Hull's method permits investigation in cases where such crystals cannot be found in nature or in ordinary technical products and can only be grown with difficulty and much expenditure of time. The amount of crystal material required is no more than 0·005 c.c., and admixture with foreign material, provided it is amorphous or of known crystalline structure, is not fatal to the success of the method. It consists in sending a narrow beam of monochromatic X-rays through a disordered mass of small crystals reduced to a finely divided form and packed in a thin-walled tube of glass which, where great precision is required, can be maintained in continuous rotation. The X-rays diffracted by the various crystal planes fall on a photographic plate, producing a definite diffraction pattern. Owing to the disordered arrangement of the crystals, assisted by frequent stirring (and rotation where necessary), the average orientation of the little crystals during a long exposure is a random one. A certain little group of the crystals will at any given instant have their 100 planes (say) at the proper angle with the X-ray stream to reflect the particular wave-length used; and similarly there will be small groups with other planes suitably oriented. In fact for a long exposure there will be on the average an equal chance for each plane of the crystal making its presence felt in the diffraction pattern. This statement is, of course, subject to the limitation that crystal planes whose distance apart is less than half the wave-length used, can only produce a diffracted wave with very small amplitude. The scattered energy is divided among a finite number of planes, each of which produces on the plate a linear image of the source. From the positions of these images the crystal structure can be deduced by a method similar to that employed by the Braggs, with this difference. In the Bragg method reflections from three

or four known planes are observed and a structure is sought which gives the spacings and intensities observed for these planes. In the Hull method a single photograph is taken containing reflections from a large number of unknown planes, and a structure is sought whose whole pattern of planes, arranged in the order of decreasing spacing and omitting none, fits the observed pattern. In both cases the method is one of trial and error, namely to try one arrangement after another, beginning with the simplest, until one is found which fits. The paper gives photographs and details for working out the structure in the case of aluminium, iron, silicon, magnesium, sodium, lithium, nickel, graphite, diamond—the results in the case of the last-named substance forming an excellent check for the method, since the structure of the diamond has already been completely determined by the Braggs, and is given in their book on *X-Rays and Crystal Structure.* When single crystals of sufficient perfection are available, the Bragg method is, of course, the simplest. When, however, perfect order of crystalline arrangement cannot be obtained, the next simplest condition is random grouping of small crystals, with equipartition of reflecting opportunity among all the crystal planes. There are two disadvantages to this; the opportunity for any one plane to reflect is so small as to necessitate long exposures of several hours, and the linear images due to all the planes appear on the same plate, so that it is impossible without calculation to tell which image belongs to which plane. However, it permits of definite numerical calculation of the position and intensity of each line, and is free from uncertainties due to imperfection and twinning of crystals. These photographs are preliminary ones, taken with rather crude experimental arrangements, but the author hopes that with improved apparatus and more accurate photographic measurements some evidence may be forthcoming not merely as regards the structure of the atoms in a crystal, but also of the structure of the nucleus and electrons within an atom, at all events in the case of the light substances. Prof. Hull states that there is evidence that some electrons are really free in the sense that they belong to no atom, but occupy definite spaces in the lattice, as though they were atoms.

To the same number of the *Phys. Rev.*, Blake and Duane contribute two papers. In one they determine the value of

Planck's "h" constant by means of X-rays using the quantum relation

$$Ve = hn$$

where e is the elementary charge and n the frequency of X-rays which are produced by the voltage V. The value agrees closely with those obtained by other experimenters using the same method, and also those obtained by Millikan and others using the photoelectric effect. The feature of the method is the design of a new type of X-ray spectrometer, resembling very closely the ordinary optical spectrometer in several of its details. The second paper by these authors deals with the more accurate determination of the frequencies at which marked increases in the absorption of X-rays by a chemical element takes place. Uhler and Cooksey give an account in the same number of this journal of experiments to determine with considerable accuracy the wave-lengths for the K series of the X-ray spectrum of gallium. They also use a spectrometer different from the usual type; for as their work proceeded, the difficulties and sources of error inherent in the usual method became so prominent as to cause the first named of the authors to make an analytical study of the general problem of determining glancing angles. The results of this study are presented in the January 1918 number of the *Phys. Rev.*, and by them it became possible to subject the old method and the new one to very thorough practical tests. Like Messrs. Blake and Duane, these collaborators designed a spectrometer resembling in some details the optical spectrometer.

In the April number of the *Proc. Roy. Soc.*, Prof. Richardson discusses the photoelectric action of X-rays from the point of view of the quantum theory, basing his remarks on the equation

$$\tfrac{1}{2}mv^2 = hn - w$$

where the left-hand side represents the maximum kinetic energy of the liberated electrons, h is Planck's constant, n the frequency of the exciting radiation, and w a constant which measures the work necessary for an electron to escape from a substance, its value being characteristic of the material under consideration. The paper is by no means easy reading; but the author advances the view that a survey of the experi-

mental data available (to which in collaboration with pupils he has made considerable contributions himself) justifies the assumption that the above equation is one of great generality, embracing "the whole gamut of the radiation scale in its interaction with every type of matter." It is true that in the region of very short wave-lengths (the X-ray region), the *maximum* energy of electrons liberated by X-rays of frequency n is very closely represented by

$$\tfrac{1}{2}mv^2 = hn.$$

This is consistent with the first equation, since the electrons with maximum velocity will come from the more superficial parts of the atom, and for such electrons w is negligible compared with hn in the case of X-rays, as n is so great for such radiation. Prof. Richardson, however, maintains that when we consider all liberated electrons, and not merely those with limiting velocities, the first equation will be found to represent the facts. He goes on in this paper to criticise Prof. Barkla's statement in his Bakerian Lecture (SCIENCE PROGRESS, April 1917), that "there is no evidence of absorption of X-radiation in whole quanta, though the conditions are frequently such as give an approximation to this." It is impossible to summarise the arguments with sufficient brevity, and they are somewhat obscure in places; but there is no doubt (as Prof. Barkla himself said in his Lecture) that a closer examination of X-ray phenomena is going to yield the most crucial tests of the generality of the quantum hypothesis, and we may expect in the near future considerable discussion of these matters.

Resonance radiation forms the subject of one or two papers contributed by Prof. R. W. Wood to recent numbers of the journals. The discovery of the existence of this type of radiation is due to Prof. Wood himself, his earliest investigations being published in 1906, and was made in connection with experiments on the fluorescence of sodium vapour. The absorption spectrum of sodium vapour in a heated condition is very complex, consisting of the principal series of lines (which contains the well-known D lines), and a whole group of fine and sharp absorption lines filling the entire spectrum except a narrow region in the yellow. If the beam of illuminating white light is very powerful, the vapour fluoresces, giving

a fluorescent spectrum as complex as the absorption one, and probably its complement. If, however, instead of white light, a strong beam of monochromatic radiation is used to illuminate the vapour, we find that it emits a series of bright lines spaced at nearly equal intervals along a normal spectrum. Various series of lines with varying distribution of intensity can be brought out by changing the wave-length of the exciting light. In every case light of the *same* wave-length as that of the exciting light is emitted by the vapour, and in addition a large number of other frequencies, which bear a definite relation to each other. On account of the equality of wave-lengths mentioned, Wood calls such a fluorescent spectrum a "resonance spectrum." Sodium vapour, for instance, has been excited by the monochromatic radiations from the cadmium-vapour lamp, the bismuth arc, the magnesium arc, the lithium arc, the mercury-vapour lamp, and incandescent sodium itself. Mercury vapour has also been shown to emit a resonance radiation when excited by the 2,536 line of the mercury arc. Iodine vapour was also found to behave in a manner similar to sodium vapour when stimulated with monochromatic light for various metallic arcs. One interesting point about these spectra is the approximately equal spacing of the lines of any one spectrum along the scale of wave-lengths, for this is bound to shed some light on the structure of the atom. As Prof. Wood remarks in his *Physical Optics*—" Prof. Rowland once said that 'a molecule is much more complicated than a piano.' In most cases all that we have been able to do is to strike the entire keyboard at once, but in the case of (the resonance spectra of) sodium it seems possible to strike one key at a time. In the March number of the *Phil. Mag.*, Wood gives new data concerning the resonance spectrum of iodine vapour, obtained since his earliest trials (in 1910) with that substance were made. Using the Cooper-Hewitt mercury-vapour lamp, he finds that the iodine vapour emits a spectrum consisting of a series of doublets (with a separation of 1·5 Angström Units), very regularly spaced along the spectrum and separated by intervals of about 70 Å.U. As we pass, however, from the green mercury line where the doublet series has its origin to the termination of the series at 7,685 in the red, the interval separating the last two doublets is 102 Å.U., and it appears that the fre-

quency intervals between the doublets increases by a constant amount as we pass from each one to the one of next higher order. A paper in the January number of the *Phys. Rev.* discusses the resonance radiation of sodium vapour when excited by the light from one of the D lines of incandescent sodium vapour. If for instance, D_2 light is used, under certain conditions D_1 light (of weak intensity) as well as D_2 light appears in the resonance radiation, and from a study of the conditions in this case (as well as of the changes introduced by admitting a little hydrogen into the resonating vapour) Wood concludes that the transmission of energy from the D_2 emission centres of the sodium atom (excited by the D_2 light) to the D_1 emission centres is in some way the result of molecular collisions either of sodium with sodium (when it is dense enough or hot enough) or of sodium with hydrogen.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Colloids and Chemical Industry.—The events of the past three and a half years have given to the whole subject of chemistry a prominence which under normal conditions it would probably not have received in this country for fifty years. At long last this country has awakened to a partial appreciation of the fundamental rôle which chemical operations play in the economic life of a nation. This awakening has made itself felt throughout the entire range of chemistry pure and applied, with the result that matters formerly regarded as relatively unimportant (even in the opinion of trained chemists themselves) have now taken on a new interest and significance.

One could scarcely find a more striking illustration than that afforded by the subject of colloid chemistry, itself a branch of physical chemistry. Although the pioneer work in this field was first carried out many years ago by Graham, the subject as a whole has remained the Cinderella of physico-chemical science, attracting indeed a limited amount of attention in certain specialised directions, but remaining practically a *terra incognita* so far as the majority of chemists, academic as well as technical, were concerned. The vast possibilities which underlie this branch of chemistry are as yet by no means fully grasped. It has been recognised, however, that

the principles of colloid chemistry are applicable to a far wider range of phenomena, and therefore to a greater variety of technical operations, than was hitherto suspected. The serious attention which this subject is now beginning to attract is reflected in the numerous papers and other publications during the last few years.

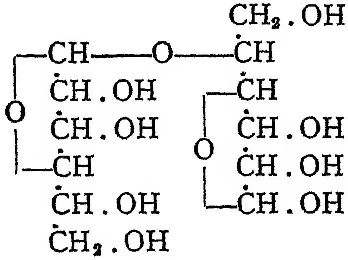
In this connection the British Association has taken an active part in forming a Committee with the object of compiling information regarding the advances which have been made in colloid chemistry with special reference to industrial processes. The First Report of this Committee has recently been issued. In spite of the difficulties which exist at the present time in connection with an effort of this kind, a promising beginning has been made. The technical subjects dealt with in this Report are: Tanning, Dyeing, Fermentation Industries, Rubber, Starch, Gum, Albumin, Gelatin, and Gluten, Cements, Nitro-cellulose explosives, and Celluloid. There is likewise an account of the viscosity of colloidal systems, and the application of colloids to biochemical subjects. The future Reports which it is proposed to issue will refer, like the first, to the more important scientific investigations published in recent years, as well as those possessing a more immediate technical bearing. Each subject is dealt with by an expert in the particular branch. In this way a valuable compilation should be built up which should serve as a source of information to all who are engaged in any branch of colloid chemistry pure or applied.

As an indication of the importance of this branch of chemistry the subjects mentioned above are instructive. This list is, of course, far from complete. Other technical operations and processes are represented in the treatment of hides, leather, and fur, mercerisation and finishing of fabrics, the manufacture of soap, dairy practice including the manufacture of butter substitutes, enzyme action, bread manufacture, treatment of clays for ceramic and other work, the flotation process in the treatment of ores, agricultural operations, soils, fertilisers, sprays, treatment of sewage, water purification, prevention of smoke and fumes, photographic materials and technique, paints, pigments, and varnishes, cellulose, wood-pulp and paper manufacture, manufacture of coloured glass, enamel, ultramarine, etc. Colloid chemistry has even found

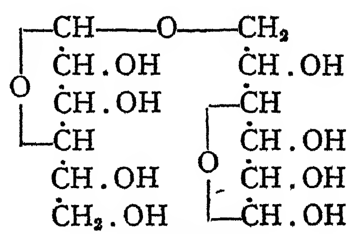
its way into the manufacture of metallic alloys, including steels. Such a list covers almost the entire range of chemical industry. Some of these operations are almost entirely colloidal in nature; in others the colloid state appears in at least one stage. As a matter of fact industrial practice has in many cases outrun the theory of the process. That in itself is the best argument for a vigorous prosecution of research in this subject.

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

The determination of the constitution of lactose forms the subject of a paper by Haworth and Leitch (*J. Chem. Soc.* 1918, 188). Any constitutional formula for lactose must account for the fact that it is a reducing sugar and gives on hydrolysis one molecule of glucose and one of galactose; these two hexoses must therefore be united together in some such way as to leave an active terminal group capable of exerting reducing action. Evidence is now furnished to show that the reducing property of lactose is attributable to the glucose moiety, the reducing property of the galactose having become latent by its combination with the glucose molecule. It has already been shown by Ruff and Ollendorff (*Berichte*, 1900, **33**, 1802) that the anhydride linking of glucose with galactose does not concern the hydroxyl groups attached to the second and third carbon atoms from the reducing end of the glucose chain; the fourth hydroxyl group is likewise not involved because this is occupied in the γ-oxide linking of the glucose complex. The galactose reducing group in its union with the glucose molecule must therefore have involved either the fifth or sixth hydroxyl group of the latter, as shown by the following formulæ:



I. (Lactose)



II. (Melibiose)

One of these two formulæ will represent the constitution of

lactose or its stereoisomer *iso*-lactose, while the other represents that of melibiose, the disaccharide obtained by the partial hydrolysis of raffinose. The first formula is the one assigned to lactose for the following reasons. When lactose is methylated by means of methyl sulphate and sodium hydroxide, eight methyl groups are introduced and heptamethyl methyl lactoside results; on hydrolysis this substance yields tetramethyl galactose and a trimethyl glucose whose constitution proves to be represented by the formula:

$$\begin{array}{ll} & CH_2.OCH_3 \\ & \dot{C}H.OH \\ \ulcorner & \dot{C}H \\ | & \dot{C}H.OCH_3 \\ O & \dot{C}H.OCH_3 \\ \llcorner & \dot{C}H.OH \end{array}$$

Theory permits of the existence of three other possible trimethyl glucoses, but all these are excluded for various reasons and the constitution of lactose is therefore definitely established. It will be seen from formula 1 that lactose contains the group:

$$>CH.O.CH<\begin{matrix} CH_2.OH \\ CH— \end{matrix}$$

which is a hitherto unknown mode of linking of two sugar molecules in the form of a branched chain; this being so, there need no longer be any reluctance in assuming that the reducing group of one hexose may be united with a second residue through any one of the hydroxyl groups of the latter. The existence of a number of isomeric disaccharides is thereby readily explained, as, for example, the four compounds maltose, *iso*-maltose, gentiobiose, and cellobiose, all of which contain two glucose residues.

The constitution of another vegetable alkaloid has recently been established by Hess and Eichel (*Berichte*, 1917, **50**, 1192). The rind of the root of the pomegranate tree contains five alkaloids, only one of which, ψ-pelletierine, has hitherto been investigated; the main constituent, however, pelletierine, has now been proved to be the aldehyde of coniine of the formula:

$$CH_2<\begin{matrix} CH_2—CH_2 \\ CH_2—NH \end{matrix}>CH.CH_2.CH_2.CHO$$

It yields a hydrazone which by heating with sodium ethoxide in a sealed tube at 156–170° is converted into racemic coniine. Methyl *iso*-pelletierine, on the other hand, has the formula :

$$CH_2\left\langle\begin{matrix}CH_2-CH_2\\CH_2-NH\end{matrix}\right\rangle CH.COCH_2.CH_3$$

and is therefore a methyl derivative of an unknown *iso*-pelletierine.

According to McCollum and Davis (*J. Biol. Chem.* 1915, **23**, 181–252), two accessory factors—a fat soluble A and water soluble B—are indispensable in the diet if growth is to occur. Later McCollum and Kennedy (*J. Biol. Chem.* 1916, **24**, 491) showed that polyneuritis set up in birds fed exclusively on polished rice or purified food stuffs was due to a deficiency of the water soluble B. This substance is also the vitamine which promotes the growth of young animals. Osborne and Mendel (*J. Biol. Chem.* 1917, **31**, 149) have now shown that this same water soluble vitamine is also contained in yeast, since the addition of an aqueous extract of this substance to a diet of purified caseinogen, starch, lard, butter, fat, and artificial protein free milk greatly increases its power to produce growth in rats. Further, rats growing on a diet containing yeast and caseinogen cease to thrive immediately the yeast is withdrawn. More recently McCollum and Pitz (*J. Biol. Chem.* 1918, **31**, 229) have found that guinea-pigs, fed on a diet which was sufficient to maintain rats in good condition, acquired scurvy. The addition to the diet of substances such as sodium benzoate or citric acid which depress the growth of micro-organisms in the intestine or of laxatives prevented the development of scurvy. Further, animals in which the scorbutic condition had been allowed to become acute were relieved by the addition of these substances to their food. The scurvy in these cases is not attributed to the absence of an anti-scorbutic vitamine, but to a retention of fæces in the cæcum owing to the unfavourable physical character of the diet, and the consequent harmful effect of the poisonous putrefaction products of bacterial origin.

In a paper dealing with the Power of Perfumes, Backman (*J. Physiol. Path. gén.* 1917, **17**, i.) puts forward the view that a substance in order to be perceptible to the smell must be soluble both in water and in lipoids, since the receptor organs are covered with a watery fluid while the cells themselves

contain lipoids. The lower members of the homologous series of alcohols are comparatively odourless since they are sparingly soluble in fats, whereas the higher alcohols, such as cetyl alcohol, being only sparingly soluble in water, likewise only smell faintly. Butyl and amyl alcohols, on the other hand, being soluble both in water and fats, smell strongly, and the same applies to aromatic hydrocarbons, nitro-compounds, amines, bromoanilines, naphthylamines, etc. The alteration of solubility in water and fats also explains the reason why acetylation increases the smell of ethyl alcohol and destroys that of aniline. The Relation between Odour and Constitution is discussed by Prins (*J. Soc. Chem. Ind.* 1917, **36**, 942). According to Magewski the odour of a substance was due to a so-called osmophoric group, but according to the present author all groups should be regarded as osmophoric, but in different degrees. The similarity in odour of all benzene derivatives is due to the predominant osmophoric properties of the benezene nucleus. Aliphatic hydrocarbons have a much weaker osmophoric influence, hence the great difference in smell between octylalcohol, octoic aldehyde, and octoic acid. Homologous substances such as octaldehyde, nonaldehyde, and decaldehyde and their corresponding alcohols have similar odours. Double bonds in the neighbourhood of an alcohol, aldehyde or carboxyl group have a marked influence on odour, as for example decaldehyde and undecenaldehyde, citronellal and citral, α-ionone and β-ionone. The influence of formic acid predominates over that of the alcohol in formic esters, and is least in the formates of strongly osmophoric alcohols such as borneol and menthol.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Stratigraphical and Regional Geology.—From the Summary of Progress (*Geol. Survey of Great Britain, Summary of Progress for* 1916, 1917, pp. 56) we learn that the work of the Survey is now restricted almost entirely to the examination of economic materials, especially refractories, coal, and iron ore, in connection with war requirements.

The Highland Border rocks of the Aberfoyle district (Perthshire), in which Prof. T. J. Jehu recently made an important discovery of fossils, are fully described in a paper by Prof. Jehu and Dr. R. Campbell (*Trans. Roy. Soc. Edinburgh*, 1917,

52, pt. 2, 175–212). The rocks are divided into two series: a lower, consisting of cherts and black shales with spilitic lavas, associated with intrusives and a group of highly metamorphic rocks of both igneous and sedimentary origin; and an upper series made up of grits, shales, and limestones, with a basal breccia. The included fossils determine the lower series as of Upper Cambrian age or as belonging to the passage beds between the Cambrian and Ordovician. The upper series is placed with confidence on a higher horizon in the Ordovician. The whole group is greatly affected by lines of crushing and dislocation, which render its relation to the Dalradian Leny Grits to the north very uncertain.

We are pleased to note that some parts of the British contribution to the *Handbuch der Regionalen Geologie* have reached this country. Dr. J. W. Evans has excellently summarised the stratigraphy of the Devonian (Devonian (Sedimentary) Rocks of Great Britain, *Handbuch der Regionalen Geologie*, Bd. 3, 1 Abth. The British Isles, 1916, 104–37). In accordance with his early views he believes that most of the Old Red Sandstone is of fluviatile origin. For the genesis of the red marls he cites as parallels the conditions obtaining at present in portions of the Bolivian plateau, and in the interior of South Australia. The Caithness Flags are believed to have been deposited in a region comparable to that of Lake Chad at the present day.

Sibly, T. F., The Geological Structure of the Forest of Dean, *Geol. Mag.* 1918, **5**, 23–8.

According to Prof. H. H. Swinnerton (*Proc. Geol. Assoc.* 1918, **29**, 16–28) the Keuper basement beds near Nottingham rest on a subaerially-eroded surface of Bunter rocks, and are defined above by a surface of planation upon which rests the Keuper Waterstones conglomerate. They have a marginal or shore facies along a line running northward from Nottingham, but change to an open-water facies east and south-east of this line. R. L. Sherlock (*Geol. Mag.* 1918, **5**, 120–5) shows that the two main gypsum bands in the Keuper keep to definite horizons, and serve as valuable datum-lines for the elucidation of Triassic stratigraphy. These horizons occur at approximately constant depths below the Rhætic, which points strongly to the conformability of the Rhætic to the Keuper.

G. W. Lamplugh has summarised present knowledge of the

underground range of the Jurassic and Lower Cretaceous rocks in East Kent (*Summ. Prog. Geol. Surv. for* 1916, 1917, 40–52). These rocks are shown to form a great wedge with a northward apex intervening between the Palæozoic floor and the basal bed of the Upper Cretaceous. Another interesting fact demonstrated by the deep bores is that the superficial anticline of the Weald rests upon a syncline of the older rocks. H. A. Baker deals with the same facts (*Geol. Mag.* 1917, **4**, 542–50), but discusses especially the unconformity between the Cretaceous and the older rocks.

Trueman, A. E., The Lias of South Lincolnshire, *Geol Mag.* 1918, **5**, 64–73; 101–11).

White, H. J. O., The Geology of the Country around Bournemouth, second ed., *Mem. Geol. Surv., England and Wales*, Expl. of Sh. 329, 1917, pp. 79.

Preller, C. du Riche, Italian Mountain Geology, Dulau & Co., London, 1917, pp. 192.

Stefanini, G., Outline of the Geological History of Venetia during the Neogene, *Amer. Journ. Sci.* 1917, **44**, 299–312.

The tuffs and lavas of the Pahang volcanic series (Federated Malay States), consisting mainly of rhyolites and andesites, are described by E. S. Willbourn (*Geol. Mag.* 1917, **4**, 447–62; 503–14). While it is not possible to define their age exactly, some of the rocks are interstratified with the Raub Shales, and are therefore of Permian age. It is probable that the volcanic activity continued well into the Triassic, as some tuffs are found interbedded with Gondwana shallow-water deposits.

Smith, Warren D., Geologic and Physiographic Influences in the Philippines, *Bull. Geol. Soc. Amer.* 1917, **28**, 515–42.

Benson, W. N., The Geology and Petrology of the Great Serpentine Belt of New South Wales. Part 6. A General Account of the Geology and Physiography of the Western Slopes of New England. *Proc. Linn. Soc. New South Wales*, 1917, **42**, 223–83.

Taylor, Griffith, Antarctic Geology, *Mining Mag.* 1917, **17**, 262–9.

In an important paper on the problem of the Cretaceous-Tertiary boundary in South America, and the stratigraphic position of the San Jorge formation in Patagonia (*Amer. Journ. Sci.* 1918, **45**, 1–53), A. Windhausen shows that there is a stratigraphic as well as a faunistic break between the Cre-

taceous and the Tertiary in Patagonia. This hiatus is correlated with the first phase of the Andean orogenic movements. On the epeirogenic side these movements caused the fragmentation of the Brazilo-Ethiopian continent, and the formation of the South Atlantic basin ; which, in turn, opened the way for the Tertiary transgression over the Patagonian continent.

Petrology.—In his Presidential Address to the Geological Society, Dr. A. Harker considers the relation between igneous action and crustal movements in Britain (*Proc. Geol. Soc.* 1918, **73**, pt. 1, lxvii–xcvi). The distribution of the alkalic and calcic provinces in relation to the great Lewisian, Caledonian, and Hercynian orogenic movements, and to the formation of the North Atlantic basin in Tertiary times, is traced ; and it is shown how closely alkalic and calcic facies follow crustal movements which are, respectively, of dominantly vertical or horizontal character.

In a review of recent work, especially Bowen's, on petrogenesis, Prof. R. A. Daly is led to renewed faith in the general explanation advanced by him for the origin of the alkaline rocks (*Journ. Geol.* 1918, **26**, 97–134). His well-known hypothesis is that of control by the syntexis of basic sediments charged with volatile matter ; and he believes that it explains all the characteristic features of the alkaline rocks, while not open to certain special difficulties which beset Bowen's theory of pure fractional differentiation.

Prof. A. Johannsen has made an elaborate scheme for the quantitative mineral classification of igneous rocks (*Journ. Geol.* 1917, **25**, 63–97). The graphic basis of his method is a double tetrahedron, each of the five trihedral angles of which represent certain mineral constituents: (1) quartz ; (2) potash felspar with the Or. molecule in anorthoclase, (3) plagioclase with the Ab. molecule in anorthoclase; (4) felspathoids; (5) mafic minerals. Any rock of which the mineral composition is quantitatively known may thus be referred to a point within this figure. The method of further subdivision is too complicated to indicate in a short paragraph, but it provides a quantitative basis for, and utilises, the existing nomenclature, with the minimum amount of disturbance and renaming.

Prof. S. J. Shand has also presented a new classification of igneous rocks based primarily on his notion of the degree of

saturation of the minerals present (*Geol. Mag.* 1917, **4**, 463–9). This provides the usual five main divisions. He then utilises the double ratio of Or.-Ab.-An., the "colour-ratio" (felsic/mafic ratio), the crystallinity, and the ratios of specific minerals or groups of minerals, in the order named. Like Holmes (SCIENCE PROGRESS, July 1917, 33) he fails to make the ratio between orthoclasic and plagioclasic elements quantitative, since the double ratio of Or.-Ab.-An. provides no data as to the relative amounts of orthoclase and plagioclase present.

SARGENT, H. C., On a Spilitic Facies of Lower Carboniferous Lava-flows in Derbyshire, *Quart. Jour. Geol. Soc.* 1918, **73**, pt. 1, 11–25.

TYRRELL, G. W., The Igneous Geology of the Cumbrae Islands, Firth of Clyde, *Trans. Geol. Soc. Glasgow*, 1917, **16**, 244–74.

LEITCH, P. A., and SCOTT, Dr. A., Notes on the Intrusive Rocks of West Renfrewshire, *ibid.*, 275–89.

Prof. S. J. Shand describes very fine examples of the granites and gneisses with the peculiar black, glassy veins and dykes, usually known as "trap-shotten gneiss," near Parijs (Orange Free State) (*Quart. Journ. Geol. Soc.* 1917, **72**, pt. 3, 198–221). He gives the appropriate name of pseudotachylyte to the black injected material. Shand is satisfied that this rock is in truly intrusive relation to its walls. The hypothesis that the pseudotachylyte represents a rock-melt developed within the granite by mechanically produced heat is believed to explain the facts best, but the source of the heat and the mechanism of the intrusive process remain obscure.

Brouwer's account of the petrography of the interesting alkali rocks of the Transvaal is brought up to date by a paper in which he describes the geological features of these occurrences (*Journ. Geol.* 1917, **25**, 741–78). The rocks belong to the great Bushveld laccolith, and are held to have been derived by differentiation from the same source as the granites and norites which constitute the main mass of that intrusion.

The Tertiary volcanic rocks of Mozambique, described by A. Holmes (*Quart. Journ. Geol. Soc.* 1917, **72**, pt. 3, 222–79), may be divided into an alkalic group consisting of solvsbergite, ægirine-trachyte, phonolite, basalt, etc., and a calcic group of basalts and andesites, thus affording a further illustration of

the close association of alkalic and calcic rocks in space and time. For both groups of lavas the theory of differentiation by the gravitational settling of the heavier crystals, and an upward movement of the lighter residual magmas, is favoured.

SHAND, S. J., The Norite of the Sierra Leone, *Geol. Mag.* 1918, **5**, 21–3.

LACROIX, A., Les ortho-amphibolites et les ortho-pyroxénites feldspathiques de Madagascar, *Comptes Rendus*, 1917, **165**, 77–83 ; La composition et les modes d'altération des ophites des Pyrénées, *ibid.* 293–8 ; Les péridotites des Pyrénées et les autres roches intrusives non feldspathiques qui les accompagnent, *ibid.* 381–7.

CUSHING, H. P., Structure of the Anorthosite Body in the Adirondacks, with rejoinder by N. L. Bowen, *Journ. Geol.* 1917, **25**, 501–14.

A new collection of highly potassic igneous rocks from Southern Celebes is described by J. P. Iddings and E. W. Morley (*Proc. Nat. Acad. Sci.* 1917, **3**, 592–7). It includes phonolites, trachytes, leucitophyres, minettes, shonkinites, and a new type called *batukite*, which is an extremely mafic leucitophyre. Leucitic rocks have now been found in scattered localities throughout Celebes, and have also a wide extension in other islands of the East Indies.

Prof. R. A. Daly discusses with considerable detail the definition of metamorphism, and of the various sub-processes which are included under that term, and arrives at the following scheme of classification (*Bull. Geol. Soc. Amer.* 1917, **28**, 375–418) :

(*A*) Regional Metamorphism (not caused by eruptive bodies):

(1) Static Metamorphism (orogenic movement not a causal condition).

(2) Dynamic Metamorphism (orogenic movement a causal condition).

(3) Dynamo-Static Metamorphism (load-metamorphism in rocks lying beneath overthrust masses).

(*B*) Local Metamorphism (caused by eruptive bodies):

(1) Contact Metamorphism (magmatic influences in control).

(2) Load-Contact Metamorphism (combination of load and magmatic influences).

In the same publication W. J. Miller has essayed a classification of metamorphic rocks (*ibid.* 451–62). He divides them up into the following groups: (1) Meta-igneous rocks; (2) Meta-sedimentary Rocks; (3) Injection foliates; (4) Foliates of unknown origin; (5) Saprolites (products of rock decay). The first two groups are then subdivided into Foliates and Non-foliates, and finally according to composition. The remaining three divisions are simply classified according to composition.

A. L. Hall gives an interesting description of the contact belt of the Older Granite, in the Barberton District and Northern Swaziland (Transvaal) (*Trans. Geol. Soc. South Africa,* 1918, **20,** 1–36). The commonest rock is andalusite-slate with or without ottrelite; then comes chiastolite-slate, biotite-hornfels, and altered quartzite. The average width of the contact aureole is about three miles, which contrasts remarkably with the twelve-mile aureole of the Bushveld complex. The difference is ascribed partly to different modes of intrusion, partly to differences in the rocks involved; but probably depends mainly on the greater abundance of mineralising agents in the Bushveld complex, which is further attested by the wide distribution of tin ore within or near it.

Teall, Sir J. J. H., Dynamic Metamorphism, a Review, mainly Personal, *Proc. Geol. Assoc.* 1918, **29,** 1–15.

Cox, A. H., Notes on Some South Staffordshire Fireclays and their Behaviour on Ignition, *Geol. Mag.* 1918, **5,** 56–63.

In a paper entitled "A Contribution to the Micropetrology of Coal," G. Hickling makes the interesting observation that the colour-density of coal (observed in thin section or in the streak) increases regularly with the carbon content, and he suggests that with standardised streak-plates it might be possible to determine carbon percentages by this method to well within 5 per cent. He also shows that all coals exhibit at least four elements: lignitoid vegetable tissue; the cuticles from the epidermis of stems and leaves; hard coats of the spores of plants; and finally a dark, structureless substance consisting of minutely-comminuted debris, which forms a matrix for the other constituents.

The investigation of the textures of dolomitic limestones

by staining and other methods by E. Steidtmann (*Bull. Geol. Soc. Amer.* 1917, **28**, 431–50) confirms the conclusion arrived at along other lines, that most dolomites are of marine origin, and that only a minority were formed by the replacement of limestone by the action of underground waters.

Tarr, W. A., Origin of Chert in the Burlington Limestone, *Amer. Journ. Sci.* 1917, **44**, 409–52.

Daly, R. A., Low Temperature Formation of Alkaline Feldspars in Limestone, *Proc. Nat. Acad. Sci.* 1917, **3**, 659–65.

Macnair, P., Notes on the Microscopical Characters of the Blackbyre Limestone in the West of Scotland, *Trans. Geol. Soc. Glasgow*, 1917, **16**, 290–304.

Ries, H., A Peculiar Type of Clay [composed mainly of dolomite rhombs], *Amer. Journ. Sci.* 1917, **44**, 316–8.

Geological Processes.—E. S. Moore describes the activity and products of the five great active volcanoes, White Island, Tarawera, Ruapehu, Ngauruhoe, and Tongariro, in the North Island of New Zealand (*Journ. Geol.* 1917, **25**, 693–714). They are situated along an almost direct N.N.E. line, following a fissure zone of immense size, which may be continued through Tonga and Samoa to Hawaii. The volcanic activity began in the Middle or Lower Miocene, but the main eruptions appear to belong to the Pliocene.

In discussing the origin of calcite and gypsum veinlets in the Silurian and Devonian strata of Central New York, S. Taber (*Journ. Geol.* 1918, **26**, 56–73) comes to the conclusion that they were not deposited in pre-existing openings, but made room for themselves by rending apart the enclosing walls. The necessary energy was supplied by the molecular forces associated with the separation of solids from solution.

In two papers on the coral reef problem Prof. E. W. Skeats discusses the evidence of the Funafuti borings and the bearing of the formation of dolomite upon that question (*Amer. Journ. Sci.* 1918, **45**, 81–90 ; 185–200). He concludes that the nature of the organisms, the rock-textures of the Funafuti bores, and the submarine contours of the island, are only explicable on the subsidence theory of Darwin. This view is supported by cogent evidence of the shallow-water origin of dolomite, and the dolomitisation of many coral reefs.

In an instructive paper on the transportation of debris by icebergs (*Journ. Geol.* 1918, **26**, 74–81), O. D. von Engeln makes

an interesting comparison between the pockets of glaciated stones found in and on the glacial lake clays of Cayuga Lake Valley (New York), and identical deposits found on the tidal flat adjacent to the end of the Columbia Glacier, Alaska, which were dropped by small icebergs.

Major R. A. Marriott adheres to an early explanation of glaciation as due to an increase in the obliquity of the ecliptic (*Journ. Torquay Nat. Hist. Soc.* 1917, 1–14 (reprint)). He accepts General Drayson's astronomical views, whereby a glacial period culminated about 13,000 B.C. and ended 5,000 B.C. Submerged forests are adduced as evidence of the recency of the glacial period; but it is permissible to inquire whether the evidence of raised beaches is not rather in opposition to the thesis of this paper.

JEHU, T. J., Rock-boring Organisms as Agents of Coast Erosion, *Scottish Geogr. Mag.* 1918, **34**, 1–11.

DEELEY, R. M., Mountain Building, *Geol. Mag.* 1918, **5**, 111–20.

POWERS, S., Tectonic Lines in the Hawaiian Islands, *Bull. Geol. Soc. Amer.* 1917, **28**, 501–14.

MINERALOGY AND CRYSTALLOGRAPHY. By ALEXANDER SCOTT, M.A., D.Sc., University, Glasgow.

Mineralogy.—A. F. Rogers (*Journ. Geol.* **25**, pp. 515–40, 1917) discusses the various naturally occurring amorphous substances. He advocates the recognition of amorphous substances as definite mineral species even where a crystalline mineral of the same chemical composition is already known. Different names should be used in the latter case, as, for example, tenorite and melaconite for the crystalline and amorphous hydrated copper oxides respectively. A short account of the more important amorphous minerals is given. Naturally occurring hydrocarbons and glasses are grouped together as "mineraloids," this term being used, therefore, in a more restricted sense than originally proposed by Niedzwiedzki (*Cent. Min.* 1909, pp. 661–3). Many mineralogists will probably disagree with the classification of pitchstone, obsidian, and so forth as "mineraloids" instead of "rocks."

H. Le Chatelier (*Bull. Soc. franc. Min.* **40**, pp. 44–57, 1917) gives a further statement of his views concerning the conditions of formation of tridymite and cristobalite, and some-

what similar conclusions are reached by A. Scott (*Trans. Cer. Soc.* **16**, pp. 137–52, 1917). A. F. Rogers (*Amer. Journ. Sci.* (4) **45**, pp. 222–6, 1918) describes two natural occurrences of cristobalite in rocks from California, the mineral in one case being paramorphous after tridymite.

In a discussion of the origin of flint, E. R. Lankester (*Nature*, **99**, pp. 283–4, 1917) considers that it is formed at a period long subsequent to the deposition of the chalk, and that the main agent is percolating atmospheric water containing silica in solution. The colour of black flint, which is supposed to consist of minute quartz crystals cemented by amorphous silica, is ascribed to carbon. On the other hand, W. A. Tarr (*Amer. Journ. Sci.* **44**, p. 409, 1917) inclines to the view that, so far as certain Lower Carboniferous rocks in Missouri are concerned, the flint has formed at an early stage, and is not a replacement of the calcium carbonate. B. Moore (*Nature*, **99**, p. 324, 1917) believes that the flint is precipitated from colloidal carbonated solutions during the interaction of the calcite and the carbon dioxide to form "acid" calcium carbonate. "Synthetic flint," which, however, is much less hard than the natural material, has been obtained in this way. S. C. Bradford (*ibid.* p. 324, 1917) considers the formation due to crystallisation from gels, while G. A. J. Cole (*Geol. Mag.* (6) **4**, pp. 64–8, 1917) adopts a theory analogous to that of Liesegang, and holds that many flints are formed by the rhythmic precipitation of silica from solution.

J. V. Samoilov (*Min. Mag.* **18**, pp. 87–98, 1917) ascribes the presence of certain minerals, barytes, celestite, etc., in some sedimentary rocks to the action of marine organisms which concentrate the salts from sea water.

R. A. Daly (*Proc. Nat. Acad. Sci.* **3**, pp. 659–65, 1917) reviews previous work on the low temperature formation of felspars, and describes a new occurrence in a dolomite in the Rocky Mountains which seems to be analogous to the occurrence of felspar in certain European Jurassic rocks. He concludes that the felspar must have been precipitated on the sea floor at a temperature under 100°, but gives no theory as to the chemical conditions.

H. Reis (*Amer. Journ. Sci.* **44**, pp. 316–18, 1917) describes a peculiar clay from Texas, which, under the microscope, is seen to be almost wholly composed of minute crystals of

dolomite, with a very small proportion of interstitial clay. Chemical analysis shows 98·5 per cent. of calcium-magnesium carbonate and 1·5 per cent. alumina and iron oxide. The plasticity is supposed to be due to the fact that the rhomb faces of adjacent crystals are separated by a very thin film of water which allows the faces to slip over each other without breaking the actual contact. Another peculiar clay is described by E. W. Hilgard (*Proc. Nat. Acad. Sci.* **2**, pp. 8–12, 1916). Chemically, it is a hydrated magnesium calcium silicate containing a small proportion of sodium salts and about 15 per cent. of absorbed water. The material is mainly colloidal except for some small grains of crystalline carbonates.

A number of papers dealing with the mineralogy of sulphides have appeared recently. R. C. Wells and B. S. Butler (*Journ. Wash. Acad. Sci.* **7**, pp. 596–9, 1917) describe a new mineral, the chemical composition of which is sulphide of tungsten (WS_2), and to which they have given the name "tungstenite." The mineral is black and opaque, has a high specific gravity, and is not attacked by mineral acids. Another new sulphide belonging to the jamesonite group is described by E. V. Shannon (*Amer. Journ. Sci.* **45**, pp. 66–70, 1918). It is probably orthorhombic, has the formula $5PbS,2Sb_2S_3$, and has been named "mullanite." An occurrence of the rare mineral chalmersite ($CuFe_2S_3$) as a prominent constituent of some ore-bodies in Alaska is described by B. L. Johnson (*Econ. Geol.* **12**, pp. 519–25, 1917).

In recent years great advances have been made in the examination of opaque minerals by methods analogous to those used in metallography, and much valuable information regarding the structure and genesis of ore deposits has been obtained by these means. The methods of identification have been admirably described by J. Murdoch (*The Microscopical Determination of the Opaque Minerals*, New York, 1916), and in a recent paper W. L. Whitehead (*Econ. Geol.* **12**, pp. 697–716, 1917) gives a fairly exhaustive treatment of the technique of "mineragraphy." The methods of grinding and polishing, of micro-examination and photomicrography are discussed, and Murdoch's tables are supplemented by several new reactions for certain minerals. The methods of "light-etching" which the author recommends for the discrimination of silver minerals are described in detail.

F. N. Guild (*ibid.* pp. 297–353, 1917) has made an elaborate micro-study of silver ores, and has critically examined the criteria used in the diagnosis of the minerals concerned. In the paper, which contains a very fine series of photomicrographs, numerous deductions are made concerning the order of deposition of the various minerals. The frequent occurrence of graphic structure is interesting, and is ascribed to two causes, firstly, replacement, as in the "intergrowths" of stromeyerite (CuAgS) with chalcopyrite and bornite, and secondly, simultaneous deposition followed by segregation, as in the intergrowths of stromeyerite with chalcocite, argentite, and galena. The peculiar structure which has been held to indicate heterogeneity in stromeyerite is shown to be due to the presence of chalcocite as an impurity, stromeyerite being held to be a true double sulphide of copper and silver. By similar methods, E. S. Bastin (*ibid.* pp. 219–36) has examined the silver ores of Cobalt, Ontario. By analogy from quantitative synthetic experiments by C. Palmer on the precipitation of silica from solution by certain arsensides (*ibid.* pp. 207–18, 1917), it is concluded that the replacements in the ores of this region can be explained by iron-bearing solutions of silver sulphate, probably containing free acid in addition.

These methods have also been utilised by L. P. Teas (*Bull. Amer. Inst. Min. Eng.*, pp. 1917–31, 1917) in an examination of the relationships of sphalerite, and one interesting conclusion reached is that chalcopyrite is present when the sphalerite has been deposited from ascending juvenile waters, but absent when meteoric solutions constitute the origin.

An elaborate paper by A. B. Dobrovolski (*Archiv. Kemi, Min. o. Geol.*, **6**, pp. 1–53, 1916) gives a critical summary of the work which has been done on the crystallography of ice. The form development, habit, and twinning of the natural mineral are discussed, and the conclusion reached that it belongs to the tourmaline class (ditrigonal pyramidal) of the hexagonal system, thus confirming Nordenskiöld's suggestion that the mineral is hemimorphous. The axial ratio, however, is still very much in doubt. The interesting question as to whether the unstable modifications, described by Tammann, Wallerent, and others, and capable of existence at atmospheric pressure, occur in Nature, is also left in doubt.

Meteorites.—The most interesting paper on this subject

which has appeared for some time is one by G. T. Prior (*Min. Mag.* **18**, pp. 26–44, 1916) on the classification of meteorites. The chief classification in vogue at the present time is the Rose-Tschermak-Brezina scheme, but this is somewhat artificial, and, particularly in the case of the chondrites, very complicated. Farrington has proposed one which is based on the American Quantitative Classification of igneous rocks, but this naturally is open to the same criticisms as the latter. Berwerth's scheme, based on the metallographic relations of the system iron-nickel, was devised at a time when this system was very imperfectly known, and no use has so far been made of recent work on the equilibrium of this system. In any case such a scheme could only be applied to the iron meteorites. Prior starts from the principle that the source of all meteorites was a nickel-iron poor in nickel and containing other metals in a free state. The first accession of oxygen results in the oxidation of the elements of lower atomic weight, and these unite to form such minerals as enstatite, giving rise to such rare types of meteorites as the Daniel's Kuil meteorite (*ibid.* pp. 1–25, 1916) in which the iron is all present in the metallic state. The further addition of oxygen results in partial oxidation of the iron and consequent enrichment of the metallic portion in nickel. As the nickel-iron becomes progressively richer in nickel, the silicates becomes richer in iron. Meteorites, therefore, are divided in three classes, chondrites, achondrites, and irons, and each class is subdivided into five groups according to the amount of nickel in the nickel-iron.

Sir W. Crookes (*Phil. Trans. Roy. Soc. A.* **217**, pp. 411–30, 1917) has carried out spectrum analyses of many aerolites, and concludes that the stony meteorites must have had a common origin, probably being fragments of some cooled planet, as the relative proportions of the four chief metals, iron, chromium, nickel, and magnesium, are more or less uniform. The radium content of a number of meteorites has been determined by T. T. Quirke and L. Finkelstein (*Amer. Journ. Sci.* (4) **44**, pp. 237–42, 1917), and it is found that in stony meteorites the amount is about one-fifth the quantity present in an average granite, while the iron-meteorites contain practically no radium.

G. P. Merrill (*Amer. Journ. Sci.* (4) **42**, pp. 322–4, 1917) notes the occurrence of calcium phosphate in many stony

meteorites in a form which closely resembles the member of the apatite group which has been termed francolite. E. T. Wherry (*Amer. Miner.* **2**, p. 119, 1917), however, points out that the mineral differs from francolite in optical properties, and as it contains no fluorine, chlorine, carbon dioxide, etc., it should be excluded from the apatite group and regarded as a pure calcium phosphate. The same writer (*ibid.*, pp. 80–81, 1917) has measured a crystalline phosphide of iron which occurs in Ruff's Mountain meteorite, and which is supposed to be identical with the tetragonal phosphide of iron measured recently by L. J. Spencer (*Min. Mag.* **17**, pp. 340–43, 1916), the angular values in the two cases being similar.

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., East London College, University, London.

Anatomy.—Kashyap (*Annals of Botany*, July and Oct. 1917) finds that there is a special endodermal sheath for each bundle in the nodes of both stem and rhizome of *Equisetum debile*, but that two distinct endodermal sheaths are present in the internodes. Occasionally these two fuse around islands of parenchyma. The prothalli of this species when crowded are unisexual, but when growing isolated, become large and at first bear archegonia and subsequently antheridia.

In the same journal Maybrook describes the anatomy of the haustoria of *Pedicularis vulgaris*, which apparently possess no phloem. The place of this tissue is taken by elongated parenchymatous cells, whilst the xylem tracheids contain abundant protoplasm.

The late Miss Holden describes two new species of *Dadoxylon* under the names *D. indicum* and *D. bengalense*, both from the Permo-Carboniferous rocks of India. The former has a large non-discoid pith between which and the xylem proper is a jacket of transfusion cells. The primary wood consists of separate endarch bundles with a brood transition zone of metaxylem. The two species agree in the possession of distinct annual rings.

Dr. Scott, as a result of his study of "The Heterangiums of the British Coal Measures" (*Journ. Linn. Soc. Bot.*, vol. xliv.), suggests the establishment, on anatomical grounds, of two sub-genera. The first of these, for which the name

Polyangium is proposed, includes *H. shorensis*, *H. tiliæoides*, *H. Lomaxi*, and probably *H. punctatum*, *H. Renaultii*, *H. Duchartrei* and *H. bibractense.*

The three first-named, and probably the latter species also, agree in exhibiting a leaf trace which starts from the stele as two distinct bundles which divide to form four or more strands in the petiole. The protoxylem groups tend to be pronouncedly exarch, and are more distinct than in the other species.

The second sub-genus, *Eu-Heterangium*, probably represents the older type, and includes *H. minimum*, *H. Grievii*, *H. alatum*, *H. polystichum*, *H. Schusteri*, and *H. Sturii.* These are characterised by less distinct protoxylem bundles, either centrally mesarch or with a tendency towards exarchy. The leaf-trace here starts as a single strand which either remains undivided or only forms two bundles in the petiole. *H. Andrei* appears to combine the features of the two sub-genera, a feature all the more interesting in view of the intermediate character of this species as between typical Heterangiums and the genus *Lyginopteris.*

The occasional presence of ray tracheids in the secondary xylem of *Quercus alba*, similar to those of *Pinus*, is noted by S. J. Record in the *Botanical Gazette* (Nov. 1917).

Mrs. Arber (*Annals of Bot.* Jan. 1918) records interfascicular cambium in *Acorus calamus*, *Tamus communis*, *Tritonia* sp., *Potamogeton natans*, *Ophiopogon japonicus*, *Phormium tenax*, and *Veratrum album.*

Taxonomy.—In the *Journal of Botany* for November 1917, W. Watson describes a new lichen from the Carboniferous Limestone of Ebbor Gorge, Somerset, viz. *Staurothele ebborensis.*

A number of new species belonging to the genera *Drimys*, *Garcinia*, *Elæocarpus*, *Eugenia*, *Rondia*, *Lobelia*, *Pratia*, *Melodinus*, *Bulbophyllum*, and *Freycinetia*, are described from Bellenden-ker, North Queensland, by Spencer Moore. In the same journal for December, Dr. Wernham contributes further descriptions of Tropical American Rubiaceæ. These include new species of *Psychotria* and *Palicourea*, and the description of a new genus *Raritebe* belonging to the tribe Hamelicæ.

Further new species from the African collections of Archdeacon Rogers are described by Spencer Moore (Jan. and Feb.

1918), and Messrs. T. and T. A. Stephenson describe a new form of *Helleborine viridiflora* from the Isle of Wight. The former include members of the following genera: *Polygala, Thespesia, Scabiosa, Helichrysum, Senecio, Gazania, Wahlenbergia, Cyphia, Jasminum, Lindernia, Dyschoriste, Blepharis, Barleria, Selago, Plectranthus,* and *Acrocephalus.*

Mr. Hornby (*New. Phyt.*, Jan. and Feb.) describes a new species of freshwater alga from Britain, viz. *Endoderma Cladophora.*

Fritsch has investigated collections of algæ from the Cape Peninsula, and describes numerous varieties and several new species (*Annals of the S.A. Museum,* vol. ix. pp. 483–611). Of these the most interesting are two new species of *Ecballocystis* and *Sphæroplea africana.* The transverse septa in the latter arise by the ingrowth of cellulose processes which in many cases, and perhaps always, leave a central aperture, so that it is an open question whether *Sphæroplea* is septate in the true sense of the term. *Scenedermus cohærens*, n.sp., is a remarkable member of the genus in which the cells normally adhere together to form large plate-like colonies of as many as a hundred cells.

The several interesting additions to the Cyanophycæ include *Schizothrix polytrichoides,* in which the trichomes within the sheath of the principal branches may number more than fifty, and often present a rope-like appearance, and two species of *Dichothrix.*

Apart from those mentioned above illustrations and diagnoses are given of new species or varieties belonging to the following genera: *Schizochlamys, Cœlastrum, Ulothrix, Stigeoclonium, Gongrosira, Cylindrocystis, Penium, Euastrum, Cosmarium, Xanthidium, Staurastrum, Spondylosium, Zygnema, Cylindrospermum, Euglena,* and *Trachelomonas.*

The Rev. W. Johnson (*Naturalist,* Mar. 1918) records a lichen new to Great Britain, viz. *Sarcopyrenia gibba,* from the shore rocks at St. Bees, Cumberland.

A new species of saxifrage (*S. Drucei*) and two new varieties, *S. Sternbergii v. gracilis* and *S. hypnoides v. robusta,* are described by Rev. E. S. Marshall, all from Ireland (*Journal of Botany,* Mar.).

Further species of Rubiaceæ from New Guinea are described by Dr. Wernham, belonging to the genera *Uncaria, Dolicho-*

lobium, *Xanthophytum*, *Mussænda*, *Lucinæa*, *Urophyllum*, *Lasianthus*, *Tarenna*, *Randia*, *Gardenia*, *Pavetta*, and *Morinda*.

Ecology.—W. H. Pearsall contributes an important paper to the current number of the *Journal of Ecology* on the "Aquatics and Marsh Vegetation of Esthwaite Water." A number of plant societies are recognised related to differences in the habitat conditions chiefly dependent upon the silt and organic content of the substratum and differences in the intensity of illumination. Exposure as a factor seems to be effective mainly through its influence on the rate of sedimentation.

The succession of the various societies depends on gradual increase in organic content and the progressive rise in level of the substratum.

Litorella lacustris and *Lobelia Dortmanna* occur to a depth of from ·3–1·2 metres where the light intensity, in terms of that above the water, rarely falls below 0·1. The substratum is usually gravelly, and at first unstable, but becomes fixed by the Strapwort after which the Water Lobelia is able to become established. In exposed situations, at a depth of from 1·25–1·5 m., the Milfoil is dominant, associated with *Potamogeton alpinus* and *P. heterophyllus var. longipedunculata*. *Isoetes lacustris* is indicative of situations where the deposition of silt is very slow, and occurs on rounded stones at a depth of from 1·5–2·7 m. where there is a low intensity of light (·06–·03).

A society of species characterised by their linear leaves is found in similar light and depth conditions as *Isoetes*. It reaches its full development, however, in sheltered situations where there is mud containing under 15% of organic matter. The chief species are *Potamogeton pusillus* (agg.), *Naias flexilis*, and *Callitriche autumnalis*. Where the organic content is very low (5·5–8·9%) *Naias* becomes dominant, whilst where it is high (15·8–18·3%) or silting is slow, *Nitella flexilis* flourishes.

Under conditions of good aeration and high organic content (18·3–19·3%) *Fontinalis antipyretica* is encountered. *Sparganium minimum* is also found where there is considerable organic material (21·3–23·6%), usually at depths from 2·4–3 m.

The large white Water Lily grows to a depth of 2·5 m. in sheltered situations where the organic content is high (23–24·6%), but the small-flowered species (*Castalia minor*) seldom occurs at a depth of more than 1·2 m. and is found where the

organic matter is abundant, combined with lack of bases. Probably scarcity and abundance of nitrates determine respectively the occurrence of *C. minor* and *C. alba*.

Reed swamps, chiefly composed of *Phragmites* and *Scirpus*, are found in stable situations that are sheltered with from 30–60% of organic material. The former extends to a depth of 0·92 m. and the latter to 1·3 m. Where the mud contains abundant silt and decay is rapid, *Typha latifolia* becomes dominant.

In areas which are exposed and where there is over 60% of organic material, a society is formed of various species of Sedge.

Under conditions of relatively rapid inorganic sedimentation, the successive dominants are: *Naias flexilis*, *Potamogeton pusillus*, etc., *Nitella flexilis*, *Sparganium minimum*, *Castalia alba*, *Phragmites*, and *Scirpus*. This succession shows a progressive increase in organic material and decrease in depth.

In the same journal Mr. Tansley describes experiments on the relation as to soil conditions between the calcicolous *Galium sylvestre* and the calcifugous *Galium saxatile*. It appears that both can establish and maintain themselves on either peaty or chalky soil, but that the rampant Heath Bedstraw is heavily handicapped when growing on a calcareous soil, and that the converse also is true. On chalk, *G. saxatile* tends to become chlorotic and die, and the percentage of germination is lower than on acid peat. Competition between the two species appears to be effective through the direct effect of the more vigorous growth of one species on its appropriate soil.

In the *Journal of Agricultural Research* (Oct. 1917), H. L. Shantz and R. L. Piemeisel contribute an account of the Fungus Fairy Rings of Eastern Colorado and their effect on vegetation. This covers some 55 pages, and is profusely illustrated with nearly 50 photographic reproductions and a number of text figures.

Three types are recognised, viz. those caused by *Agaricus tabularis* as a result of which the vegetation is either killed or badly damaged; those caused by *Clavatia*, *Catastoma*, *Lycoperdon*, *Marasmius*, etc., in which the vegetation is stimulated; and those caused by *Lepiota* species which produce no visible effect. A typical ring formed by *A. tabularis*

consists of three zones surrounding a central area of short grass (*Boutelona gracilis, Bulbilis dactyloides*). Next the central area is a zone exhibiting more vigorous growth and different floristic composition, next follows a bare zone of dead vegetation where usually only a few annuals are present. Still further out is a second narrow stimulated zone, where the fruit bodies of the fungus are produced. When the mycelium first begins its development, it spreads out equally in all directions; the stimulation in the outermost zone is attributed to the utilisation of the organic material available by the fungus and consequent increase in nitrogen content. The next phase represented by the bare zone is brought about by the great increase in mycelial development and consequent decrease in soil moisture, so that only ephemeral species flourish if rains are frequent. The third phase represented by the broad inner zone of stimulated vegetation marks the period of decay of the mycelial complex with accompanying liberation of nitrogenous material; this phase is associated with an invasion of ruderal species which give place to short-lived grasses, and finally to the grasses of the natural turf which occupy the centre. The annuals of the bare zone comprise such species as *Plantago purshii, Festuca octoflora, Hedeoma hispida,* and *Lepidium ramosissimum.* These species luxuriate in the inner stimulated zone, and in addition the following are characteristic: *Chenopodium incanum, Lappula occidentale, Cryptanthe crassisepala,* and *Amaranthus blitoides.* The third zone comprises *Schedonnardus panniculatus* sometimes accompanied by *Malvastrum coccineum* and *Sitanion hystrix.*

Morphology.—In the January number of the *Annals of Botany,* Prof. Bower makes a further contribution to the study of the Phylogeny of the Filicales dealing with the *Pteroideæ.*

P. A. Murphy describes the sexual organs of *Phytophthora erythroseptica.* The oogonial rudiment grows through the antheridium and on emerging at the other side forms an oogonium. Each sexual organ primarily contains numerous nuclei, but about two-thirds of these degenerate whilst the remainder undergo a single division. The oogonium exhibits differentiation into ooplasm and periplasm and, as a consequence of the degeneration of all the nuclei in the latter and one of the two daughter nuclei of the former, a uninucleate oosphere

results. All the nuclei of the antheridium also degenerate except one which, together with the greater part of the cytoplasm, passes through the fertilising tube to the oosphere.

Dr. Weston describes *Thraustotheca clavata*, an aquatic fungus belonging to the *Saprolegniaceæ*. The sporangiospores are non-motile and escape by bursting the sporangium wall. The zoospores are laterally biciliate and reniform in side view.

Cytology.—Prof. Mottier deals with the origin of Chloroplasts and Leucoplasts which he regards as derived from granular or rod-shaped primordia. These latter are considered to be permanent structures of the cell originating only by division, and therefore ranking with the nucleus. As such it is suggested they may share in the transmission of hereditary characters. There are in addition similar bodies to those which develop into plastids, and these chrondriosomes are likewise regarded as permanent structures.

Genetics.—The results of A. B. Stout's experimental breeding of chicory show (*Journal of Genetics*, Feb. 1918) that self and cross-incompatibles occur, and that even after three generations a self-sterile roce may still give rise to sporadic plants which are self-compatible. The offspring of the latter, however, do not breed true in this respect. The degree of self-fertility varies and self-sterile individuals arise sporadically from self-fertile parents. No simple Mendelian formula fits the results obtained, which indicate a great variability in hereditary transmission.

PLANT PHYSIOLOGY. By Prof. V. H. Blackman, Sc.D., F.R.S., Imperial College of Science and Technology, London. (Plant Physiology Committee.)

It is well known that in many plants there is a well-marked antagonism between growth and reproduction. This is clearly seen in the case of many fruit trees where the conditions which lead to active vegetative growth may be inimical to the reproductive processes. In such cases the reduction of vegetative growth, as by root pruning, may bring about vigorous flower and fruit production. The study of the effect of external conditions on these two processes, growth and reproduction, is obviously of great importance. In the case of the higher plants, however, the difficulty of investigating such a problem is increased by the close connection under ordinary conditions

of the various external factors; it is thus very difficult to alter one factor without altering others at the same time. In the case of algæ and fungi, which can be grown in the laboratory under artificial conditions that can be easily varied at will, the difficulties are not so great, and it is not surprising that in this field of work our knowledge is mostly based on experiments with the lower organisms. The art of growing micro-organisms, such as bacteria, fungi and also algæ, in pure culture has been carried to a high pitch of perfection, but since the growth of bacteria and fungi takes place within such wide limits and under a wide range of conditions, the analytic study of environmental factors has been largely neglected in the development of pure-culture methods. Some bacterial parasites of animals are markedly sensitive to temperature conditions, but the majority of fungi will grow within a wide range of temperature so the effect of temperature on the growth of fungi has not been fully studied. Again it is convenient in culture-work to grow fungi in tubes plugged with cotton-wool, *i.e.* under conditions in which gaseous exchange must be reduced to a low level. Yet, since most fungi tolerate readily such conditions, the effect of aeration on the growth of fungi has been neglected. A certain amount of analytic work with the help of synthetic media was carried out by earlier workers, such as Pasteur and Raulin, and later by Winogradsky and Beijerinck. In 1896 Klebs published the first of his series of papers on the effect of external conditions on algæ and fungi grown in pure culture. Klebs did not confine himself to the effect of such conditions on growth, but he studied the effect of external conditions on reproduction also. Klebs put forward the view that growth and reproduction are processes which depend upon different conditions, and that as long as the conditions favourable for growth are present, reproduction in the lower organisms does not occur. Klebs brought out also a point of great importance, that the conditions suitable for reproduction are more restricted than those for growth, so that reproduction is liable to be inhibited by too high or too low intensity of some factor.

It is well known to mycologists and plant pathologists that though there is little difficulty in growing most fungi in pure culture, the production of reproductive organs by fungi under these conditions is quite another matter. Anything

which will enable one to control the reproductive processes of such fungi is thus not only of great physiological interest, but of considerable practical importance in plant pathology. Reference may thus be made in this article to a valuable paper —not of most recent date, but very generally overlooked—by G. H. Coons on the factors involved in the growth and pycnidium formation of *Plenodomus fuscomaculans* (*Journ. Agric. Research*, v. 713–769, 1916), in which the relation of growth and reproduction to external conditions is very carefully studied. The fungus in question is one of the Sphæropsidaceæ and is parasitic on the apple.

It was found that in agreement with the dictum of Klebs there was a wider range of conditions suitable for growth than for reproduction. A small amount of growth will take place in conductivity water (sp. cond. 2×10^{-6}) in vessels of resistance glass. Such a growth is certainly very surprising. The number of spores used for inoculation was not more than fifty, so the growth observed could not be explained by transference of organic material from the spores. The salts required for development under these conditions, and in ordinary distilled water, were no doubt obtained from the glass, but the source of nitrogen, and especially of carbon is obscure; there is the possibility, first suggested by Elfving, that volatile substances may be absorbed from the laboratory air. It is interesting to note that while in conductivity water there was a just perceptible growth, in ordinary distilled water the growth was not only better, but a few pycnidia were actually produced. Under the conditions of the experiment conductivity water is the lower limit for growth, but "distilled water" the limit for reproduction. As Coons points out, the sensitiveness to extremely small quantities of salts renders the problem of determining the necessary elements for this fungus almost insoluble with our present technique.

Up to a certain limit, possibly up to M/50, increase in concentration of the food supply increases reproduction; after that point increase of food supply retards and finally inhibits reproduction. The organism was found to be sensitive to the reaction of the medium, and the different effect of different media was largely due to the reaction of medium not only at the start, but in later stages of growth. Many media, while having a favourable reaction at start, showed an unfavourable reaction

later, with corresponding checking of growth. It was found that while growth can take place between the acid and alkali limits of $+30$ and -10 to phenolpthalein, yet reproduction is stopped by a reaction only slightly on the acid side of neutrality. Maize broth is a much better substratum than oat broth, but if the latter be acidified with an acid phosphate, or even hydrochloric acid, it becomes almost as good a medium as maize. The various laboratory media are rightly condemned as " rather purposeless, clumsy devices in which this organism is overfed." Progress can only be made by the use of synthetic media, and a large number of experiments were made with a medium containing in various proportions potassium dihydrogen phosphate, magnesium sulphate, maltose and asparagin. A solution containing these four substances in concentrations of M/100, M/500, M/100, M/500, respectively, was found to be an almost ideal culture medium for the growth and reproduction of this fungus; the pycnidium production was far higher than in any other medium. In this synthetic medium the inhibition of reproduction as a result of increasing or decreasing the carbohydrate or asparagin was very marked.

Light was found to be essential for reproduction, though not for growth. The light need not be continuous, for a short exposure to strong diffuse light of cultures which are ready to produce pycnidia will allow, for a time, the production of these bodies in the dark. Abundant aeration was found to be essential, while transpiration was found to be a factor of only secondary importance.

The extremely interesting and important observation was made that *the stimulus of light could be replaced by a few drops of hydrogen peroxide*. This observation was extended, and it was shown that a number of other oxidising agents, such as nitric acid, potassium permanganate, ferric chloride, would produce the same effect and cause the production of pycnidia in the dark. The view is put forward that among the parts of an organism there exists a strong competition for oxygen, and that under conditions which favour growth the available oxygen is all used for ordinary metabolic processes. If the food supply is reduced, as by transfer to media of lower concentrations or to distilled water, a " hunger-state " sets in and ordinary respiration is lowered. If the organism is now stimulated by light or by some oxidising agent, oxidation of the richer cell

materials, such as fat and protein, sets in, and a large amount of energy is set free. " This energy is used in reshaping the reserve stuffs into complex protein bodies, the spores."

ZOOLOGY. By Prof. Chas. H. O'Donoghue, D.Sc., F.Z.S., University of Manitoba, Winnipeg, Canada.

Protozoa.—Dobell has written "On *Oxnerella maritima*, nov. gen., nov. spec., a New Heliozoon and its Method of Division; with Some Remarks on the Centroplast of the Heliozoa " (*Quart. Journ. Micro. Sci.* vol. lxii. Pt. 4, Dec. 1917). This new form belongs to the same group as *Actinosphærium* but differs considerably from any other member indeed to such an extent as to justify regarding it as belonging to a separate genus. It is uninucleate, generally spherical in form and of a mean diameter of 14μ. A detailed account of its division, which is somewhat similar to that of *Acanthocystis*, is given, particular reference being made to the nucleus and the " central granule " for which the author proposes the name of centroplast. This body plays the part of the centrosome in a metazoon cell, but has other functions in addition, so that it is suggested that it is not strictly accurate to regard the two structures as analogous but only as homologous.

Another Heliozoon is dealt with by Doflein in " Studien zur Naturgeschichte der Protozoen: IX. *Rhizochrysis*, eine Ubergangsform unter den niederen Protozoen " (*Zoolog. Jahrb.* 1917). This is a well-illustrated description of a little-known form of a genus first described by Scherffell in 1901. A very complete account is given not only of its structure and mode of life, but also of its division, of which a very full series of stages was obtained. The present species is apparently new and named *Rhizochrysis pascheri.* Its main interest, perhaps, lies in the fact that it is a member of the Chrysomonadina, and is one of the forms that has a good deal in common with the Rhizopoda, and so is undoubtedly a lowly member of the Heliozoa. According to the author it recalls most nearly the Vampyrellidæ.

Other papers include: " New Facts and Views Concerning the Occurrence of a Sexual Process in the Myxosporidian Life Cycle " by Erdmann (*Amer. Naturalist,* Dec. 1917).

Invertebrata.—Under the title of " Worm Nodules in Cattle "

(Melbourne, 1917), the Commonwealth Advisory Council of Science and Industry have published their second Bulletin dealing with Onchocerciasis in Australian cattle. Although it has yet to be proved that beef infected with these worm nodules is in any way injurious to man, indeed there are good grounds for supposing it is not, the British authorities will not allow the importation of infected parts. This entails in Australia a considerable loss of money owing to the actual loss of the joints, the cost of removing them, and the fall in price consequent upon the disfigurement of the carcase, and in this country it means a reduction of the amount of meat coming in. In order to reduce this loss to a minimum and to discover whether the disease could be combated in any way, a special committee was appointed to enquire into its distribution, means of transmission, and, if possible, preventive measures. The present pamphlet gives a summary of the work so far done and future lines of inquiry suggested.

Other papers include "Uber die Entwicklung und temporale Variation des Keimdotterstockes und Eibildung von *Pterodina patina* Müll.," by Hartman (*Zoolog. Jahrb.* 1917).

A useful paper, "Notes on the Morphology of *Bathynella* and some allied Crustacea," has been published by Calman (*Quart. Journ. Micro. Sci.* vol. lxii. Pt. 4, Dec. 1917). The form, a number of new details in the anatomy of which are here added, is of importance as it is a representative of the Syncarida, a group commonly found in the Carboniferous but represented now only by this species and others from Australia and Tasmania. Compared with its allies, *Bathynella* is either primitive or degenerate, and is of small size—one of the smallest of the Malacostraca. Until its rediscovery in 1914 comparatively little was known of it.

Other papers include: "South African Talitridæ," by Stebbing (*Ann. and Mag. Nat. Hist.* Dec. 1917); "Note on the Structure of the Maxillary Gland of *Cypridina Hilgendorfii*" (*Journ. of Morph.* vol. xxix. Sept. 1917): "On an apparently Undescribed English Saltatorial Mite (*Speleorchestes poduroides* n.sp.), belonging to the family Cupodidæ (*Prostigmata*)," by Hirst (*Journ. Zool. Research*, Nov. 1917); "Description of a New Species of Isopoda of the Genus *Paridotea Stebbing*," and "Descriptions of Some Further new Variations of British Woodlice," both by Collinge (*ibid.*); "Notes on Myriapoda:

VII. A new member of the order Ascospermophora (*Jacksoneuma bradæ*, gen. et sp. nov)," by *Brade-Birks and Brade-Birks* (*ibid.* Dec. 1917); "On the Re-occurrence of *Ligidium Hypnorum* (Cuv.) in Great Britain," by Collinge (*ibid.* Dec. 1917).

At the present time considerable importance attaches to the study of lice and their relations to man, as under war conditions, where cleanliness is a matter of great difficulty, if not impossibility, infection among the troops is widespread. The matter becomes urgent in view of the fact that these pests undoubtedly carry certain diseases. The following three papers by Nuttall then should prove of value: "Bibliography of Pediculus and Phthirus, including Zoological and Medical Publications dealing with Human Lice, their Anatomy, Biology, Relation to Disease, etc., and Prophylactic Measures directed against them"; "The Part played by *Pediculus humanus* in the Causation of Disease"; and "The Biology of *Pediculus humanus*" (all in *Parasit.* vol. x. Nov. 1917). In the second it is shown that *Pediculus* undoubtedly carries both typhus and relapsing fevers, and in addition it may carry plague and in a mechanical way "mycotic or pyogenic infections like farus, pityriasis, impetigo contagiosa, and furunculosis." Various pathological conditions appear to follow more easily from the general lowering of the vitality brought about by pediculosis. The third paper is a very full account of the general biology of the pest, including its modes of dissemination and prevalence, feeding, and its reproductive and life history. Certain reactions to various stimuli are also described, and the whole paper will form a good base from which to attack the parasite at the vital points in its life cycle.

Other papers include: "On Three new Parasitic Ascari" (*Ann. and Mag. Nat. Hist.* Dec. 1917); "Ant-like Spiders from Malaya, collected by the Amandale Robinson Expedition 1901–2," by Hirst (*Proc. Zool. Soc.* Oct. 1917), and "Notes on Head and Body-Lice and upon Temperature Reactions of Lice and Mosquitoes," by Howlett (*Parasit.* vol. x. Nov. 1917).

Two papers of general interest to students of the germ-cells of insects have been produced by Hegner: "The Genesis of the Organisation of the Insect Egg: I. The Complexity of Organisation of the Insect Egg" (*Amer. Naturalist,* Nov. 1917), and

"The Genesis of the Organisation of the Insect Egg: II. Interaction of Nucleoplasm and Cytoplasm" (*ibid.* Dec. 1917). By a detailed investigation of the internal structure and relations of the egg of beetles the author is led to the conclusion that at the time of fertilisation the ovum is a mosaic of different cytoplasmic units each predetermined to produce definite parts of the embryo. This complex results from the interaction of nucleus and cytoplasm which is particularly noticeable during such periods of activity as the extrusion of chromidia. The importance of such studies will be realised when we consider the many cases where the cytoplasm controls even the behaviour of the chromatin.

Other papers include: "Descriptions of New Pyralidæ of the Sub-families *Hydrocampinæ, Scoparianæ,* etc." (*Ann. and Mag. Nat. Hist.* Oct. 1917) and "Descriptions of New Pyralidæ of the Sub-family *Pyraustinæ*" (*ibid.* Nov. 1917), both by Hampson; "New Species and Forms of Sphingidæ," by Joicey and Kaye (*ibid.* Oct. 1917); "New Indo-Malayan Lepidoptera," by Swinhoe (*ibid.* Nov. 1917); "New South American Rhopalocera," "New South American Arctiidæ," "New Butterflies from Africa and the East," "Gynandromorph of *Papilio lycophron,*" and "Three Aberrations of Lepidoptera," all by Joicey and Talbot (*Proc. Zool. Soc.* Nov. 1917); and "Notes on a Collection of Heterocera made by Mr. W. Feather in British East Africa," by Fawcett (*ibid.*).

"The Structure, Bionomics, and Forest Importance of *Myelophilus minor,* Hart," is the subject of a paper by Ritchie (*Trans. Roy. Soc. Edin.* vol. lii. Dec. 1917). Two species of *Myelophilus* are indigenous to Britain and well known to the forester. The present note is an anatomical and bionomic study of the less known of these. They are very destructive to pine plantations in all stages, feeding on the young shoots and boring into the stem for breeding purposes. It appears to be confined to the Scots Pine, though on the Continent it attacks other trees, but luckily enough in Scotland it is only able to pass through one generation in a year. However, it is possible for the same parents to have two broods in the year after an interval during which the reproductive organs recover their activity. To some extent the pest can be controlled by arranging trap trees in the plantation.

Other papers include: "Contributions to the Knowledge

of the Family Chermesidæ: No. 1, The Biology of the Chermes of Spruce and Larch and their Relation to Forestry," by Steven (*Proc. Roy. Soc. Edin.* Dec. 1917); "A Note on the Coleopterous Genus *Euxestus*," by Arrow (*Ann. and Mag. Nat. Hist.* Nov. 1917); "The Homoptera of Indo-China," by Distant (*ibid.* Oct. 1917); "Records of some British *Symphyla*," by Bagnall (*ibid.* Nov. 1917); "Notes on Fossorial Hymenoptera: XXIX. On new Ethiopian Species" (*ibid.* Oct. 1917); "Notes on Fossorial Hymenoptera: XXXI. On Psammocharidæ in the British Museum" (*ibid.* Nov. 1917), and "Notes on Fossorial Hymenoptera: XXX. On new Ethiopian Scoliidae" (*ibid.*), all by Turner; "Descriptions and Records of Bees, LXXVII" (*ibid.* Oct. 1917), and "Descriptions and Records of Bees, LXXVIII" (*ibid.* Dec. 1917).

Papers on Echinoderms include: "British Fossil Crinoids: XI. *Balanocrinus* of the London Clay," by Bather (*Ann. and Mag. Nat. Hist.* Dec. 1917); "New Genera and Species of *Brisingidæ*," by Fisher (*ibid.*); and "The Early Development of a Starfish, *Pateria* (*Asterina*) *nuneata*," by Heath (*Journ. of Morph.* vol. xxix. Sept. 1917).

In a paper "The Cytoplasmic Inclusions of the Germ-Cells: II. *Helix aspersa*," by J. Brontë Gatenby (*Quart. Journ. Micro. Sci.* vol. lxii. Dec. 1917), this author continues his observations on those constituents of the cytoplasm of the germ-cell that are usually overlooked when ordinary fixatives are employed, but which nevertheless from their behaviour appear to be structures of considerable importance. Two different sized bodies are to be found in the cytoplasm, the larger macromitochondria and the smaller micromitochondria, the latter appearing in the early spermatid and form the front sheath of the sperm. The behaviour of the "Nebenkern" is described and its probable function discussed. Not merely are these bodies described but details are added to our knowledge of the derivation of the sperms, eggs, and nurse cells. From apparently indifferent cells in the hermaphrodite gland of the snail, either sperms or ova can be produced, but the determining factor is by no means clear—it does not appear to be the proximity of yolk cells as has been suggested by certain authors.

Vertebrata.—"Proboscis Pores in Craniate Vertebrates, a Suggestion concerning the Premandibular Somites and Hypophysis," by Goodrich (*Quart. Journ. Micro. Sci.* vol. lxii. 1917)

is the title of a suggestive paper which deals not only with these structures in Craniate Vertebrates, but also with Hatschek's pit in Amphioxus, of the structure and development of which a detailed account is given. It seeks to homologise these with the "proboscis pore" of Balanoglossus and the water pore of the Echinoderm, all being of the nature of cœlomostomes. The homology of the premandibular somites of the Craniates with the anterior cœlomic sacs of Amphioxus is pointed out. It is concluded that the hypophysis of the Craniata is represented in Amphioxus by the wheel organ situated in front of the true mouth, and that its original function was probably to drive food into the alimentary canal.

Watson has dealt with "The Evolution of the Tetrapod Shoulder Girdle and Limb (*Journ. of Anat.* vol. lii. Oct. 1917). Any one who has had to deal with the shoulder girdle and limb of the higher vertebrates is immediately confronted with a great deal of confusion and inconsistency. This paper is welcome, since it attempts to give a satisfactory account, from the evolutionary point of view, of the changes undergone by that region from the lowest to the highest forms. The matter is dealt with under two headings; the first treating of the actual facts and their interpretation and the second of the functional significance of the changes described. A large series of forms both recent and fossil are dealt with, including a wax-plate model reconstructed from a series of sections of an 11·5 embryo of the marsupial *Trichosurus.* The way in which the girdle and limb of such a form as *Eryops* can be derived from those of a fish is indicated. The coracoid element of the Lizards, Sphenodon, Chelonia, and Birds is homologised with the precoracoid, as is also the epicoracoid of Monotremes. The omosternum of the Theria is considered the equivalent of the old reptilian interclavicle. In early forms, *e.g.* Pelycosaurs, the humerus is only able to move parallel with the ground with a backwards and forwards motion being at right angles to the body in its middle position. A steady change from this condition to that in Cynognathus, where the humerus "moves in a vertical plane at an angle of about 45° with the animal's length," is demonstrable.

Other papers include: "Descriptions of new Fishes from Lake Tanganyika forming part of the Collection made by the late Dr. L. Stappers for the Belgian Government," by Boulenger

(*Ann. and Mag. Hist. Hist.* Nov. 1917); "A Hermaphrodite Dogfish," by Bamber (*Proc. Zool. Soc.* Nov. 1917); "On the Herring in Captivity and on an Effect of the Environment on the Structure of this Fish," by Williamson (*Journ. Zool. Research*, Nov. 1917); "Sinneslinen und freie Nervenhügel bei *Chimæra monstrosa*," by Ruud (*Zoolog. Jahrb.* 1917); "Descriptions of new Frogs of the Genus *Rana*," by Boulenger (*Ann. and Mag. Nat. Hist.* Dec. 1917); and "On the Skull of *Tritilodon longævus*, Owen," by Petronievics (*ibid.* Oct. 1917).

Two papers deal with the primordial cranium, viz. "The Primordial Cranium of *Microtus amphibius* (Water-Rat) as Determined by Sections and a Model of the 25 mm. stage, with Comparative Remarks," by Fawcett (*Journ. of Anat.* July 1917), and "The Primordial Cranium of the Cat" by Terry (*Journ. of Morph.* vol. xxix. Sept. 1917). It is not possible to summarise these papers as their value lies in the accuracy and minuteness with which the primordial cranium is described. The excellent illustration of the model renders it an easy matter to follow the description. Although still in an early stage of development, a fair amount of ossification had already set in in some of the cartilages. The work of the second is in the main based upon the wax-plate model of the chondrocranium of an embryo of 23·1 mm., but a number of other stages have been utilised. The two researches add considerably to our knowledge of the mammalian cranium.

In "The Cytomorphosis of the Marsupial Enamel Organ and its Significance in Relation to the Structure of the Completed Enamel," by Carter (*Phil. Trans. Roy. Soc.* B, vol. ccviii. Dec. 1917), we have for the first time the description of the enamel organ and completed enamel studied by satisfactory modern histological methods upon well-fixed material. Only too often in odontological work does the fixation and technique leave much to be desired. Marsupial teeth differ from those of most mammals in the presence in the enamel of structures, presumably organic, passing sinuously outwards from the dentinal tubules with which they are continuous. These "fibrils" arise from "the coagulation or gel-formation of an organic substance not usually present in colloidal secretion shed by the enamel cells." Their development is associated with distinct changes in the ameloblasts which elongate and

become partly transformed into metaplasm, and they seem to lie within the substance of the rods and in the interprismatic tissue. The enamel calcifies by the adsorption of calcium salts into a honeycomb organic gel-formation.

Other papers include: "Notes on Agoutis, with Descriptions of New Forms," "A new *Heliophobius* from North-eastern Rhodesia," and "The Spalax of the Grecian Archipelago" (*Ann. and Mag. Nat. Hist.* Oct. 1917); "A new Rat of the Genus *Mylomys* from the Upper Nile" (*ibid.* Nov. 1917) and "Notes on *Georhynchus* and its Allies" (*ibid.* Dec. 1917), all by Thomas; "On a new Species of Shrew from Corea," by Sowerby (*ibid.* Oct. 1917); "The Classification of existing *Felidæ*," by Pocock (*ibid.* Nov. 1917); "A Case of Accessory Lungs associated with Hernia through a Congenital Defect of the Diaphragm," by Gladstone (*Journ. of Anat.* vol. lii. Oct. 1917); "Form and Function of Teeth: A Theory of Maximum Shear," by Shaw (*ibid.*); "The Earliest Stages of Development of the Blood-Vessels and of the Heart in Ferret Embryos," by Wang (*ibid.*); "Inheritance of Fertility in Southdown Sheep," by Wentworth (*Amer. Naturalist,* Nov. 1917); "Evidence for the Death in Utero of the Homozygous Yellow Mouse," by Ibsen and Steigleder (*ibid.* Dec. 1917); "Deformity of os penis in a *Phoca caspica* Nilsson," by Alphéraky (*Proc. Zool. Soc.* Nov. 1917), and "The Structure of the Orbito-temporal Region of the Skull of *Lemur,*" by Wood Jones (*ibid*).

General.—An address given by Gates at the Pacific Coast meeting of the American Association for the Advancement of Science is published under the title "The Mutation Theory and the Species Concept" (*Amer. Naturalist,* Oct. 1917). Its general conclusions are summarised by the author in the following words: "I have endeavoured to show that in plant and animal species there are two distinct types of variability, having different geographical relations. The one is discontinuous, independent of environmental or functional influence, and has given rise to many specific and generic characters notably in plants, but also in higher animals. The other is continuous and apparently represents the results of the stress of the environment on the species in its dispersal, leading to the gradual differentiation of local races or sub-species whose peculiarities are ultimately intensified and fixed. The latter type of speciation is notably exemplified in birds and mammals, organisms

in which, unlike plants, the individuals can migrate from place to place and so substitute for a stress resulting from over-population an environmental stress caused by a new set of climatic or physiographic conditions."

Four papers deal with the nuclear apparatus from more or less general points of view. "The Period of Synapsis in the Egg of the White Rat, *Mus norvegicus albinus*," by Pratt and Long; "The Synapsis and Chromosome Organisation in *Chorthippus* (*Stenobothrus*) *curtipennis* and *Trimerotrops suffusa* (Orthoptera)," by Wenrich; "The Multiple Chromosomes of *Hesperotettix* and *Mermiria* (Orthoptera)," by McClung, and "Multiple Complexes in the Alimentary Canal of *Culex pipiens*," by Holt (all in *Journ. of Morph.* vol. xxix. Sept. 1917). The first two add to the already considerable literature on Synapsis. The first concerns the phenomenon in a mammal on which group but little has been previously done, but with this these results are in agreement. The second, an extension of the author's previous work, is also corroborative of the main points already known, but some further points of detail are elucidated. The third, too, is an extension of the writer's previous work on an unusual condition in the chromosome complex in orthopteran spermatocytes. It includes the study of a large series of specimens of different species and reveals a close relationship between cytological characters and taxonomic grouping. The last records that a considerable increase in the number of chromosomes in the cells of the pupal intestine takes place during metamorphosis. The process, moreover, is not accidental since it occurs in all cells before they disintegrate, and can hardly be regarded as a degeneration phenomenon owing to its uniformity and regular occurrence. Its meaning is not obvious.

Other papers include: "Zellen und Plasmodien: Eine kritische Studie," by Schaxel (*Zool. Jahrb.* 1917); "Further Observations on the Effects of Alcohol on White Mice," by Nice (*Amer. Naturalist*, Oct. 1917); "Genetics versus Palæontology," by Gregory (*ibid.*); "Linked Quantitative Characters in Wheat Crosses," by Freeman (*ibid.* Nov. 1917); "On Reversible Transformability of Allelomorphs," by Teras (*ibid.*); and "Some Observations upon Concealment by the Apparent Disruption of a Surface in a Plane at Right Angles to the Surface," by Mottram (*Proc. Zool. Soc.* Nov. 1917).

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc.

THE *Journal of the Royal Anthropological Institute* is once more the periodical which calls for the first notice in these notes. The publication for the second half of 1917 (vol. xlvii. pt. 2) contains more than one contribution which should be of interest to the ordinary educated reader as well as to the professed anthropologist. Mr. J. Reid Moir, who is now so well known as a pertinacious advocate of extreme views on the antiquity both of the surviving species of man and of the human tribe as a whole, contributes an article entitled " On some Human and Animal Bones, Flint Implements, etc., discovered in two occupation-levels in a small valley near Ipswich," which should cause widespread interest. Mr. Moir is one of those who believe that there exist definite proofs that *Homo sapiens* was living in Europe prior to the Aurignacian Age (the Aurignacian being the oldest period in which it is universally agreed that *sapiens* was inhabiting this continent), and a special section of the evidence upon which he relies for the support of this thesis is admirably described in this article.

The excavations described were carried out in the brickfield of Messrs. A. Bolton & Co., Ltd., Henley Road, Ipswich, and the research extended over more than two years, from April 1914 to May 1916. The small valley in which the brickfield is situated is now streamless, but is a tributary valley of the River Gipping. Mr. Moir thinks that the little valley must have been excavated by the melting of snow and ice which accumulated on the small plateau at the head of the valley in days when the climate was colder than it now is. " But whatever the cause, the valley, as now developed, has cut down through the Chalky Boulder Clay, the Glacial Gravel, the Red Crag, the London Clay, and into the underlying Woolwich and Reading beds." Two occupation-levels " cut the surface some feet (at about 80 O.D.) above the present valley-bottom."

It is to be presumed that the floors on the two sides of the valley were once continuous, each to each, and that subsequent erosion has removed the intermediate portions of the floors. Mr. Moir believes that the lower floor is Upper Mousterian and that the higher occupation-level is Aurignacian. A typical series of the flint implements was sent to Prof. V. Commont of Amiens, and this scholar gave it as his opinion that the lower floor was indeed Upper Mousterian, and that the upper floor

was Aurignacian, and he was thus in agreement with Mr. Moir and Mr. Reginald Smith. Several large fragments of primitive pottery were found in the lower floor, and these would, of course, constitute a most remarkable discovery if the supposed Mousterian date of the floor could be placed beyond doubt. The associated mammalian remains cannot be said, however, to lend any very decisive support to the theory of a Pleistocene date.

The bones found belong to *Cervus elephas*, *C. capreolus*, *C. megaceros* (probably), *Bos longifrons*, *Sus scrofa*, *Capra hircus*, several varieties of horse, and to a species of elephant. The Elephantidæ are represented only by a single specimen, the part of a long bone, probably a tibia. The general appearance of the fauna is thus essentially Holocene, but the elephantine bone (which is presumably that of a mammoth) is, of course, a contradiction. One would be glad to know any reasons which may exist for doubting that this apparently anachronistic tibia is of the nature of a derived fossil. The plants found in the associated peat are also of modern types. A few human remains (a fragment of a skull, another of a humerus, and another of a femur) were found in the lower floor, and these are declared by Prof. Keith, and less decisively by Dr. Duckworth, to belong to *H. sapiens*. In this respect also, therefore, it will be seen that the discovery, if it be correct to assign it to the Mousterian, is of the first importance. In the case of the famous Grime's Graves, excavated by Mr. Moir's friends of the Prehistoric Society of East Anglia, there was a keen controversy as to whether the date was Neolithic or Pleistocene. In that instance I thought that the evidence of the mammifauna was convincingly in favour of a Neolithic date. The students of flints have no doubt over-reached themselves, but it must be admitted that in this case the mammalian remains are not equally decisively against the Pleistocene dating.

Another interesting contribution to the same number of the *Journal* is: " Some South Slav Customs as shown in Serbian Ballads and by Serbian Authors," by Edith Durham. The article includes translations of certain tales by one Vuk Vrchevitch who lived in the first half of last century, and Miss Durham adds to the interest of these stories by giving the comments made thereon by primitive Montenegrins among whom she lived. The article is entirely untechnical and gives an

admirably clear insight into South Slav character and manners. Other articles in the *Journal* are : " The People and Language of Lifu, Loyalty Islands," by Sidney H. Ray ; " The Menhirs of Madagascar," by A. L. Lewis ; " Arab and Swahili Dances and Ceremonies," by R. Skene ; and " Studies in Primitive Looms " (pt. 3), by H. Ling Roth.

The *Proceedings of the Prehistoric Society of East Anglia* for 1916–17 (being vol. ii. pt. 3) have now been published. The article of most general interest is the first, which is by W. G. Clarke (the energetic Honorary Secretary of the Society), and is entitled " Are Grime's Graves Neolithic ? " Mr. Clarke comes to the conclusion that this question should be answered in the affirmative. Among the other articles, the following may be mentioned : " Chipped Flints from below the Boulder Clay at Hertford," by H. G. O. Kendall ; " A Cissbury-Type Station at Great Melton," by W. G. Clarke and H. H. Halls ; " Plateau Deposits and Implements," by Reginald Smith ; " The Position of Prehistoric Research in England," by J. Reid Moir ; and " Further Excavations at Grime's Graves," this being the Presidential Address by A. E. Peake. The latter writer would assign a Moustier-Aurignac date to the " Graves."

In the recent numbers of *Man*, the only article which would appear to call for special mention is one (in March) entitled : " The People and Language between the Fly and Strickland Rivers, Papua," by the Hon. J. P. W. Murray (Lieutenant-Governor of Papua), which is communicated by S. H. Ray. This is an important contribution to the ethnography of the great island.

ARTICLES

SOME ASPECTS OF ANIMAL COLOURATION FROM THE POINT OF VIEW OF COLOUR VISION

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PART I

As far as we are aware, animal colouration has not been considered from the point of view of colour vision: and as the knowledge of colour vision has been greatly advanced (Edridge Green, Science Progress, Jan. 1915) during recent years it would seem that such a consideration is now called for. No attempt is here made to deal exhaustively with the subject, but only to discuss a few of the more important facts, to correct certain interpretations, and to give to others an importance which has been overlooked.

The dorsal surfaces of the wings of diurnal Lepidoptera is the material first chosen for analysis because they present great diversity of colouration, and unlike the ventral surfaces, seldom directly imitate natural surroundings. The Indian Lepidoptera (Moore's *Lepidoptera Indica*) were selected on account of their careful description and illustration in colour (the Hesperidæ were omitted). It was found that the colours can, with hardly any difficulty, be classified into red, orange, yellow, green, blue, violet, black, brown, and white. Other colours, such as purple and grey, so rarely occur that a very much larger series would require to be investigated, in order to obtain information of value about them. Certain difficulties arise when such colours as orange-red, yellow-orange, and yellow-brown have to be classified, but in the majority of cases, description in the text decides the case: a second opinion was also often

requisitioned. Each colour has been further classified according as to whether any single mass of it occupied a large area (more than two-thirds of either wing); a moderate area (more than one-third of either wing); or a small area (less than one-third of either wing). Omitting those species which mimic or which present sexual colour differences, the distribution of colour shown in diagram No. 1 results. It can be seen that brown is found more often than any other colour, followed in order by black, white, yellow, orange, red, blue and violet, and lastly, green. Blue and violet are not considered separately.

As green foliage so commonly forms a background in Nature, it is remarkable that butterflies do not more often present a

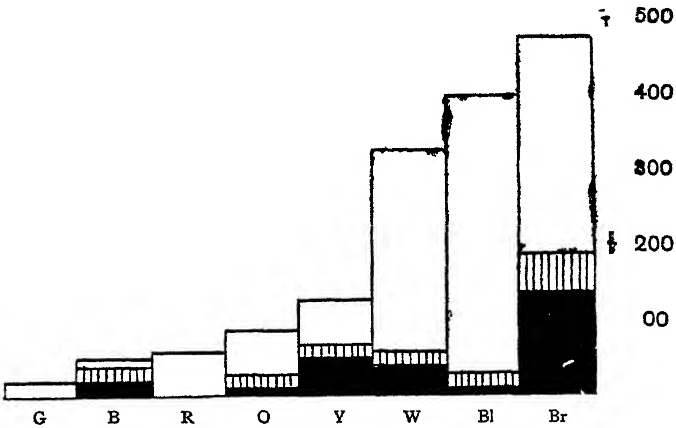

Diagram No. 1.

G., green; B., blue; R., red; O., orange; Y., yellow; W., white, Bl., black; Br., brown. Black areas represent large areas of colour. Ruled areas represent moderate areas of colour. White areas represent small areas of colour.

green colouration, seeing that their patterns and colouration in the vast majority of cases render them inconspicuous. Had the colours of lepidopterous larvæ been investigated, a much greater use of green would be found, as well as a dimorphism which may explain the rarity of green butterflies. It is not uncommon for some individuals to be coloured brown, whilst others of the same species are green; and this dimorphism occurs not infrequently in other insect groups. In many cases this is not associated with either environmental differences or differences of habit. If, however, cognisance be taken of the evolution of colour vision, a ready explanation of this irregularity is available. Red and violet were the colours to become

differentiated first, followed by green in the mid-spectral region. It is probable that many animals have not developed beyond this trichromic vision, and to such a colour perception brown and green are indistinguishable and would not therefore be a factor for selection ; thus it would be immaterial to a species, in respect to the attack of enemies, whether it be coloured green or brown against either green or brown backgrounds : there is

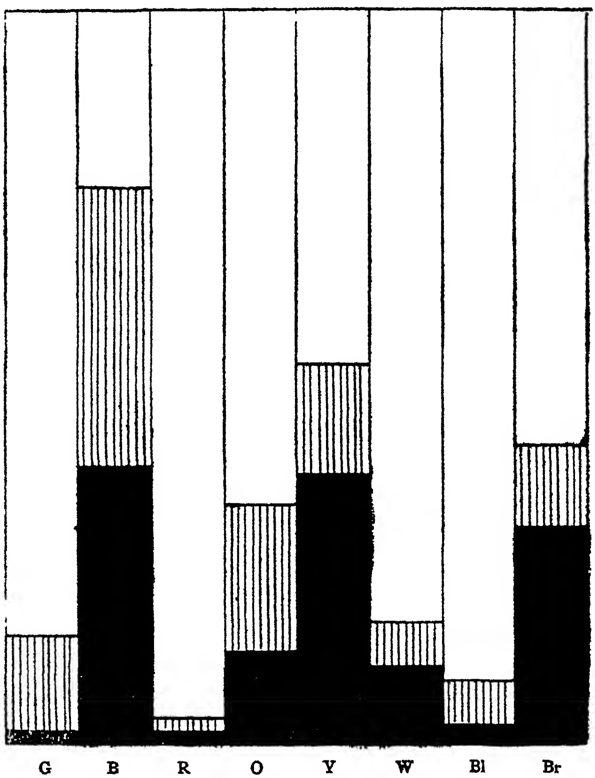

Diagram No. 2.

In this diagram the occurrences are all brought to 100.

therefore nothing to hinder the green-brown dimorphism and no reason why a butterfly should be coloured green rather than brown to imitate foliage.

The green of many larvæ is derived from the chlorophyl of their food, but in butterflies such a source is not available. It would seem that the metabolism to form adult characters is more easily able to produce brown.

The Consideration of Brown

Referring to diagram No. 1, it is seen that the colours are for the most part laid down in small areas; only in the case of blue, yellow, and brown do large and moderate areas commonly occur. This is better seen in diagram No. 2, where all the colours have been reduced to 100. Large brown occurs in 30 per cent. Details of this distribution are more fully given in the diagrams which deal with five groups given separately—the Euploeinæ (diagram No. 3), the Satyrinæ (diagram

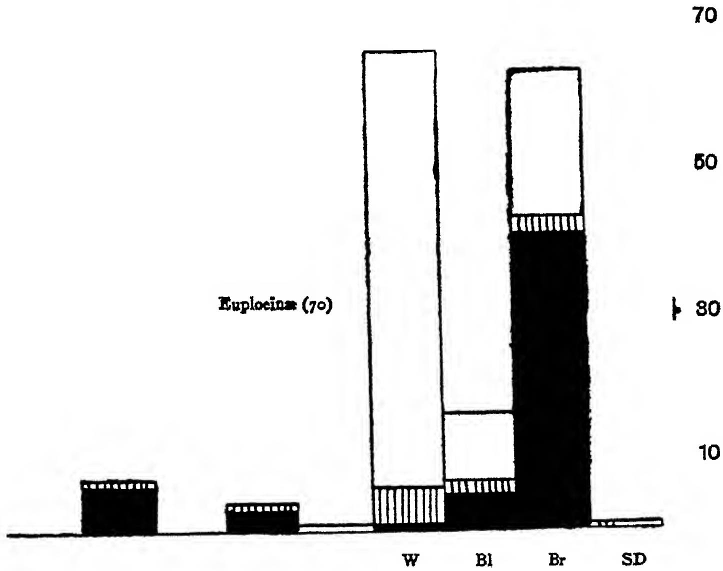

Diagram No. 3.

SD. represents species in which there is sexual dimorphism in colour. The numbers after the group names give the number of species dealt with.

No. 4), the Nymphalinæ (diagram No. 5), the Papilionidæ (diagram No. 6), and the Pierinæ, Eroninæ, and Coliinæ (diagram No. 7). It can be seen that large brown is chiefly to be found in the Satyrinæ and Euploeinæ. The arrangement of these large areas is entirely different in the two groups: in the Satyrinæ it occupies the centre of the insects' wings and is bounded by marginal pattern, whereas in the Euploeinæ the whole wing is coloured an even dark brown, or if a pattern is present it is situated at some distance from the margin. In previous papers (*Proc. Zoo. Soc.* 1915, p. 679; 1916, p. 283) it has been shown

that marginal patterns render an insect relatively invisible and that the absence of such pattern makes for conspicuousness, which may account for large brown areas being found in the relatively unpalatable Euploeinæ as well as in the palatable

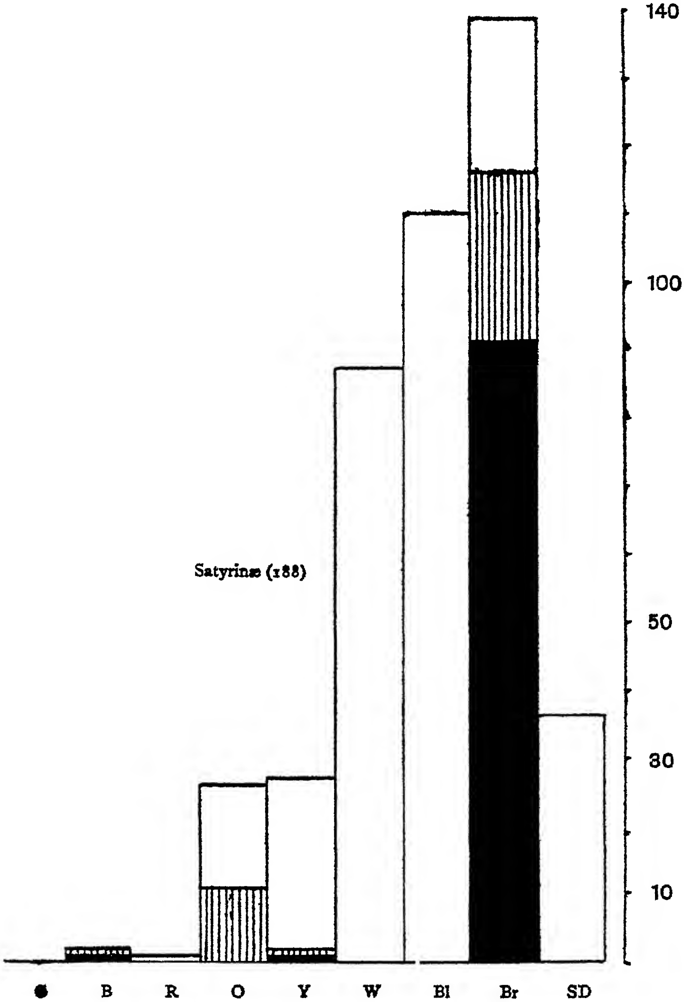

Diagram No. 4.

Satyrinæ. There is a further reason why this colouration in the Euploeinæ makes for conspicuousness: in many cases the wing is outlined by a band of black which has been shown experimentally (*loc. cit.*) to accentuate the insect's outline and

to be a very conspicuous arrangement: from the point of view of colour vision also, this results in great conspicuousness, as will be shown later.

Brown occupying moderate or small areas forms marginal

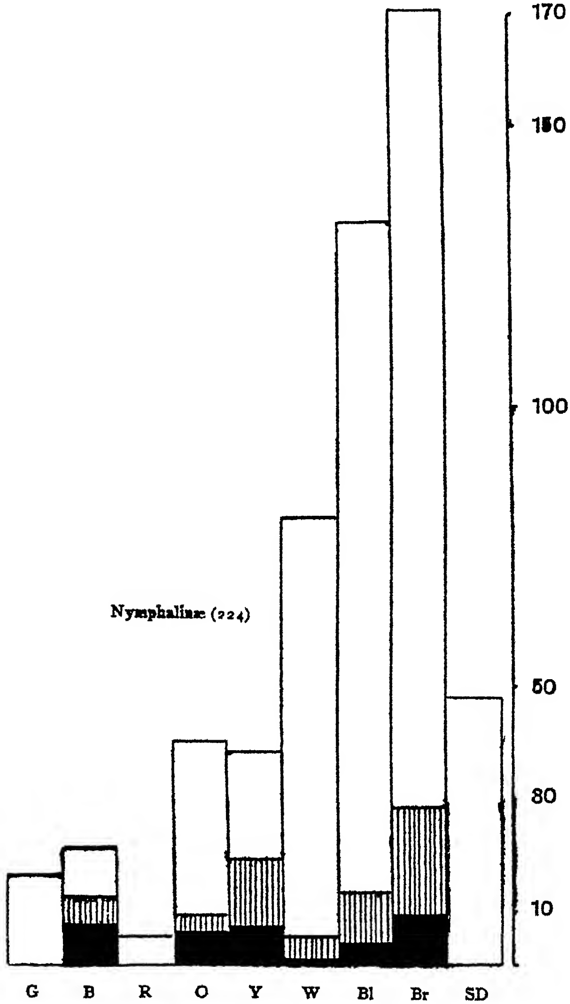

Diagram No. 5.

and other patterns which render the insects inconspicuous against their surroundings: these will be referred to when dealing with the other colours, as they may be considered to form a groundwork upon which the other colours are laid.

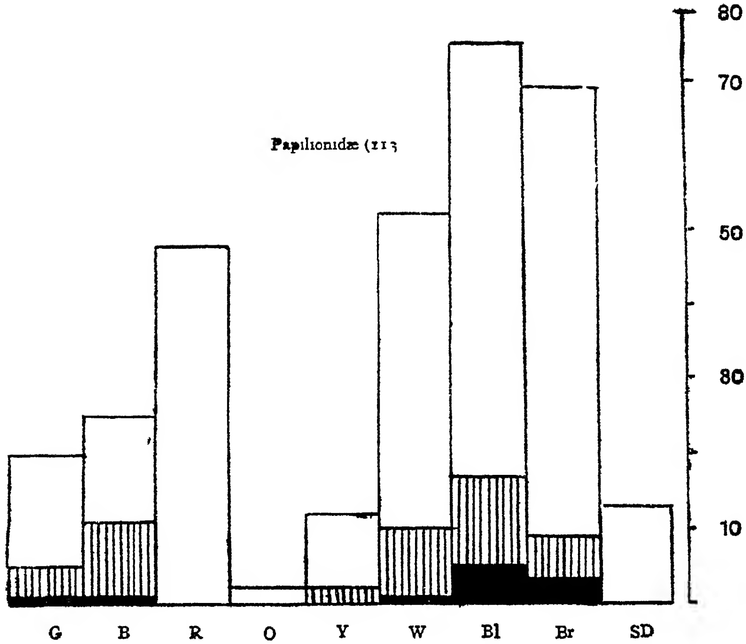

Diagram No. 6.

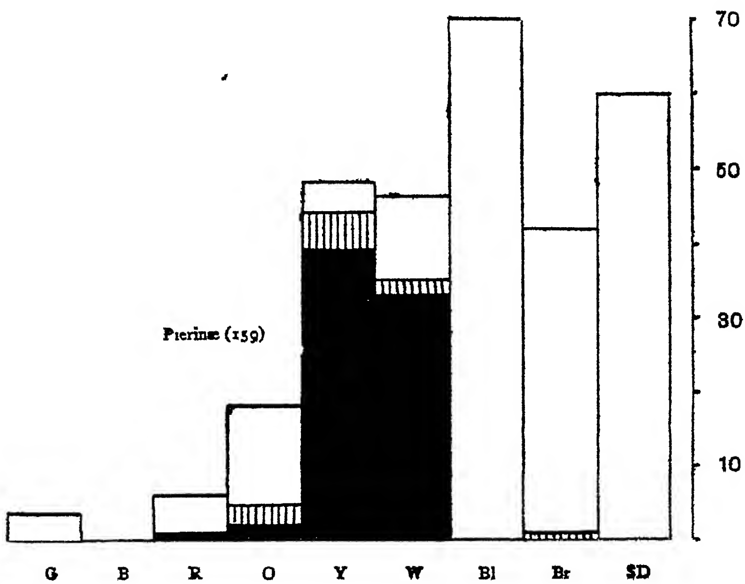

Diagram No. 7.

The Consideration of Black

Referring to diagrams Nos. 1 and 2, it is seen that black is, after brown, the most frequently found and that it is very largely laid down in small areas (91 per cent.). In the majority of cases it is associated with small areas of white, in the production of eye-spots and marginal patterns. It is also largely used in the form of spots upon yellow or orange insects as in the Argynninæ. It has been previously shown (*loc. cit.*) that small black used in this way must be considered an inconspicuous arrangement.

Small areas of black are used in conjunction with large areas of yellow, orange, brown, white, and blue to render conspicuousness in the form of a border which protects the colour from admixture with the insect's background, as fully explained on p. 74. Used in this way, the conspicuousness may be said to depend upon two distinct factors: (1) the production of a pattern of two components, (*a*) the central light in tone, (*b*) the outer dark, which has been shown in a previous paper (*P.Z.S.* 1916, p. 402) to be the most conspicuous arrangement of tones which an object can present; and (2) the protection of a conspicuous central colouration from admixture with the colours of the insect's background (see p. 74).

Moderate areas of black are most often found as a wide marginal band similar to the arrangement just described, as in the Nymphalinæ, genus Haridra.

TABLE NO. 1

Species.	Extent of Black Colouration.
Papilioninæ.	
1. Ornithoptera ceberus	whole of forewing.
2. O. heliaconoides	whole of forewing.
3. Sainia protenor	whole of hindwing, except small red spot.
4. Laertes pammon (normal form)	whole of forewing and half of hind.
5. Byasa bootes	whole of forewing.
Nymphalinæ.	
6. Prothoë regalis	whole of hindwing and half of fore.
7. Kaniska canace	both wings, except for blue bar across both wings.
8. Euvanessa antiopa	whole of centre of both wings outlined with band of buff.

Species.	Extent of Black Colouration.
EUPLOEINÆ.	
9. Nemana simulatrix	whole of both wings.
10. Tronga nicévillei	centre of forewing, with margin of white spots.
11. Penoa alcathoe	whole of forewing.
12. „ menetriesii	whole of forewing.
13. Narmada montana	whole of forewing, except for a few very small white spots.

Large areas of black appear to be in almost every case laid down in a manner which results in a conspicuous appearance. Those examples found in the groups considered separately (diagrams Nos. 3 to 7) are shown in table No. 1; with the exception of No. 10, there is a complete absence of an associated marginal pattern; either the whole wing is coloured black, or a pattern is present as in No. 7, which cannot result in a conspicuous appearance. No pattern or colouration is thus found associated with large areas of black which will in any way counteract the conspicuous appearance which large areas of black have against natural backgrounds.

The Consideration of White

White is the third most commonly used colour; it is found in small areas in 83 per cent. of the cases and almost entirely in the form of marginal patterns, ocelli, and other small markings associated with small areas of black. An advantage in concealment, of the use of white in this way, depends upon the following facts:

If a red object be viewed against a yellow background, it is seen that the red near the yellow becomes yellowish, and the yellow near the red reddish. This diffusion of colour occurs with all colours; it is, however, best seen when white is used, because it changes from being colourless to becoming coloured. If a narrow white line be drawn across red and blue backgrounds, the white across the red will appear pale red, and across the blue, pale blue. It follows that an insect presenting a marginal pattern of black and white will at a distance be seen to have a grey border fading on the one side into the colour of the background and on the other into the colour of the rest of the insect's wing. Not only in colour will this graduation occur, but also in tone, as shown in previous papers (*loc. cit.*).

In contrast to this use of white is the effect of a black marginal band outlining a coloured object. A white line crossing red and blue backgrounds enclosed on either side with black lines will not appear pale red or blue but will maintain its original whiteness. Similarly the white, yellow, red or other colour of an insect which is outlined with black will not be affected by any background against which it may be viewed. For this reason a black marginal band is a great aid to conspicuousness, as it isolates the colour of the insect from that of its background.

White is also sometimes used as a groundwork on which darker patterns of spots and stripes are laid, as in the Parnassiinæ and in some of the Papilioninæ. On referring to the Euploeinæ, diagram No. 3, it is seen that small white is largely used in this group, but its distribution is unusual: instead of forming a marginal pattern, it is often scattered through the wing in the form of small spots which do not approach the margin. This arrangement will be less conspicuous than a plain wing but more conspicuous than one in which the pattern is near the margin. As to whether this moderately conspicuous pattern is related to the visual perception of their enemies, cannot here be discussed. In a few cases white is used in moderate amount, chiefly in the form of bars crossing one or both wings; this condition has not been subjected to experimental analysis. Thayer looks upon such marks as concealing by disrupting the insect's outline and form (Thayer, *Concealing Colouration in the Animal Kingdom*, p. 77).

Large white forms 12 per cent. of the cases in which white is used. Referring to the diagrams it is seen that the vast majority of these are found in the Pierine group, where thirty-three examples occur. In these insects, the whole wing may be white, but more often it is outlined by a black marginal band, an arrangement which must render the insect very conspicuous for several reasons: (1) because a light-centred, dark-margined eye-spot pattern results (see p. 72); (2) because of a great tone and colour contrast between the natural background and the white of the wing's centre which is protected from diffusion by the black margin; and (3) because of the great luminosity of the colour white. The factor luminosity is dealt with on pp. 75 and 76.

The Consideration of Yellow

Between the number of times that black, white, and brown are used and the number of times the other five colours are found in these insects' wings, there is a wide difference. The five colours green, blue, orange, yellow, and red added together little more than equal white, which is the least of the first three (see diagram No. 1). Of the second group, yellow is the most common; it is found in large areas nearly as often as in small (see diagram No. 2). On referring to diagram No. 7 dealing with the Pierines and Coliines, it is at once apparent that this group of insects accounts for most of the large areas of yellow. The colour is here used in a conspicuous manner, it is centrally placed, and outlined by a band of black just as are large areas of white in the same group of insects. Whether in this case the yellow is conspicuous to the insect's enemies by reason of its colour, will depend upon their colour vision; however, even if they were colour blind to yellow, nevertheless yellow used in this way must be considered to be conspicuous by reason of its high luminosity. It is noteworthy that in this group the yellow is, as a rule, high in tone.

Yellow is occasionally used as brown is, in the Satyrinæ namely, as a central large area bounded by a marginal pattern. In these cases, the yellow is not high-toned nor of great purity as in the Pierines, but low-toned and muddy. The distribution of small areas of yellow is similar to that of white, but rarely occurs with black to form marginal patterns: they are chiefly found as yellow spots on a dark background, or as a ground on which black or other spots are distributed: such patterns will blend at a distance to form a brown tone with indistinct margins. Thus it is seen that yellow is chiefly used to produce inconspicuousness, and even in the Pierine group it is doubtful whether colour is a factor in the conspicuous arrangement. It has already been mentioned that green was the third colour to become differentiated visually. There is considerable evidence that the other colours were evolved in the following order: yellow, blue, orange, and lastly indigo, which is only seen as a distinct colour by very few people. This has an important bearing on the use of yellow in animal colouration, for even if the majority of animals are not at present colour-blind for yellow, at any rate this colour can have only recently become

a factor of selective value. When, therefore, we find an animal coloured yellow, it may well be that it appears inconspicuous to its enemies who cannot distinguish yellow from green, though to our eyes it may seem conspicuous. There is ample evidence, especially amongst birds, that yellow is not a conspicuous colour, though by reason of its high luminosity it is sometimes used to produce conspicuousness, and in the vast majority of these cases it is sharply cut off from the rest of the animal's colouration by bands or large areas of black.

The Consideration of Orange

Orange comes next to yellow in frequency of occurrence. It differs from yellow in that it is much less often found laid down in large areas, and this is due to the fact that orange is not found giving rise to conspicuousness in the Pierine as is yellow (see diagram No. 7). The fact that the orange is not used in this manner lends support to the conclusion that large yellow in the Pierines is conspicuous by reason of its high luminosity, rather than by its colour. Large orange is occasionally used in a conspicuous manner in the Euploeinæ with a marginal band of black, and then is very light in tone and approaches towards red rather than yellow.

Large orange is chiefly found in the Argynninæ occupying the centre of the wings and surrounded by a pattern of black spots. In the Satyrinæ, centrally placed orange is also found, but so encroached upon by marginal pattern that only moderate areas are found (see diagram No. 4). Just as yellow with black appears brown at a distance, so orange and black will become copper-brown, simulating the colour of dying vegetation and giving rise to an inconspicuous effect. Orange and yellow are laid down in moderate areas chiefly in the forms of bars crossing the wings, as in Kallima, or in apical blotches as in some of the Pierines. The significance of these arrangements of colour is not clear; in Kallima the orange bar is in juxtaposition to an area of blue, its complemental colour, and this will again be referred to in Part II.

Small orange is found in the vast majority of cases, as a ground on which a pattern, usually of black spots or bars, is laid, as in the Argynninæ, evidently for purposes of concealment. Like yellow, the rarity with which orange is used con-

spicuously may possibly be accounted for by its late appearance in the evolution of colour vision.

The Consideration of Red

Red is the next colour in order of frequency and it occurs almost entirely in small areas (96 per cent.) (see diagram No. 2). In the groups dealt with in detail it only occurs once as a large area, in *Tachyris galba*; in this insect both wings are coloured almost entirely red, not a very pure red, but one bordering upon orange. In the text, mention is made that this rare insect is by no means conspicuous in nature and matches in colour the local fallen leaves (Moore, *Lepidoptera Indica,* vol. viii. p. 16).

Small red is rarely found except in the Papilionidæ, where it occurs eight times in the Parnassiinæ, once in the Thaidinæ, and thirty-nine times in the Papilioninæ. In the Parnassiinæ it is the form of eye-spots; in the Papilioninæ as semi-lunar spots or black-centre eye-spots upon the hindwing, sometimes also there is a small spot of red on the forewing close to the insect's body. It is noteworthy that these red marks occur mostly in positions which have been shown to be of little value as sights for pattern in the production of inconspicuousness, namely towards the centre of the wings as in the Parnassiinæ, and along the inner margin of the hindwing or close to the insect's body as in such species as *Sainai protenor, Iliades agenor* (several forms), *I. polymnestoroides, Charus daksha,* and *Achillides tamilana.*

Apart from the question of position, there are other reasons why red, even in small areas, must be considered to be a conspicuous colouration. Firstly because red arose early in the evolution of colour vision, secondly because it is almost never to be seen in natural backgrounds, and lastly because it has great power of penetration; it is less dulled at distance by the opacity of the atmosphere than other colours. On the other hand, a combination of small areas of red and green would give rise to an inconspicuous appearance, as will be explained in Part II. There is a further condition in which red appears inconspicuous, namely, at very low illuminations: this depends on the Purkinje effect, wherein the maximum visual perception, which at high illuminations is at orange-yellow, at low illuminations is shifted towards the blue. For this reason, red colouration in nocturnal animals will be inconspicuous. Although the Purkinje effect

does not appear to enter into the consideration of diurnal Lepidoptera, nevertheless in view of the great conspicuousness of red in very high illumination on account of its great penetrating power, it is necessary to inquire whether insects which present this colouration are shade or sun-loving : more especially because it is considered that to appear conspicuous at great distance would be of no advantage to any animal : all the requirements of warning or distinctive colouration (Swynnerton, *Ibis*, 1916, vol. iv. p. 265) are fulfilled at comparatively short distance. It follows that red should be used for this purpose in forest and shade-loving insects, and as will appear later, blue in those frequenting open and sunny localities. It has been mentioned that a red and green pattern will be inconspicuous beyond the blending distance ; but close at hand red against green will be very conspicuous : thus such a colouration would fulfil the requirements for " warning " or " distinction." In Papilio, red occurs associated with blue and yellow, and in Sabaria, Pangeranopsis, and Achillides associated with green.

To return to the question of illumination, the majority of the Parnassiinæ live upon open grassy mountain tops, close to the snow line, where the lighting is likely to be poor, though this is by no means certain; much will depend upon the prevalence of mist. Out of the thirty-nine Papilioninæ presenting small red areas, ten are mentioned in the text as inhabiting forests and jungle, four living in the open, and in the remainder the habitat is not given. The following are the four species which are mentioned as inhabiting open situations; it is noteworthy that they present far less red than is usually the case:

TABLE NO. 2

Sainia protenor .	a single eye-spot close to the posterior angle of the hindwing.
Orpheidea demoleus	a single spot close to the posterior angle of the hindwing.
Papilio asiatica .	a single spot close to the posterior angle of the hindwing.
Achillides paris .	a single eye-spot close to the posterior angle of the hindwing.

There is other evidence that these insects are conspicuous: (1) they are not baskers, as many Lepidoptera, but prefer to settle in shady places ; (2) after settling they often depress the forewings so as to cover the red-spotted hindwings; and (3) lastly, they belong to a group which is considered to be relatively unpalatable.

(*To be continued*)

THE FOOD REQUIREMENTS OF A "NORMAL"[1] WORKING-CLASS FAMILY

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It is a matter of importance to determine as accurately as possible the quantity of food required to maintain an average working-class family in a state of health and efficiency. This knowledge supplies a constant factor essential to every consideration of the cost of living, and indirectly of production. To be at all complete it should be supplemented by an estimate of the outlay in money necessary to provide the food. This latter is, however, a factor which varies both with time and place. Unlike the quantity of food, it has no fixed or permanent value, and has to be determined for each locality and period. No useful purpose would be served by supplying a statement of cost in the present instance.

The following attempt to arrive at a safe estimate of quantities was made in response to inquiries from more than one quarter. The family is supposed to consist of father, mother, and four children of the respective ages of 13, 11, 9 and 7 years.

Before proceeding to consider the requirements in actual food it is necessary to fix at the outset the number of calories or units of energy needed per day and per week by each member of the family. The father is assumed to be engaged in average manual work, and the mother to be occupied with household duties.

The requirements given below in units of energy, it is believed, will be conceded to represent an adequate, but not excessive, supply. The food is assumed to be "as purchased."

[1] A "normal" working-class family is defined as consisting of father, mother, not more than five children, the eldest being under fourteen years of age, and without lodgers or dependants.

This implies that a margin of 10 per cent. over all is allowed for waste in cooking and serving.

CALORIES OR UNITS OF ENERGY REQUIRED

	Per day.	Per week.
Father	3,400	23,800
Mother	2,750	19,250
Boy 13	3,000	21,000
Boy or girl 11	2,400	16,800
Boy or girl 9	2,000	14,000
Boy or girl 7	1,750	12,250
Total	15,300	107,100

The weekly supply of energy in food may therefore be taken as 107,000 large calories. That is to say, the family of six persons requires the equivalent in food of 4·5 men each receiving 3,400 calories per day, or as it is usually expressed, has a " man-value " of 4·5.

There are many ways in which this calorie requirement could be met in actual foods. In theory the number and variety of ways are endless, but limits are set in practice by the circumstances of the family and by the foods available on the market. It seems, therefore, best to give instances of how these requirements have been met in actual life by labouring families under different circumstances.

In the first place three instances are given of food actually consumed by families of the labouring class having a man-value of approximately 4·5. They are as follows: (A) (man-value 4·4) based on statistics of food consumed by agricultural families collected by Mr. Wilson Fox and published by the Board of Trade in 1903; (B) (man-value 4·5 [1]) based on similar statistics collected by the Board of Trade regarding food consumed by urban working families and published in 1904; (C) based on information collected in 1917 by the War Emergency Committee and supplied to the Ministry of Food.

[1] The actual man-value of these urban working-class families was estimated to be equivalent to 4·14 men. The quantities of food per week in dietary B have therefore been proportionately raised to meet the needs of a family of 4·5 men.

Foods.	(A) Agricultural families, 1902	(B) Urban working families, 1903.	(C) Rural and urban working families, 1917.
	lb. oz.	lb. oz.	lb. oz.
Bread and flour (as bread)	40 0	35 0	38 0
Other cereals	1 8	3 0	2 14
Beef and mutton	3 8	7 8	7 1
Pork and bacon	4 0	4 0	1 6
Fish	—	—	1 9
Milk	5 pints	10½ pints	11½ pints
Butter and margarine	2 0	2 8	3 0 (Butter, margarine, lard and dripping together)
Lard and dripping	1 0	1 0	
Cheese	1 4	1 0	0 14
Potatoes	28 0	21 0	18 0
Other vegetables (fresh)	7 0	7 0	4 0
Pulse (dried)	—	—	1 9
Sugar	4 8	6 0	3 6
Jam and syrup	2 0	2 0	2 12
Fruit (fresh)	—	—	2 2
„ (dried)	—	0 12	0 1½
Eggs	—	8 (only)	—
Cocoa	—	—	0 1½
Sundry foods and meals out	—	9 pence	—
Calories per week	99,890	107,700	99,257
„ „ man per day	3,240	3,410	3,160

The mean of the three dietaries (A, B, C) gives the following weekly quantities for the family in question, omitting fractions of an ounce :

	lb. oz.		lb. oz.
Bread	37 10	Other fresh vegetables	6 0
Other cereals	2 7	Pulse (dried)	0 8
Beef and mutton	6 0	Sugar	4 10
Pork and bacon	3 2	Jam and Syrup	2 4
Fish	0 8	Fruit (fresh)	0 11
Milk	9 pints	„ (dried)	0 4½
Butter, margarine, lard, and dripping	3 3	Eggs	3 (only)
Cheese	1 1	Cocoa	0 0½
Potatoes	22 5	Sundry foods and meals out	3 pence

Mean total calories per week	102,282
„ calories per man per day	3,270

Considering the dietaries separately it will be seen that of the three, dietary (B) is the only one which furnishes 3,400 calories per man per day. It may, therefore, be taken as adequate. The average income of the household, namely

56*s.* 10*d.*, was at that time (1903) sufficient to provide enough food for all the members of the family of four children on the scale mentioned.

In the case of dietary (A) based on the food consumed by agricultural families, the quantities of food under each head have been increased to the next highest half or quarter pound. The food actually supplied was therefore somewhat less than is here shown. The average wage per family, namely 18*s.* 6*d.* per week, was too little even at the period when the information was collected (namely 1902) to provide enough for all. It is usually assumed in such cases that the wage-earner of the family receives more than his share of the total food to enable him to do his day's work, the other members of the family having to be content with correspondingly less. The effect of the low wage is indicated also by another feature of this dietary, namely, a low consumption of meat and milk, together with a correspondingly high consumption of bread and potatoes.

Dietary (C) shows some features of interest. The data were collected in 1917 at a time when potatoes and other fresh vegetables were scarce. The supply of sugar was reduced, and the price of flesh meat was higher than in pre-war times. To make good for a reduction of supplies under these heads the working man's family had to resort to breadstuffs, of which the consumption shows a distinct increase.

A comparison of the proportions of the total food energy supplied by each class of food in the three dietaries is shown in the following table :

	(A) Per cent.	(B) Per cent.	(C) Per cent.
Bread and cereals	50·5	43·6	51·1
Meat and fish . . .	12·0	15·6	11·8
Butter, margarine, lard, etc.	10·8	11·7	10·8
Milk and cheese . .	4·25	5·5	6·2
Sugar, jam, syrup . .	10·1	11·8	8·8
Vegetables, etc.	12·25	11·6	11·0
	99·9	99·8	99·7

It is worth noting that bread and meat in all the dietaries provide approximately 60 per cent. of the total energy, and that there is a reciprocal relationship in the degree to

which each of these two foods contributes to make up this amount.

Three other estimates (D, E, F) are also given, illustrating a different way by which the food requirements of the family in question can be approximately determined.

They are based on the average consumption of food per head in the country as a whole. Two points are brought out in these, namely, the improvement in the feeding of the working man which would result if the total food at present consumed could be more evenly shared by all classes, and at the same time the limits set to variation in the diet by the actual foods available.

Scale D is based on the food consumption of Great Britain in 1908 as estimated by the writer; Scales E and F are based on the estimates of the Food Supply of the United Kingdom as drawn up by the Food Committee of the Royal Society; Scale E representing the average for the quinquennium 1909–13, Scale F that for the year 1916.

Foods.	Scale D. lb. oz.	Scale E. lb. oz.	Scale F. lb. oz.
Bread and flour (as bread)	36 0	32 8	34 11
Other cereals	1 0	3 0	3 12
Beef and mutton	10 4	11 0	9 6
Pork and bacon	4 9	4 0	4 10
Other meat	0 15	0 7	0 7
Fish	4 0	4 12	2 12
Milk	21 pints	20 pints	20½ pints
Butter	2 0	1 13	1 5
Lard, margarine, dripping	1 4	1 3	2 2
Cheese	1 0	0 14	0 10
Eggs	17 (only)	11 (only)	9 (only)
Potatoes	30 0	24 0	24 8
Other vegetables (fresh) } Pulse (dried) }	9 0	7 0[1]	6 0[1]
Sugar	9 0[2]	9 0[2]	7 10[2]
Cocoa and chocolate	—	0 5	0 6
Fruit (fresh)	7 8[3]	6 0[3]	5 5[3]
„ (dried)	1 0	1 0	0 11
Calories per week	124,550	129,100	129,650
„ „ man per day	3,954	4,100	4,116

[1] Refers to cottage and garden produce not estimated by weight.
[2] Includes sugar used in manufactures.
[3] Includes fruit used in jam making.

The mean of the three dietaries (D, E, F) gives the following quantities, omitting fractions of an ounce :

	lb.	oz.		lb.	oz
Bread	34	6	Cheese	0	13
Other cereals	2	9	Eggs	12 (only)	
Beef and mutton	10	3	Potatoes	26	2
Pork and bacon	4	6	Other vegetables (fresh) } Pulse (dried)	7	5
Other meat	0	10			
Fish	3	13	Sugar	8	8
Milk	20½ pints		Cocoa and chocolate	0	4
Butter	1	11	Fruit (fresh)	6	4
Lard, margarine, and dripping	1	8	„ (dried)	0	14

Mean total calories per week 127,760
„ calories per man per day . . . 4,040

These estimates represent the quantities of food calculated to be available in this country at three different periods of time for a family of six persons including four children of the ages specified, if the total food of the country were evenly distributed amongst the population. In all the important items there is a striking similarity. An approximate average such as is given above may therefore be taken as closely representing the quantities of different foods available for a family such as is under consideration. These are sufficient to provide a gross energy supply of 4,000 C. per man per day; but these quantities represent supplies delivered at the port or produced on the home farm. Before they reach the consumer there is an unavoidable loss in distribution probably approaching 10 per cent. If a deduction be made to allow for this, the balance would approximately furnish 3600 C. per day as purchased by the consumer, which is comparable to the 3,400 C. shown in table B.

The proportion of the total energy of the day's diet supplied by each class of food is shown in the following table :

	Scale D. Per cent.	Scale E. Per cent.	Scale F. Per cent.
Bread and cereals	35·9	35·2	38·0
Meat and fish	19·3	18·7	18·2
Butter, margarine, lard, etc.	9·4	8·5	9·8
Milk and cheese	8·2	7·4	7·2
Sugar	13·0	12·5	9·7
Vegetables, etc.	14·0	17·6	17·0

Comparing these figures with those based on the actual consumption of food by working-class families, the latter show

a higher relative consumption of breadstuffs, together with a lower consumption of meat and milk. The proportion of total energy provided by bread and meat together is also higher in the working-class diets than in the food supplied to the country as a whole—namely, 60 per cent. as compared with 55 per cent. The figures only express what is a matter of common knowledge respecting the dietary habits of different classes of the community. Such differences cannot, however, be taken to indicate that the diet shown in scale B is less valuable as a source of energy than one providing the same number of calories according to a scale represented by the averages of D, E, F. Both diets provide sufficient total protein and sufficient total calories, and it does not seem to be a disadvantage in the case of diet B that more of its calories are supplied by breadstuffs and less by meat, than in the case of the other diets. It is even possible that it may be an advantage, since flesh protein exerts a stimulating action on the combustion of food within the human body, increasing the heat production without producing a corresponding increase in the output of work.

POPULAR SCIENCE

THE 34 SUPERMAGIC SQUARE

By BRIGADIER-GENERAL F. J. ANDERSON, C.B.

In the progress report of the Superintendent of Hindu and Buddhist Monuments for 1915-16 was reproduced a Hindu magic square, found inscribed on a hidden portion of a lintel, brought to light by a fall of masonry, in the Chota Surang shrine at Dudhai in the Jhansi District, India.

This square, which is said to date from the first half of the eleventh century, is as follows :

7	12	1	14
2	13	8	11
16	3	10	5
9	6	15	4

In addition to the usual claim for such squares that the rows, columns, and diagonals each total 34, the discoverer in this case points out that the sub-squares (*i.e.* the numbers in the four cells clustered around any point where two lines intersect) each give a similar total, but we shall see presently that this enumeration by no means exhausts the supermagic properties of the square.

The following general definition of the term " Magic Square " is given by Hutton (*Recreations in Mathematics*, 1803), and seems to be generally accepted :

" The name magic square, is given to a square divided into several other small equal squares or cells, filled with the terms of any progression of numbers, but generally an arithmetical one, in such a manner, that those in each band, whether horizontal, or vertical, or diagonal, shall always form the same sum."

Hutton gives various rules, some original, some derived from previous writers, for the formation of such squares, but it will suffice here to reproduce the result so far as a 16-cell square of the first sixteen numbers is concerned :

1	15	14	4
12	6	7	9
8	10	11	5
13	3	2	16

Hutton's 34 Square.

This solution fulfils the requirements of his definition, but it falls short of the claims for the Dudhai square, in that some only of the sub-squares total 34.

Popular attention having been directed to the 34 square by a competition in one of the weekly journals some thirty years ago, the following solution was arrived at :

14	1	15	4
7	12	6	9
2	13	3	16
11	8	10	5

It was found to possess the following, at first sight "super-magic," properties, which I give *in extenso* for the benefit of the curious, without entering into any explanation as to how the possession of certain properties is involved as a natural sequence to that of others. The following each total 34 :

(*a*) All rows, columns, and diagonals.

(*b*) All sub-squares of four numbers.

(*c*) The four corner numbers.

(*d*) Parallel semi-diagonals, *e.g.* $(1 + 7 + 16 + 10)$, $(15 + 9 + 2 + 8)$, etc.

(*e*) Parallel quarter- and three-quarter diagonals, *e.g.* $(14+9+3+8)$, $(11+1+6+16)$, $(4+7+13+10)$, etc.

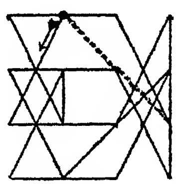
(*f*) Opposite parallel half-sides taken anywhere, *e.g.* (14 + 1 + 11 + 8), (1 + 15 + 8 + 10), (11 + 2 + 16 + 5), etc.

(*g*) The four corner numbers of every square of 9 cells, *e.g.* (14 + 15 + 2 + 3), (12 + 9 + 8 + 5), etc. It may be noticed that the opposite corner numbers total 17, *e.g.* (14 + 3), (15 + 2), (12 + 5), (9 + 8), etc. Hence the two diagonals of any 9-cell square are equal to one another.

(*h*) Similar terminations of any Knight's move on opposite sides of any central line of the square, *e.g.* (14 + 13 + 4 + 3), (12 + 11 + 6 + 5), (8 + 16 + 1 + 9), etc.

(*i*) The sum of the two central numbers in any line, horizontal or vertical, and the two outer numbers of a parallel line next-but-one to it, *e.g.* (1 + 15 + 2 + 16), (12 + 13 + 5 + 4), etc.

In addition to these properties, it is found that the "graph" produced by joining the consecutive numbers 1, 2, 3, etc., in rotation (including the line, shown dotted, joining 16 and 1) forms an absolutely symmetrical figure, thus:



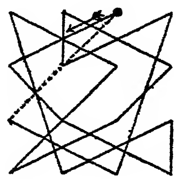
The symmetry of the figure will be the more apparent if it be turned over on its right-hand side.

Now it is to be noted that both the Dudhai Square and that reproduced below, which was discovered on the gate of the fort of Gwalior, share all these properties.

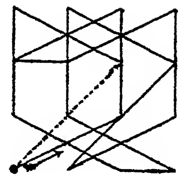
15	10	3	6
4	5	16	9
14	11	2	7
1	8	13	12

Gwalior Square.

Their respective " graphs " are as follows :



Dudhai Square.



Gwalior Square.

The contortions through which any perfect 34 square may be put without any sacrifice of its supermagic properties are somewhat extraordinary. For instance, taking any one primary square, which we will assume to be that shown with the number 1 in its left-hand top cell, we can imagine it inscribed on a cylinder with vertical axis, revolving to the left or right (say the left in this instance), thus bringing each column in rotation into the position occupied by the first column in the primary square, thus :

1	8	10	15
14	11	5	4
7	2	16	9
12	13	3	6

Primary Square.

8	10	15	1
11	5	4	14
2	16	9	7
13	3	6	12

10	15	1	8
5	4	14	11
16	9	7	2
3	6	12	13

15	1	8	10
4	14	11	5
9	7	2	16
6	12	13	3

First Derivatives.

We can then imagine each of these four squares in turn similarly dealt with on a cylinder with horizontal axis, revolving

say upwards towards us from the plane of the paper, producing three more forms, thus from the first square :

14	11	5	4
7	2	16	9
12	13	3	6
1	8	10	15

7	2	16	9
12	13	3	6
1	8	10	15
14	11	5	4

12	13	3	6
1	8	10	15
14	11	5	4
7	2	16	9

Example of Secondary Derivatives.

and similarly for the other three squares.

We have now a total of sixteen squares with each of the numbers 1, 2, 3, etc., occupying the left-hand top cell in turn.

Again, we can read each of these sixteen squares in four ways, according to which corner we allot to the left-hand top cell. Thus the primary square may be read as :

12	7	14	1
13	2	11	8
3	16	5	10
6	9	4	15

if we suppose it turned on its right side, or

15	4	9	6
10	5	16	3
8	11	2	13
1	14	7	12

if we suppose it turned on its left side, or

6	3	13	12
9	16	2	7
4	5	11	14
15	10	8	1

if we assume it turned upside-down.

The primary square thus assumes sixty-four guises, each of which has in addition its corresponding reflected, or looking-glass, form, thus raising the total to 128.

We are not yet at an end of the juggling to which our primary square so readily lends itself, for we can imagine its corner sub-squares converted into successive horizontal lines by supposing them unrolled in rotation (*a*, *b*, *c*, *d*) as shown in diagram A, or we can imagine outer opposite half-sides (*a*, *b*) and inner sub-squares (*c*, *d*) similarly unrolled as shown in diagram B, thus producing two new primary squares :

1	8	10	15
14	11	5	4
7	2	16	9
12	13	3	6

Primary Square.

Diagram A.

1	8	11	14
12	13	2	7
6	3	16	9
15	10	5	4

Diagram B.

1	8	13	12
15	10	3	6
4	5	16	9
14	11	2	7

New Primary Squares.

Each of these new primary squares also give us a further sixty-four solutions (or 128 if looking-glass forms be included).

The result is a grand total of 192 forms (or 384 if looking-glass solutions be added).

The following Key-Tables (I., II., and III.) present all these solutions in a compact form.

To use them the reader can readily make a stencil by cutting out of a piece of cardboard a square hole of the exact size required to embrace 16 cells of a key-table.

1	8	10	15	1	8	10
14	11	5	4	14	11	5
7	2	16	9	7	2	16
12	13	3	6	12	13	3
1	8	10	15	1	8	10
14	11	5	4	14	11	5
7	2	16	9	7	2	16

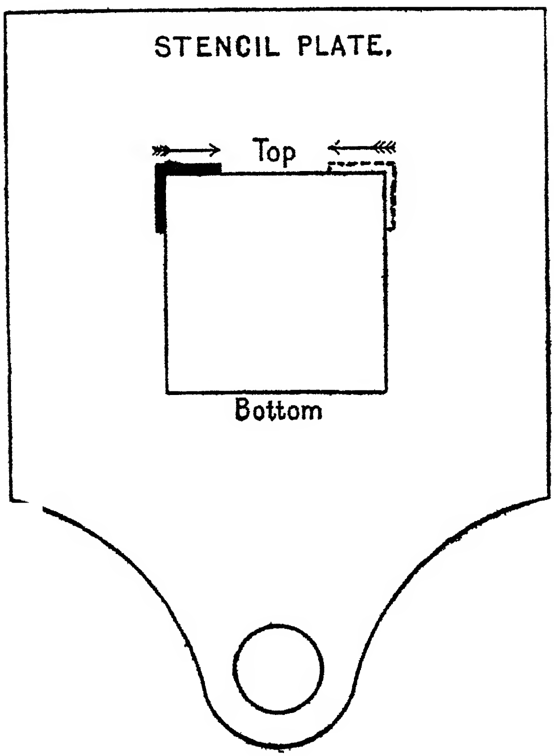
Key-Table I.

1	8	11	14	1	8	11
12	13	2	7	12	13	2
6	3	16	9	6	3	16
15	10	5	4	15	10	5
1	8	11	14	1	8	11
12	13	2	7	12	13	2
6	3	16	9	6	3	16

Key-Table II.

1	8	13	12	1	8	13
15	10	3	6	15	10	3
4	5	16	9	4	5	16
14	11	2	7	14	11	2
1	8	13	12	1	8	13
15	10	3	6	15	10	3
4	5	16	9	4	5	16

Key-Table III.



To illustrate the method of using the stencil: Let it be required to know the forms of all squares having say the number 13 in the left-hand top corner.

Keeping the bottom of the stencil horizontal, and placing the left-hand top cell (indicated by the dark band) on whichever of the two numbers 13 in Key-Table I. does not involve the stencil travelling off the Table, we read, from left to right as indicated by the arrow :

13	3	6	12
8	10	15	1
11	5	4	14
2	16	9	7

Next, turning the stencil so that its bottom occupies a vertical position on the right, we read :

13	2	11	8
3	16	5	10
6	9	4	15
12	7	14	1

Similarly, turning the bottom to the left, we have :

13	8	11	2
12	1	14	7
6	15	4	9
3	10	5	6

and, finally, turning the stencil upside-down, we get

13	12	6	3
2	7	9	16
11	14	4	5
8	1	15	10

Again, applying the right-hand corner of the stencil (indicated by a dotted border) in a similar, but back-handed, manner, we obtain four looking-glass squares :

13	12	6	3
8	1	15	10
11	14	4	5
2	7	9	16

13	8	11	2
3	10	5	16
6	15	4	9
12	1	14	7

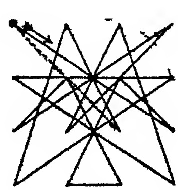
13	2	11	8
12	7	14	1
6	9	4	15
3	16	5	10

13	3	6	12
2	16	9	7
11	5	4	14
8	10	15	1

thus providing 8 solutions from Key-Table I., while Key-Tables II. and III. each yield a similar number, giving a grand total of 24. Similarly for any of the first sixteen numbers selected.

Mention has been made above of the " graphs " of these squares, and I will therefore only remark here that some of them produce very pretty patterns. As an example I give a square, derived from Key-Table I., with its graph.

1	15	10	8
12	6	3	13
7	9	16	2
14	4	5	11



I should have mentioned that the term " Nasik " is now generally applied to such squares.

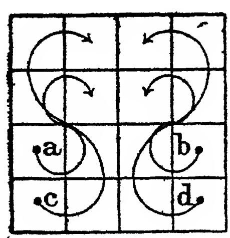
Allusion is sometimes made to the following magic square:

16	3	2	13
5	10	11	8
9	6	7	12
4	15	14	1

which appears on a 16-paned window in the background of Albrecht Dürer's picture "Melencolia." The date of the picture is recorded as 1514 in the left-hand bottom corner under the initials "A. D."

Now, while the square is not a supermagic square, it possesses the peculiarity that the two centre numbers on the lowest line form a chronogram of the date.

The square is readily derivable from the Dudhai square, reproduced at the head of this article, or indeed from any supermagic 34 square. The following diagram indicates how this may be effected.



It remains only to notice that, while the Dudhai square was so placed as to be beyond the ken of the vulgar, Dürer's bastard version of it appears in the midst of certain emblems, and this coupled with the fact that the artist lived at Nurnberg, an architectural centre, may serve to suggest a common masonic origin for both efforts.

NOTES

The late Sir Alexander Pedler, C.I.E., F.R.S.

We very much regret to announce the death of Sir Alexander Pedler, C.I.E., F.R.S., which occurred suddenly on May 13. Sir Alexander was born at Dulwich in 1849, and was educated at the City of London School and the Royal College of Science. Many years of his life were spent in India, where he did much distinguished work as Director of Public Instruction for Bengal, Vice-Chancellor of the University of Calcutta, and additional member of the Legislative Council of the Governor-General of India. In recognition of his great public services he received the Order of Knighthood in 1900, and was created C.I.E. in 1901. He retired from India in 1906 and subsequently lived in London, where he continued to identify himself actively with scientific work and interests. His publications include various papers in the *Proceedings of the Royal Society*, the *Journal of the Chemical Society*, etc. From 1907 until his death he acted as Hon. Secretary of the British Science Guild, in which he took a keen interest and for which he did invaluable work. He was a man of broad outlook and sympathies, and had many friends by whom his death will be deeply felt.

Cd. 9011 (By Charles Mercier, M.D., F.R.C.P.)

This symbol is the official designation of the Report of the Committee appointed by the Prime Minister (Mr. Asquith) in 1916 to inquire into the position of Natural Science in the Educational System of Great Britain, and a more admirable Report it has never been my good fortune to peruse.

At the present time the country at large is deeply stirred by a consciousness of the deficiencies of our system of Education. It is not the first time that has happened. Three hundred and fifty years ago, under the stimulation of Erasmus, Colet, Ascham, and other scholars and reformers, a great movement for the improvement of education began, and many Grammar Schools were founded in various parts of the country. A hundred

years ago, Charles Lamb made his humorously querulous complaint that the schoolmaster was abroad. Fifty years ago, under the initiation of the Prince Consort, a vain attempt was made to stir the dry bones of classical education, and to vivify them by the vitalising influence of an infusion of science. Forty years ago, after the tremendous impression made upon the country by the rapid and overwhelming success of Prussia and her German Allies against France, a success that we attributed without much reason to the superior education of the Germans, the first Act making education compulsory on every child in these realms was passed; and since then the system of Education pursued in our schools and Universities has been perpetually tinkered at and altered in this detail and in that.

It is the terrific strain of racing that reveals the latent weaknesses of a motor car, and shows in what respect it needs strengthening and improving, and it is the terrific strain of such a war as the present one that reveals the weaknesses of nations. The war has revealed to an astonished world the most abysmal ignorance, not only here and there, but generally, and almost universally, among the men to whom we have entrusted the government of the country, both the politicians whose cultivation of the arts of the politician has left them neither time nor inclination for the cultivation of any other, and the permanent officials of the Government, who gain their position by passing examinations, and in whom, therefore, extreme and discreditable ignorance is more surprising. Some of these instances of ignorance were so gross and astounding, and had so direct an effect upon the conduct of the war, that a clamour broke out, demanding that our rulers should be educated. A great gathering of men of the highest scientific eminence formed itself into a Neglect of Science Committee, and urged upon the Government and the nation the claims of Natural Science to a position in the national scheme of education. Committee after Committee has been appointed, has sat, has reported, and no other Committee has produced so admirable a report as that officially known as Cd. 9011.

In ordinary circumstances and in ordinary times, the appointment of the Committee would have served its purpose in allaying public clamour, and in tiding the Government over an awkward and embarrassing moment; by the time its Report had appeared, public interest in the subject would have evaporated,

and the official world, as it tied up the Report in red tape and stuffed it into a pigeonhole, would have chuckled with satisfaction at having once more befooled the nation by a stale but infallible device. There the Report would have remained, accumulating dust, until it was disinterred by some subsequent Committee, appointed on a similar occasion, whose Report would have been similarly disposed of in its turn. The Reports of several previous Committees, long since forgotten, and of course disregarded, are referred to in Cd. 9011. I may be unduly sanguine, but it does seem to me impossible that the Report of this last Committee should be utterly disregarded. It is so reasonable, so moderate, so convincing, so persuasive; it displays such a comprehensive grasp of the whole problem of education; that it must be exasperating indeed to the mind of the official and the classical schoolmaster, hidebound in the prevailing antiquated system; and if it could only reach the public at large, it must surely produce a great effect on the public mind, preoccupied though it is with the conduct of the war.

We can imagine the Prime Minister of the day, a scholar of Balliol, a man steeped in the Public School and Oxford tradition of the cult of uselessness and verbiage, accustomed both in his primary profession of the law and in his subsequent profession of politics to regard words as omnipotent—we can imagine him suddenly aroused by the war to a consciousness that after all words are not everything; that there are such things in the world as hard facts, which no words will dissipate, and which must be reckoned with; we can hear him inquiring whether there is anyone who knows anything about these awkward, embarrassing, degraded things called facts, and being told that there is a realm of knowledge called Science, whose function it is to disregard words and to study the facts that are symbolised by words; and we can see him determining reluctantly that after all perhaps it would be well to demean his scholarship and his mastery of words by taking counsel of those whose business it is to study, not words, but facts. Hence the appointment of this Committee.

The Committee was appointed to inquire into the position occupied by Natural Science in the Educational System of Great Britain, and to advise what measures are needed to promote its study, and this the Committee has done; but

it has done far more than this. It has elaborated an organised scheme of education, beginning with the Elementary and Preparatory Schools, extending through the Secondary Schools to the Universities and Professions, and through and beyond the Universities to post-graduate education. I shall have a few objections to make, but the main task of the Committee has been admirably performed.

The Committee does not formulate the aim or aims that education should set itself to accomplish; and no doubt, in view of the national detestation of general principles, the abstention was wise, though for my own part I must regret it; for the validity of the whole scheme depends upon the degree in which it is calculated to fulfil the ultimate purpose of education, whatever that may be; but the Committee leaves us in no doubt as to the purpose of its own scheme of education, as distinguished from the general purpose of education itself. The scheme of the Committee is designed to put a stop to the shocking waste of ability that at present prevails; to discover ability wherever it may be found and of whatever kind it may consist; to give it its chance; to educate it, develop it, and utilise it for the benefit of its possessor and for the service of the nation and of mankind. Surely this is a laudable purpose. Surely it is well that the Committee has stepped outside its province to point the way.

The Committee prefaces its Report with some general remarks:

"There can be no need now to labour the important part that Science should play in our education, but memories are short and it may be well to register in formal words for future comfort, if not reproach, what all would readily grant at this moment. It is not possible to give an exhaustive account of the scope of Science, but it is not superfluous to point out that it has several distinct kinds of educational value. It can arouse and satisfy the element of wonder in our natures. As an intellectual exercise it disciplines our powers of mind. Its utility and applicability are obvious. It quickens and cultivates directly the faculties of observation. It teaches the learner to reason from facts which come under his own notice. By it, the power of rapid and accurate generalisation is strengthened. Without it, there is a real danger of the mental habit of method and arrangement never being acquired. Those who have had much to do with the teaching of the young know that

their worst foe is indolence, often not wilful but due to the fact that curiosity has never been stimulated and the thinking powers never awakened. Memory has generally been cultivated, sometimes imagination, but those whose faculties can best be reached through external and sensible objects have been left dull or made dull by being expected to remember and appreciate without being allowed to see and criticise. In the science lesson, the eye and the judgment are always being called upon for an effort, and because the result is within the vision and appreciation of the learner he is encouraged as he seldom can be when he is dealing with literature. It has often been noticed that boys when they begin to learn Science receive an intellectual refreshment which makes a difference even to their literary work."

There is more than this, however. On a subsequent page the Committee says:

"The introduction of practical work into the curriculum widens the opportunities of discovering ability which might otherwise be overlooked. One witness writes: 'Often, very often, the dunce of the form when put on to practical work becomes brilliant.' It is the function of any system of education to discover ability as well as to develop it, and under a system in which the curriculum is entirely bookish many able boys may be depressed and lose all interest in their work from want of success and of opportunity to reveal their ability."

As to this I can personally testify. At school I was regarded as an utter dunce. I spent seven wasted years in the fruitless endeavour to acquire the rudiments of Latin, and I failed egregiously and ignominiously. I never was able to construe a simple sentence of Latin. I left school with the reputation of a dunce, and the disheartening conviction that I was incurably imbecile. When I entered the hospital and took to the study of Science, I cleared the board of every prize and scholarship that was open to competition.

The Committee will of course be accused of desiring to oust literary and linguistic studies altogether from the scheme of education that they recommend, and upon this will be founded a charge of encouraging a gross and sordid materialism, blind and deaf to every consideration but that of material and crapulent prosperity. This is the pseudo-argument that is always advanced against those who advocate the inclusion of Natural Science in Education. Let us hear, therefore, what the Report says on the matter.

"Some of the advocates of scientific training have damaged their cause by claiming too much for their subject, and by seeming to depreciate the value of the literary studies which had tended to monopolise the attention of the ablest boys who enjoyed secondary education. To many Greek and Latin have seemed enemies who, from having occupied the educational ground betimes, have been able to dig themselves in and to hold an almost impregnable position, due not to their merit as educational instruments, but to the accident of priority. There is truth in this, but we do not think that the surest method of victory is to be found in the overstatement of the merits of Science or the depreciation of the value of classics. Some of the ablest minds have received from their classical instruction enduring gifts that have been of great service to the State and of great refreshment to their possessors. It is our belief that a better service can be done and a like refreshment gained by those whom we hope to see educated on the wider lines laid down in our Report. The humanising influence of the subject has too often been obscured. We are, however, confident that the teaching of Science must be vivified by a development of its human interest side by side with its material and mechanical aspects, and that while it should be valued as the bringer of prosperity and power to the individual or the nation, it must never be divorced from those literary and historical studies which touch most naturally the heart and the hopes of mankind."

Nor is this only a pious opinion. It is taken into account in the recommendations of the Committee with respect to the time to be allotted to the teaching of Science in schools. The Committee demands in Secondary Schools for boys, not less than four "periods" per week in the first year after the age of twelve, and not less than six "periods" thereafter. It appears that a "period" is three-quarters of an hour, so that all that the Committee demands is, in Boys' Secondary Schools, from three hours to five and a half hours per week, and in Girls' Secondary Schools three hours per week. Even this scanty allowance is difficult to obtain, owing to the insistent demands of other subjects that are already in possession of the field. In my opinion it is too little, and might easily be increased by dropping out of the regular curriculum subjects that should be only exceptional, and restricted to a minority of the pupils. The Committee, if it does not wholly accept, yet does not reject the view that the prevailing curriculum is sacrosanct, and

must not be disturbed. The timidity with which it ventures to suggest modification of the curriculum may very likely be due to a hope of propitiating the party in possession, and deprecating its opposition. This hope will probably be disappointed, and I cannot but think that the Committee would have had less chance of being stung if it had grasped the nettle boldly instead of touching it gingerly with the tips of the fingers.

A general education should, in the opinion of the Committee, be pursued up to the age of sixteen, and should provide for the study of English, including History and Geography; Languages other than English; Mathematics and Natural Science; and a set of subsidiary subjects, such as Drawing, Music, Handicraft, Domestic Subjects, etc., in various proportions. It is satisfactory to find the study of English placed first, and satisfactory, too, that the Committee insists again and again throughout its Report on the importance of this most neglected subject. If we may judge by the result, instruction in the structure and use of the English language is utterly unknown in schools, and the consequence is that not one English writer in a thousand can use his own language without making shocking and discreditable blunders. The study of the mother tongue should be the very first and most prominent subject in education, not only because of its direct importance, but because clearness of expression means clearness of thought; and he who does not express himself clearly does not think clearly. The inclusion of History and Geography in English is of course absurd, but no doubt in acquiescing in this inclusion the Committee was for politic reasons bowing itself in the House of Rimmon. Of course these subjects should be taught, but Geography might well be taught as a Natural Science, which it is, and in conjunction with Physics, Physiography, and Geology.

It is with the second group of subjects that I quarrel. The Committee takes it as a matter of course, as practically every other authority upon education does, that the acquisition of a second, and even of a third language is of primary importance in education. With this I profoundly disagree. I think it is a superstition that has been inherited by the present generation from the Dark Ages of education, when the acquisition of Latin and Greek was the beginning, middle, and end of education. The acquisition of a second and of a third language has an

undoubted utility in certain walks of life, and for those who are to follow these walks of life it is desirable; but in very many occupations it is of no utility at all, and the time devoted to it, which is usually excessive, even if the acquisition were important, is doubly excessive. I am very far from erecting practical utility as a standard by which the desirability of subjects of education should be gauged; but if languages are not to be learnt for their practical utility, I am at a loss to find any other reason for their cultivation. We are told that the learning of a second and third language, especially if it is a dead language, is a splendid means of cultivating the mind—another superstition, which rests upon no evidence whatever as far as I have been able to ascertain; and we are told that the acquisition of the dead languages is morally and intellectually elevating, a safeguard against materialism, a refining and spiritualising influence, without which a man remains destitute of all high and worthy motives, sunk in debased and sordid aims and pursuits. I have examined this assertion again and again, and I can find no evidence whatever to support it. It is an assertion as baseless as that the wearing of a charm will ward off misfortune; and the same cast of mind that entertains the one superstition cherishes the other. And the devotion of disproportionate time and attention to languages, whether dead or living, in the scheme of education, is not merely waste; it is actively pernicious and baneful. It does irreparable harm to the growing mind. It fosters and increases that logolatry, that worship of words, that inability to distinguish between words and things, that pseudo-solution of problems by the invention of neat phrases, that pursuit of such flimsy will-o'-the-wisps as socialism, war to end war, destruction of militarism, efficiency, democracy, spiritual influence, and so forth, which to nearly all the people who use them have no clear meaning, but are mere "words of power" like Abracadabra and Kogula. The pursuit of words is no less eager, the worship of words is no less devoted, in science than in other realms of endeavour, and in medicine is rampant. The more "scientific" the medical practitioner, the more importance he attaches to diagnosis; and diagnosis means in nearly every case the discovery of an appropriate name, and means no more. Never have I been consulted about a difficult case without the question being put to me, What do you call it? And when I have attached

a name to the malady, the inquirer has been quite satisfied, and has been convinced that he then knew all there is to be known about it. Similarly, engineers demand that the action of a machine, or of one part of a machine upon another, shall be "positive," and if they are told that it is positive they are satisfied. The worship of words is universal, and is to be ascribed in very large part to the prominence given to words in our system of education, and especially to the cultivation without sufficient reason of languages.

There is another sound and most important reason why the inculcation of a second and even a third language, as the Committee recommends, should not form, as a matter of course and of routine, an integral part of the education of every child. It is not every child that has the ability to acquire, by no matter how much devotion of time and energy, a second language. The language faculty is a special faculty, a sporadically distributed faculty, which some persons possess in high degree, and others are utterly destitute of. It is vain and futile to attempt to teach music to those who are tone deaf; and it is equally vain and foolish to attempt to teach languages to those who are language deaf. Complete tone deafness is perhaps not frequent; but it occurs, and is consistent with the highest ability in other directions, as the cases of Dr. Johnson and Macaulay testify. Complete language deafness also is not frequent; but it also occurs, and it also is consistent with the highest ability in other directions. If it is vain and futile to teach music to those who are totally deaf to tone, is it not nearly as vain and futile to teach those who are partially tone deaf, and to make music a common ingredient in the education of every child, as was done fifty years ago in the education of girls? And is it not equally vain and futile to make languages a common ingredient in the education of every child, boy or girl, whether the language faculty is possessed or not, and whether it is possessed in high or in low degree? The demands of Natural Science to its proper place in the scheme of education are constantly refused, minimised, and whittled away because of want of time. The curriculum is already full, and it is impossible to squeeze in an additional subject. The difficulty is artificial, adventitious, and unnecessary. Strike languages out, and there will be ample time for the proper cultivation of Natural Science as well as of other subjects now neglected.

Let it not be supposed that I undervalue literary subjects. I should allot to them a much larger share of the curriculum than they now possess; and I should find time for them by striking out languages, which are quite distinct from literary culture, and to a great extent antagonistic to it, though they are confused with it and mistaken for it. I agree most heartily with the Committee that literary culture is a most important part of the mental equipment of everyone; but it would be absurd not to recognise that in school life literary culture can only be begun. In school life only the foundation can be laid, and the most important part of the foundation for this and for most other mental acquisition is a thorough education, not in foreign languages, but in the mother tongue. For this reason I rejoice to see the emphasis with which the Committee again and again insists on the importance of the teaching of English. Literary expression has never, I think, in the whole history of this country, been at such a low ebb as it is at the present time. By literary expression I do not mean flowing language and the use of tropes and ornaments. I mean the ability to express plain meaning in plain words, so that the reader shall be in no doubt as to what the meaning is, and shall experience no difficulty, no hesitation, and no fatigue that can be avoided, in apprehending the meaning. This ability is extremely rare. I think it was never so rare as it is at the present time. It is also extremely discreditable—discreditable to the writers, and doubly discreditable to the schools in which they have received a so-called education. What sort of competence schoolmasters possess for the task of instructing their pupils in the use of their mother tongue may be estimated from the analysis, given in SCIENCE PROGRESS for October, 1917, of a literary composition by the head master of a great public school.

The Report under consideration, the product of a Committee of men of science, is extremely clear and well written, very unusually so for an official document, but it contains many peccadilloes, and is much to seek in the application of the difficult art of punctuation. It is, however, upon the whole a most statesmanlike document, broadminded, far-seeing, comprehensive, and persuasive. It ought to have a most powerfully beneficial effect upon the education of our future citizens. Whether it will or not remains to be seen.

The Future of Geophysics in Great Britain (H. S. J.)

The term Geophysics has, for the want of a better title, been coined to include those subjects which, as the word implies, are related to the physics of the earth. Such subjects include, amongst others, geodesy, tides, terrestrial magnetism, atmospheric electricity, seismology, latitude variation, and meteorology. Geophysics, therefore, borders on to many distinct branches of science, and it is necessary that a worker in any one branch should be acquainted with what is being done in the other branches, since developments in those branches may have a direct bearing on his own work. This has been very difficult in the past owing to the fact that there has been no common meeting-ground where those interested in these subjects could read and discuss papers. It is true that special branches of geophysics are catered for by different societies, latitude variation by the Royal Astronomical Society for instance, but it is the common meeting-ground that is required for the advancement of our knowledge. Moreover, geodetic work, including surveying, precise levelling, and gravity determinations, is practically confined in this country to a State Department. Without reflecting in any way on the Ordnance Survey Department, which has done excellent work, it cannot be denied that opportunities for investigation and research are limited on this account, and that it is difficult for those outside the Survey Department, who are interested in geodesy, to become acquainted with what is being done in this country. For these reasons progress in geodesy is restricted, and it is well known that there is an almost complete dearth in this country of students of higher geodesy.

It was therefore a step in the right direction which was taken last autumn by the British Association when it appointed a Committee to arrange meetings for the discussion of geophysical subjects. The members of this Committee are: Sir F. W. Dyson (Chairman), Dr. C. Chree, Col. Sir C. F. Close, Prof. E. B. Elliott, Mr. J. H. Jeans, Prof. A. E. H. Love, Major H. G. Lyons, Prof. A. Schuster, Sir Napier Shaw, Prof. H. H. Turner, Dr. G. W. Walker, and Dr. S. Chapman (Secretary) Meetings were held on the second Wednesday of each month from November until June, with the exception of January, the meetings being held at Burlington House, in the rooms of the Royal Astronomical Society. At the first meeting in November Dr. Chapman gave a report on magnetic surveys and charts by land and sea throughout the world, whilst Dr. Walker gave an account of his recent magnetic survey of the United Kingdom, made under the auspices of the Royal Society and the British Museum. At the December meeting Sir Napier Shaw opened a discussion on the general constitution and condition of the atmosphere, whilst Mr. Jeans dealt with the theoretical aspect of the subject. At the February meeting Col. Sir C. F. Close spoke on the influence of barometric pressure on mean sea-level, and Major Henrici opened a discussion on precise levelling. At the March meeting Dr. Chree opened a discussion on auroræ and the electrical state of the upper atmosphere, Prof. Fowler dealt with the spectrum of the aurora, and the Hon. R. T. Strutt spoke on the evidence for the existence of ozone in the upper atmosphere. The April meeting was devoted to seismology: Prof. Turner and Dr. Walker dealt with earthquake waves, and Mr. Oldham spoke about the frequency of earthquakes. At the May meeting Sir F. W. Dyson dealt with the variation of latitude and the movement of the earth's pole. The final meeting in June was devoted to a discussion on tides, which was opened by Prof. Lamb.

It will be seen that a wide range of subjects were covered, and the discussions were found both interesting and instructive by those who were able to attend

them. The attendance was sufficient to encourage the hope that a continuance of the meetings will be arranged for next winter. The Committee have under consideration the question of placing the project on a more permanent basis, and also the question of arranging for the publication of papers presented at the meetings. At present there is no means of publication, and so original papers are not presented. The first series of meetings was devoted mainly to the presentation of accounts of work up to the present time in various subjects ; these have not been published in detail, although of great value, but abstract reports of some length were given in the *Observatory*. It is to be hoped that it will be found possible to place the scheme on a basis which will ensure its future success : a great step towards the advancement of geophysical study in this country will then have been taken.

The Commencement of the Astronomical Day (H. S. J.)

It is probably known to most people that the astronomical method of reckoning time differs from the civil, the civil day commencing at mean midnight, whilst the astronomical day of the same date commences at the succeeding mean midday. The question as to the desirability of the uniform adoption of the civil day for both civil and astronomical purposes is one which is raised periodically. Laplace proposed the unification of the two systems as long ago as 1804, and his proposal was agreed to by the French Bureau of Longitudes, but no action was taken to carry it into effect. In 1884 the question was raised afresh at the International Congress at Washington, which decided in favour of unification. At a Congress at Geneva in the following year the resolution of the Washington Congress was the subject of a long discussion, and mainly through opposition from the German astronomers the matter was allowed to drop. The question was raised afresh a few years later by the Astronomical Society of Toronto, and again the French Bureau of Longitudes approved of the change. An able report on the *pros* and *cons* of the case by the late M. Poincaré is contained in the *Annuaire* of the Bureau of Longitudes for 1895 (a translation was given in the *Observatory*, **40**, 323, September 1917). For the change to be of value it is necessary that it should be adopted simultaneously by the Governments which publish the principal ephemerides. This agreement has not hitherto been obtained.

The question has recently been once again brought forward by the Astronomer Royal and Prof. Turner. It may be thought that the question is one of mere academic interest. This is not so, as it would affect the various Nautical Almanacs. The change, in fact, is now advocated on account of the simplification which would result to navigators, who find the present system confusing and liable to lead to mistakes. Thus Capt. Fulton, of the Board of Trade, gives the following instance : To find the Greenwich date of the Moon's meridian passage on board ship, "one must enter the *Nautical Almanac* under the heading Meridian Passage (Upper), and, if in West Longitude, take the difference between the time of passage on the given day and the day following, or, if in East Longitude, take the given day and the preceding day, using simple proportion to find the time of passage at ship. This is a simple operation and easily remembered, but, as it refers to astronomical time, the observer must remember that the Moon passes the meridian after midnight during the half-lunar month, and he must, therefore, note whether the observation is A.M. or P.M. As an example, suppose a ship in West Longitude on, say, the 16th July A.M., at ship then the dates for the meridian passage will be the 15th and 16th, and in East Longitude the 15th and 14th." It can readily be under-

stood that the alteration to astronomical time before the application of longitude is a real stumbling-block to many navigators.

Much has been said about the difficulties which would be caused to astronomers by the change. We do not think these objections have much weight except, perhaps, that a discontinuity would be introduced into astronomical records. For this reason it is desirable that the change, if adopted, should be adopted universally. Perhaps it would meet the case if civil time were to be adopted in the national ephemerides, and if astronomers were to agree to publish their observations in civil time.

The national ephemerides are prepared several years in advance, so that the change could not be made in any case for a few years. The matter is, however, under consideration by the Admiralty. Meanwhile, with a view to having the subject considered, expressions of opinion as to the desirability of a change, and, if considered desirable, as to the most suitable date for it, are invited by the Astronomer Royal, Royal Observatory, Greenwich, and Prof. H. H. Turner, University Observatory, Oxford.

The State, Science, and Agriculture (G. W. Harris, Little Hundridge Farm, Buckinghamshire)

In these days when interference by the State is rapidly becoming a habit in nearly every sphere of national activity, it may perhaps be not inopportune to consider soberly the problem of agricultural development in so far as it is a matter of official recognition. It is not to be assumed that State interference is, *per se*, good for any department of activity; but we seem to be rushing precipitately after the manner of the most approved Gadarene porker into that steep place. Hitherto agriculture has been a kind of happy hunting-ground for the vote catcher. The ordinary politician, like his ancestral amœba, engulfs what he can and where he can, but his path is indeterminate, being guided only by the opportunity for acquiring a brief notoriety. Consequently, a promise of more beer for the labourer, or fewer hours, or extra help for dusting turnips was considered a very liberal recognition in the matter of agriculture. A definite or national policy was unthinkable.

If after the war a return is made to unrestricted free trade—a procedure which is possible in view of the ex-Premier's recent utterances—the case for agriculture will be for ever damned. Under stress of circumstance and with the zealous, if unenlightened assistance of the Government, agricultural practice has been greatly modified in the last two years. The sole object of agriculturists at present is to increase production in every possible way. A return to pre-war methods would be disastrous to the farmer and calamitous to the nation. The farmer, after such a material change, would be faced with bankruptcy if he was compelled to revert suddenly to old methods, while the nation will be once more on the drift and the immense sacrifices made by the fighting men will be rendered entirely nugatory. Should, however, the problem of maximum production be continued as the fundamental basis on which the new England is to be reconstructed, the question then arises as to the best means to attain this end economically.

The present governmental methods, which have their own particular merit as the children of emergency, are not calculated to stand the test of normal times. They are, for the most part, ill-conceived, ill-considered and poorly executed. Judgment is too obviously lacking. Concessions are made which should never

have been required. Advice is asked when it is already too late to carry it out, and the capacity of the advisers is often open to question.

What is wanted to carry out thoroughly the policy of maximum production is the adequate combination of science with the art of farming, and this can only be attained by the liberal and benevolent assistance of the State. There are innumerable problems in farming which require solution, and which the farmer himself has not time to undertake. Tillage experiments, for example, the destruction of insect and fungoid pests, to mention but a few, all need investigation. The scientific expert, however, must be a somewhat exceptional man. In order to have a comprehensive grasp of his subject he should have had some general farming experience, for an expert without practice is infinitely more detrimental than a practical farmer without science. The scientific expert receives a fixed salary and can therefore evolve any number of schemes without being compelled to practise them on the *corpus vile* of his own pocket. With the farmer it is otherwise. He is bound to follow that course which pays best, irrespective of its merit and demerit from the purely scientific aspect. He does not require to be bombarded with advice from gentlemen whose creed may be summed up in the phrase: "I believe in the calory with the big C."

Many more experimental stations are therefore required, and these should be entirely State supported, the profit or loss on the undertaking being retained or borne by the State. The staffing of these establishments should be most carefully considered. Not infrequently they are like a den of Gallios who care for none of the practical difficulties of the farmer and are entirely out of sympathy with him. They dictate a course of action as if giving the law from Sinai, and the result is merely an estrangement between two classes of the community who are mutually necessary to one another. Agricultural journalists, who filch the writings of scientific men, are particularly useful in producing such a feeling of distrust, for the natural conclusion is that with all the knowledge apparently possessed by such a writer he would do far better farming, were he capable of it.

The influence of such establishments should be educational in the highest degree. They should be so conducted that neighbouring farmers will be anxious for their advice, and the results of their experiments should be published in a form accessible to the farmer. Such a book as Dr. Russell's on Manures is a typical example of the way in which such results should be written.

True it is that the practice of the very best farmers leaves but little for science to teach them, yet the majority are very far from having attained this degree of excellence. The farmer's outlook would be widened, and, generally speaking, the wider his outlook the more adaptable he is and the more ready to welcome improvements. The number of these experimental stations would depend on the size of the county, but there should be one at least in every county. Rothamsted is a pattern for all the world, and curiously enough, we have not plagiarised ourselves overmuch. The practice of ejecting from their farms those who, in the opinion of a motley committee composed of lawyers, auctioneers, and ex-farmers, are guilty of bad husbandry, contains a gleam of soundness, despite the crudity of the execution. If, after the war, we are really to aim at high production, the responsibility of the farmer will be greatly enhanced, and it will certainly be the business of those who control agriculture to see that adequate capital is possessed by the farmer and that his practice is not deleterious to the interests of his neighbours, and consequently to the community in general.

This brings us to the consideration of the nature of a body which shall control the agricultural policy of the country. Before all things agriculture must be

removed from party politics. The present Board of Agriculture is hopeless—a Royal Commission could do no worse. A new Board should be formed, not unlike, in constitution, the Royal Agricultural Society, composed of men who farm and have farmed and of those who are engaged in the scientific investigation of agricultural problems. Unless both science and practice are represented in this Board there will be a repetition of the futile practice by which party catch-words are transformed into agricultural policy. Nothing could have been more foolish than the cry of "back to the land," nothing more chimerical than the propaganda of small holdings. These were pushed by the idle hands supported at the Board of Agriculture, with the assistance of a servile press. With very few exceptions, small holdings, other than market gardens, are foredoomed to failure. They require more capital per acre to farm them, they allow but little respite from eternal drudgery, and, *ceteris paribus*, a big farm well farmed is always better than a little farm likewise well farmed. This, too, is apparently the opinion of Sir A. D. Hall, who, on pages 136 and 137 of his *Pilgrimage of British Farming* observes, "The good land was divided into large farms; small holdings did not exist upon it, nor in all probability could they pay the rent that would be demanded, because no small holder could hope to rival the pitch of productiveness to which the land had been raised on the present system."

England will not be turned into an earthly Eden *via* the market garden. That the old adage—

> He who by the plough would thrive
> Himself must either hold or drive,

is not literally true, is obvious, but the truth that underlies it is clear. We must have a Board of Agriculture that knows something about farming, a body of scientific workers who will be ready to assist with criticism, based upon understanding and sympathy for the farmer's difficulties. Such, briefly, are the chief points which seem, in the opinion of the present writer, to require attention in the immediate future. The Art and Science of agriculture are not to be divided: they form a whole, for science is in agriculture just as the salt is in solution, requiring only the reagency of scientific insight. The ultimate decision on any course in agriculture rests on the judgment of the man on the spot. The better equipped his mind, the more likelihood of sound judgment. The future of the country should rest with agriculture.

British Scientific Products Exhibition

The British Science Guild, under the Presidentship of the Rt. Hon. Lord Sydenham, G.C.S.I., G.C.M.G., G.B.E., F.R.S., is organising a comprehensive exhibition of products and appliances of scientific and industrial interest which prior to the war were obtained chiefly from enemy countries but are now produced in the United Kingdom. His Majesty the King has graciously consented to become Patron of the exhibition, and the Marquess of Crewe, K.G., is President. Among the Vice-Presidents are: the Prime Minister; Mr. Winston Churchill, Minister of Munitions; Sir Albert Stanley, President of the Board of Trade; Mr. H. A. L. Fisher, President of the Board of Education; Dr. Addison, Minister of Reconstruction; Lord Moulton; Lord Sydenham; Sir J. J. Thomson, President of the Royal Society; and other distinguished public men.

The exhibition, which will be held at King's College from about the first week in August until the first week in September, will show in the first place products chiefly imported from Germany before the war, but now made in this country; but

it will also illustrate the remarkable developments that have taken place generally in our scientific industries. In many of these, as a matter of fact, Great Britain always excelled, and it is only our national quality of self-depreciation which has prevented the public from appreciating the fact that we were able to export to Germany apparatus and products embodying the highest scientific knowledge and technical skill.

The general scope of the exhibition has been set forth in a preliminary leaflet which has been issued, from which it is noted that the exhibits will include: chemical products; thermal, electrical, and optical appliances; glass, quartz, and refractory materials; photographic apparatus and material, surgical and medical appliances, and papers and textile products.

It is believed that the effect of the exhibition will be to have a most stimulating influence upon scientific and industrial research; and the exhibits, and the demonstrations and lectures that will be given in order to explain them, will undoubtedly bring home to manufacturers, as well as to the general public, the great and growing part that science plays in industry.

Further particulars may be obtained from the Organising Secretary, 82, Victoria Street, London, S.W.1.

Remarks on the Minute of the Executive Committee of the Carnegie Trust for the Universities of Scotland, communicated to the British Science Guild (Prof. Soddy, F.R.S.)

I merely suggested as a reasonable interpretation of the Trust Deed of Mr. Carnegie that the subjects included could be divided into primary and legitimate ancillary, those not included being for the purpose termed illegitimate. The interpretation may or may not be capable of strict defence. By concentrating on this single point, the Executive Committee of the Trust seek to evade the real criticism, fairly summed up and endorsed by the British Science Guild.

Substantial and undenied examples were brought forward of just the same neglect of, contempt for, and unfair discrimination against science which, operating during the past century, mainly through educational channels, has now brought about the position of national insecurity and peril, manifest to all, and which the Founder of the Trust himself stigmatised in the heartiest manner in 1906.

In an address entitled "Modern Needs in Universities," delivered at the opening of the new Carnegie buildings of the Natural Philosophy and Engineering Departments of the University of Edinburgh (*Nature*, 1906, **74**, 648), Mr. Carnegie, after referring to the millions being devoted to science and practical studies and the progressive influences at work in the Universities of America and Canada and of the five principal English cities, continued:

> "Scotland has to keep marching on. The progress of scientific departments in British Universities, considerable as it has recently been, of which the schools we are about to open here to-day are gratifying evidence, yet has not kept pace with the startling progress of science itself and the wonderful discoveries that threaten to revolutionise human conceptions.
>
> "The older branches of learning in our Universities may well welcome the newer branch, cap in hand, not only as the foundation of material progress, but also as one of the very highest agencies in the imaginative domain.
>
> "This mighty force of our day—science—has hitherto been the Cinderella of the sisterhood of knowledge, but the Prince has appeared at last and taken her by the hand. It is now the turn of the elder sisters to greet the once neglected princess. She will more than justify the millions that are now being showered

upon her in the most progressive lands. Thus has the University developed to the present all-embracing type through the successive reigns of scholasticism, theology, and ancient classics, always behind the age, conservative in the highest degree. Science has arisen and established her claim to equality. We have long had the Republic of Letters; we now hail the Republic of Knowledge."

These quotations do not appear to admit of much doubt as to what was the Founder's own view of the purpose of his benefaction. They are refreshingly clear and frank, with a point capable even of penetrating the admit-nothing, dispute-everything defence, which the advocate, unable to face facts, invariably puts up. It is an especially curious commentary upon the cry that it is now the turn of Arts, heard at the last quinquennial distribution, and which, in the University of Aberdeen, has been the interpretation of the gift from the beginning, that what Mr. Carnegie actually said was "it is now the turn of the elder sisters to greet the once neglected princess."

If the legal instrument, which Mr. Carnegie signed to give effect to his intentions, was being administered by a body of men of like mind to himself, in a broad and sympathetic spirit, without any desire to strain it beyond its natural interpretation and twist it to serve ends not intended, legal questions as to its exact meaning could scarcely arise. But if, unfortunately, at any time that should not be the case and it becomes necessary to consider the Deed as an instrument to ensure that the purposes of the Founder, whatever they were, shall be permanently respected without regard to the outlook and sympathies of those administering it, it will be found to be curiously impotent.

Although, among the intentions of the Founder set forth in the preamble of the Trust Deed, only two objects are referred to, the encouragement of scientific study and research and the payment of students' fees, in the operative part, which embraces the Trust Constitution, a new and totally distinct purpose, technical or commercial education, not mentioned in the preamble, is added on to share with scientific study and research, without any specific instruction of the apportionment of the funds for each, which is left to the discretion of the Trustees, the share of the payment of fees purpose alone being strictly defined. So that by concentrating entirely on the new purpose, scientific study and research could be effectively excluded and the first of the two intentions of the Founder frustrated. Whether, however, the Trustees could justify doing this on a narrow construction of the Deed or not, no reasonable beings could claim they were thereby carrying out the declared intentions of the Founder, as set forth in the Trust Deed. Apart, therefore, from a second Mr. Carnegie, willing to take the Deed into Court to get it interpreted, the question of the relative share of the different objects set forth must remain more or less open.

Admitting this, and allowing to the Trustees the most absolute power of discretion, it is still extremely difficult to see how the current uses to which the moneys are being put can be defended.

The clause to which the Executive Committee refer does not exactly or convincingly convey the particular construction they put upon it, and therefore had better be requoted:

"One-half of the net annual income shall be applied towards the improvement and expansion of the Universities of Scotland, in the Faculties of Science and Medicine; also for improving and extending the opportunities for scientific study and research, and for increasing the facilities for acquiring a knowledge of History, Economics, English Literature, and Modern Languages, and such other subjects cognate to a technical or commercial education, as can be brought within the scope of the University curriculum, . . ."

The word *other* shows that the specified Arts subjects are included as cognate to a technical or commercial education, but the Faculties of Science and Medicine and scientific study and research participate on their own merits independently, and not as subserving or ministering to a technical or commercial education. It is recognised by the clause that technical or commercial education can only to a limited extent be brought within the scope of the present University curriculum. That it is technical and commercial education rather than the subjects of the present University curriculum that are to be benefited is shown by the concluding paragraph of Clause B, which deals primarily with the payment of students' fees:

> "In the case of Schools or Institutions in Scotland established to provide Technical or Commercial Education, the Committee may recognise classes which, though outside the present range of the University curriculum, can be accepted as doing work of a University level, and may allow them and the students thereof to participate under both A and B to such an extent as the Committee may from time to time determine."

It is thus natural to inquire in the cases to which I directed attention in which scientific and medical studies had not received a due share of the moneys, whether technical or commercial education has received it. It is only necessary to reiterate a specific instance. In the University of Aberdeen, the scientific and medical subjects: Chemistry—Inorganic, Organic, Physical, Agricultural, Physiological, and Technological; Physics—Mathematical and Experimental; Mathematics; Astronomy; Engineering—Civil, Mechanical, Electrical, and Marine; Geology; Botany; Physiology; Pathology; Bacteriology; Anatomy; Embryology; and the subjects of Medicine and Surgery in their numerous subdivisions, received one endowment for a lectureship in Geology. Whereas in Arts subjects endowments were given for History and Archæology, Political Economy, French, German, Education, and Constitutional Law and History, without regard to whether or not these subjects were taught with reference to the requirements of technical or commercial education.

If this had been done genuinely in the interests of technical or commercial education, and Aberdeen in comparison with the other University centres had in this respect a specially urgent and pressing need, it would be only the discretion of the Trustees that was in dispute. But it was not. It is true that, since the war, the commercial community have realised the need of higher commercial education on a University level. These endowments were allocated long before the war, and the best proof that the needs of commercial education were not the consideration at the time of the allocation is that they are now being considered and a Faculty of Commerce is in process of being brought into existence.

The powers of the Trustees in law may be so great as to enable them to override the claims of both science and technical or commercial education, in order to elevate Arts subjects that can be in any way regarded as cognate to the latter. But, if so, it would be sanguine to expect that any one ever again will provide funds for the improvement and extension of the opportunities for scientific study and research in the Universities of Scotland or in the efficacy of the law to accomplish the object when the funds were provided.

The question being whether the Trust as constituted has in point of fact fulfilled the wishes and intentions of the Founder, the second head of the Executive Committee's reply hardly calls for comment, except in so far as it raises a point of interest. At the date of the minute, January 7, 1918, there were eight original nominated Trustees, and five subsequently appointed. The Rt. Hon.

H. H. Asquith was appointed in 1909, W. J. Dundas, Esq., in 1914, the only three scientific members among the nominated Trustees having been appointed in 1917. There is still one vacancy, caused by the death of Lord Kinnear at the end of 1917, to be filled up, and, when this has been done, it is to be hoped that the statement made, that the vacancies have for the most part been supplied by the appointment of men eminent in various branches of science, may continue to be true.

February 9, 1918.

Postscript added February 21, 1918:

The papers announce that at the Annual Meeting of the Carnegie Trust, held on February 20, 1918, the vacancy above referred to was filled by the appointment of Lord Sands, so that the British Science Guild will draw its own conclusion as to this misleading statement.

F. S.

The Awakening East

Busy as we are nowadays, it is always pleasurable as well as profitable to read those well-conducted magazines, the *New East* (Tokyo) and the *Hindustan Review* (Allahabad). They are, in fact, not second in interest to anything published in this country, while the views are more novel and the atmosphere is fresher. In both of them we find British as well as Japanese or Indian names among the contributors—which gives us the pleasant assurance that in literature as in science there is, after all, only one nation. This is indeed the great ideal of civilisation; let us not puff our chests and crow the one against the other, pretending that our own nationals are so much better than the rest of the world—which is not true. It is a higher wisdom to join hands round the world in order to make it better everywhere. And in these periodicals we can see how rapidly the million-peopled East is now coming up level with the West. A day may arrive when the East shall excel the West in science, art, and literature, even as now the people of the East excel those of the West in manners, gentleness, and many virtues.

We are, therefore, much concerned to read in the *New East* that Russian literature is now fashionable in Japan and that Shakespeare is neglected for Tolstoy. That literature is really based on a lie—that men should prefer their rights to their duties—the fundamental lie of all the so-called radicals, socialists, revolutionaries, nihilists, Bolsheviks, and Sinn Feiners. The lie has ruined Russia and Ireland; it poisons all it touches; it turns men into a kind of arguing apes. Not these doctrines, believe it, will ever better the world. Compare for a moment the benign, all-seeing Shakespeare, and that dishevelled mumbling old crank, Tolstoy!

In the *Hindustan Review* for January (containing many good articles) Rao Bahadur M. V. Kibe, M.A., remarks that "when India came under foreign rule, the goddess Poetry deserted the land." This is scarcely borne out by the fact that the only Nobel prizeman for literature in the British Empire besides Kipling is Tagore; nor by the fact that India is now producing much good work of all kinds. Contrary to what political parrots repeat, nations have often to thank Heaven for having lived under foreign rule. Progress in science and art, not in demagogy, is the true test of advance. The world is sick both of autocrats and democrats, who have brought it to its present pass. The object of government is to organise prosperity—not to feed the fads of rights-mongers. India and Japan

will do well to take warning from Russia and to learn that prosperity and political claptrap cannot exist together.

We have received from Dr. Appiah of Madras a most interesting book, *Swami Ram Tirtha, M.A., His Life and Teachings* (Ganesh & Co., Madras, 2 vols.), full of the Swami's essays (in English). It is astonishing how any one could have written so well in what was to him a foreign language. The philosophy is to us very largely unreal; but reality breaks from the clouds every moment, and the portraits of the author (now dead) assure us that his was a singular and beautiful personality. The book is an unconscious psychological study of modern India—full of mysticism, perhaps, but also of high thought, poetry, and a noble philosophy of duty.

Brains and Bravery

Our press is always loud in deserved praise of the bravery of our soldiers, but seldom says anything of the skill, sagacity or military learning of our generals, so that one would think that these qualities are little valued by our public. In March, however, we were amazed to hear that the Germans had invented a cannon capable of delivering a shell seventy-four miles away, and then a few Britons began to recognise that even brains may be of as much value in war as bravery is. In *Nature* of March 28, Sir G. Greenhill, F.R.S., wrote that "the German gunner has 'wiped the eye' of our artillery science"; and the newspapers, especially the *Morning Post*, commented in strong terms on the slackness of our politicians and officials, educated as most of them are merely on a pabulum of dead languages. This censure is deserved, for every one remarks on the fact that the Germans have led the way in this war, not so much in making new inventions, as in utilising old ones—Zeppelins, submarines, aeroplanes, poison gas—while the allies have only tanks and helmets to their score, and therefore seem to be always on the defensive against the "diabolical new inventions" of the enemy. Yet the British, French and Americans are all much more radically inventive than the plantigrade Germans. Why, then, are they now so behindhand in warlike inventions? Probably because their inventors cannot so easily persuade the authorities to adopt or even to consider their ideas. And why not? Because the Germans have long made a scientific study[1] of war, fully recognise the disconcerting effect of new inventions on the enemy, and do everything possible to exploit them. The British, on the contrary, though they themselves are the most faddy and irrational people in the world, look upon inventors with as much contempt as they bestow upon poets, artists, composers, men of science, tacticians, strategists, and all the rest of the "intellectuals," down even to philosophers. The gentlemen who grow fat and stupid in shops, banks, offices, and parliament naturally despise the lean and keen acolytes of the Goddess of Ideas—are too highly uneducated in Latin grammar to understand their explanations, or even too dull after lunch to hear them: while the masses possess higher ideals in the comedians of the music-hall or the hustings, or in Bounding Bill of the Prairies. As Prince Lichnowsky, recently the German Ambassador in London, has said admiringly of us, "An hospitable house with friendly guests is worth more [in England] than the profoundest scientific knowledge, and a learned man of insignificant appearance and too small means would, in spite of all his

[1] According to a quotation on page 10 of Mr. Wilmore's *The Great Crime* (Hodder & Stoughton), the Germans were publishing 700 books on the science of war annually, to, say, 20 books published in England.

learning, acquire no influence. The Briton hates a bore and a pedant." Unfortunately the Briton now has to pay for his hatred, because learned men of insignificant appearance, and even bores and pedants, sometimes prove astonishingly useful in war-time. An old woman once laughed at Nelson because she thought he had an insignificant appearance. No, the fault lies with the British people as much as with their politicians and officials.

At its best, as in Shakespeare, Newton, Faraday, Darwin, the British intellect can probably give some points to the best found elsewhere; but by every test we attempt the average does not seem high and the lowest is low indeed. Not to dwell on details—what strikes one most is the enormous prevalence of subrationalism or even absolute irrationalism among the public. Few seem to recognise the difference between probability and proof, and nearly every person one meets appears to keep the skeleton of some absurd superstition hidden away in a secret cupboard of his mind—though he may be reasonable enough in other matters. At every dinner party one is sure to find a spiritualist, telepathist, theosophist, ghost-hunter, antivivisectionist, antivaccinationist, antibellumist, universal-rights-monger, immoralist, or some other long-eared exponent of unreason; and we have often thought that the war was due to the fact that the German waiters who overheard dinner-table conversations in England became so impressed with the idiocy of the modern Briton that they urged their imperial master to commence the attack at once before educational reform had time to improve the nation's mentality! But seriously, the same irrationalism permeates all public life and has long paralysed the executive efficiency of the country. It is enough to read the proceedings of Parliament to be convinced that that assembly, though it doubtless contains many able men, is undermined by its numerous fools. We may surmise that most of our great political questions are really impostures, created like Borborygmarol Pills to benefit the makers but not the consumers. We find that they always deal with every one's dues but nobody's duties, and that whichever way the solution may lie, no one will receive any real benefit from it, except perhaps at the expense of others, and, of course, excepting the politicians who create the said questions and live by them. What are we to think of the intelligence of a nation which is so easily deceived by such frauds?

On the other hand, offer to this nation some priceless benefit—a scientific discovery, a new invention, a great work of art—and it will yawn in your face; politicians, officials, editors, publishers, patent-agents, learned societies, and academies will yawn in your face. Why—because it is too much trouble to think hard about anything.

The fact is that for some generations we have been expressly taught by our politicians, pedagogues, priests and prophets (for their own advantage) to prefer "feeling" to intellect and "character" to mind, to "be good and let who will be wise." They have expressly taught our youth to despise science, ridicule art, depreciate all intellectual effort—to make a business of games and a game of business. Under this teaching the nation has bartered away its brains for a mess of political or sentimental pottage, and it is now paying the penalty. "Man may forgive, but Nature never."

Notes and News (D. O. W.)

At the meeting of the Royal Society, held on May 2, the following candidates were elected to the Fellowship of the Society: C. Bolton (Lecturer in Clinical Medicine at University College Hospital Medical School); H. C. H. Carpenter

(Professor of Metallurgy at the Royal School of Mines, South Kensington); T. A. Chapman (Consulting and Analytical Chemist); G. P. L. Conyngham (Superintendent of the Trigonometrical Survey of India since 1912); C. C. Dobell (Lecturer on Zoology at the Imperial College of Science); E. Gold (Superintendent of Statistics, Meteorological Office, London); H. B. Guppy (well known for his work on the geology of the Pacific Islands and on the dispersal of seeds); A. G. Hadcock (Managing Director of Armstrong, Whitworth & Co., Ordnance Engineer); A. V. Hill (Lecturer on Physical Chemistry, Cambridge University); J. C. Irvine (Professor of Chemistry at St. Andrews University); T. Lewis (Physician in Charge of the Cardiographic Department, University College Hospital); S. Ramanujan (Research Student in Mathematics, Cambridge University); A. W. Rogers (Director of the Geological Survey, Union of South Africa); S. Smiles (Honorary Secretary of the Chemical Society and Assistant Professor of Chemistry, University College, London); F. E. Smith (Principal Assistant in the Physics Department at the National Physical Laboratory).

Sir Alfred Keogh, G.C.B., has been appointed to the Order of the Companions of Honour on his retirement from the Director-Generalship of the Army Medical Service.

The Longstaff Medal of the Chemical Society for 1918 has been awarded to Lieut.-Col. A. W. Crossley, C.M.G., for his work on hydroaromatic compounds.

Sir Napier Shaw, who was recently elected a Foreign Honorary Member of the American Academy of Arts and Sciences, Boston, has been appointed scientific adviser to the Government in meteorology for the period of the war. He will be relieved of his administrative duties in view of the important and varied weather problems which now require constant consideration in the direction of the war. The acting director of the Meteorological Office will be Lieut.-Colonel H. G. Lyons, at present keeper of the Science Museum, South Kensington.

The Council of the Royal Society of Edinburgh has awarded the Keith Prize to Mr. R. C. Mossman for his work on the meteorology of the Antarctic regions: and the Neill Prize to Prof. W. H. Lang for his paper on *Rhynia Gwynne-Vaughani* (in collaboration with Dr. Kidston), and for his work on Pteridophytes and Cycads.

The annual gold medal of the Institution of Naval Architects has been awarded to Prof. G. W. Hovgaard, of the Massachusetts Institute of Technology, for his paper on the "Buoyancy and Stability of Submarines."

Prof. Rollin D. Salisbury has been awarded the Helen Culver Gold Medal of the Geographic Society of Chicago.

Dr. W. F. G. Swann, who left Sheffield University to take up a post in the Department of Terrestrial Magnetism at the Carnegie Institution of Washington, has been appointed Professor of Physics at the University of Minnesota.

Among those whose decease has been announced during the last quarter we note with great regret the following: Prof. P. Blaserna, Vice-President of the Senate and Professor of Experimental Physics at the University of Rome; Lord Brassey; Dr. J. Deniker, the French anthropologist; Prof. E. A. Engler, President of the Academy of Science, St. Louis; Dr. G. H. Hinde, F.R.S., the palæontologist; Prof. C. I. Istrati, Professor of Organic Chemistry at the University of Bucharest; Prof. G. A. Lebour, Professor of Geology, Armstrong College, Newcastle; Prof. E. A. Letts, Professor of Chemistry, Queen's University, Belfast; Lieut.-Col. (Dr.) John McCrae, of the Canadian Army Medical Service and McGill University, well known for his work in clinical medicine and as a Canadian poet; Prof. G. Meslin, Director of the Physical Institute of the

University of Montpellier; Sir Swire Smith; Dr. Armand Thevenin of the Sorbonne, President of the Geological Society of France in 1914—died as a result of his experiments on poisonous gases; H. Vidal de la Blache, Professor of Geography at the Sorbonne, and co-Director of the "Annales de Geographie."

It is stated in *Science* that the Carnegie Corporation has presented McGill University with $1,000,000 in recognition of the institution's "devoted service and sacrifice towards Canada's part in the war."

The Annual Meeting of the British Association, arranged to be held at Cardiff next September, has been cancelled.

Further particulars are now available of the British Scientific Products Exhibition which, as announced last quarter, is being organised by the British Science Guild. It is to be held at King's College, London, in July or September, probably during the latter month. It will include all those "products and appliances of scientific and industrial interest which, prior to the war, were obtained chiefly from enemy countries, but are now produced in the United Kingdom," the chief purpose of the Exhibition being "to make clear the necessity of Scientific Research with respect to the application of its results in the arts and industries; and, further, to display to the public and to those intimately concerned how much has been successfully achieved in this regard since the advent of the war in the production of articles of prime importance not only for the home, but also for foreign markets hitherto manufactured in or imported from other countries." It is proposed to arrange the exhibits in the following sections: chemical products and processes, thermal, electrical, photographic and surgical and medical appliances; optical apparatus, glass, quartz, refractories, etc., measuring instruments, papers, textile specialities, and, finally, a section dealing with the exploitation of British natural products. His Majesty the King has graciously extended his patronage to the Exhibition; the Marquess of Crewe is President, and there is a notable list of Vice-Presidents, including the Premier. Prof. R. A. Gregory is Chairman of the Exhibition Committee, and Mr. F. S. Spiers, 82, Victoria Street, S.W.1, the Organising Secretary. The success of the Exhibition will, of course, largely depend on the co-operation of the manufacturing interests involved; it is to be hoped that they will rise to the occasion, more especially as similar exhibitions were held successfully last year both in France and America.

The Treasurers of the Ramsay Memorial Fund announce that they have now collected over £32,000 towards the total of £100,000 for which an appeal was issued. It is anticipated that a considerable sum will be received in the course of the next few months from the Overseas Committees which have been formed in the principal British Dominions, Colonies and Dependencies, and in the principal allied and neutral countries. The first country to complete its collection and to send in its report is Japan, where Prof. Sakurai, of the University of Tokyo, has collected 3927.50 yen (approximately £400). No definite decisions have yet been reached in the countries concerned with regard to the proposed Ramsay Memorial Fellowships to be held in the United Kingdom by chemists trained in the respective countries, but the proposals are arousing considerable interest especially in the British Dominions.

At the meeting of the Optical Society, held on February 14, it was decided to form a new class of membership termed "Fellows." These are to be elected from the members by the Council, which will from time to time decide the necessary qualification. This will always be such as shall, in their opinion, tend to make the Fellowship of the Society an indication of ability in one or other of the branches of optics or the arts and sciences relating thereto. It should be

noted that there is at present no entrance fee on admission to the Society, but a fee of one guinea will be required from members elected after July 1 next.

A movement is in progress for the formation of an Institute of Physicists working on somewhat similar lines to the Institute of Chemists. No definite programme has, so far as is known, yet crystallised out from the discussions which are taking place between the several physical societies. It is understood, however, that the chief objects of the Institute would be (i) to forward the professional status of physicists in general, (ii) to impress upon the industrial world the importance of the works physicist, and (iii) to form a connecting link between the societies dealing with subjects of a physical character.

The Faraday Society arranged a meeting to consider the "Co-ordination of Scientific Publication" on May 7 last. The discussion was opened by Sir Robert Hadfield, President of the Society and member of the Sub-Committee appointed by the Conjoint Board of Scientific Societies to deal with the "Overlapping between Scientific Societies." Among others who spoke were Prof. Schuster, Dr. R. Mond, Mr. Longridge (President of the Institute of Mechanical Engineers) and Mr. Wordingham (President of the Institute of Electrical Engineers). Sir Robert Hadfield's chief suggestion was that there should be a Central Board (such as the Conjoint Board) appointed to receive all scientific papers and to allot them for reading and discussion to the society to which they would be of most interest. In addition the Board should circularise other societies likely to be interested in order that their members might be aware of what had been done and enabled to attend and take part in the discussion if they so desired. This plan would, of course, involve some degree of federation between all the larger societies; a federation which was evidently regarded very favourably by those present at the meeting. It has indeed already taken place in Germany, where a Union of Technical and Scientific Societies, with a roll of some 60,000 members, has been formed more especially to cope to the best advantage with the problems which must arise at the end of the war. In New York also the United Engineering Societies have a central building and library, provided by the generosity of Andrew Carnegie, where the several societies meet for discussions, and where they are brought into closer contact than is possible with the decentralisation which obtains here. Nor should the federation be limited to the United Kingdom alone. The great societies should have Colonial representatives, particularly those dealing with problems of an industrial character. In pre-war days the Iron and Steel Institute had a representative of the German Empire, which was thus kept in touch with English research, but no representative from our own Dominions. With a federation of this kind it might be possible to maintain a common building (*e.g.*, an enlarged Burlington House) for meetings and to house a joint library which should contain, in particular, all the publications referred to in the International Catalogue. Several speakers dilated on this idea, Dr. Mond suggesting that it should have a staff of translators competent to provide complete translations of papers written in the more difficult languages (*e.g.*, Russian or Japanese) when they were required; while Mr. Longridge went further in desiring a College of Librarians; men able to discuss research with inquirers and not merely to put them on the track of past work, but also to inform them of the work then in actual progress! Less utopian was the demand for uniformity in publication. It is most desirable that all Proceedings, Transactions, etc., should be printed on the same sized paper and in the same type so that collected papers on any one subject may be bound together. The scheme for the pooling of papers was opposed by the Institutions on the ground that they

awarded prizes for the best papers submitted to them and that, under the scheme, this incentive to research might disappear. Obviously, however, this difficulty might easily be overcome if each society retained the right to print any papers sent to them irrespective of their ultimate fate at the hands of the Board. A more serious objection is that a paper is usually written for a particular class of reader. A treatment suitable for the Physical Society would probably not be best for the Iron and Steel Institute. Having regard to this fact it seems probable that a Central Board would find its most important function in issuing a weekly or monthly list of forthcoming papers with intelligible abstracts, as suggested by Prof. Schuster.

Since Tyndall made his famous experiments in 1872 it has been assumed that the light scattered by dust-free air is too faint to be observable with the small thicknesses which can be used in the laboratory; although it has been well established that air molecules are competent to produce such scattering. Prof. Strutt has, however, recently succeeded in demonstrating the effect experimentally. He states that the chief essentials for success are to avoid, as far as possible, stray light diffused from the walls of the vessel used and to observe the beam transversely against the blackest possible background. He employed a cross-shaped vessel made of brass tubing of 1½ in. diameter, painted dead black inside. A beam from an arc was directed down one cylinder, being admitted through a quartz window, while one-half of the other cylinder formed a black cave against which the beam was viewed through a glass plate covering the end of the other half. The air was dried and then filtered through a tube 4 ft. long filled with cotton wool. It was forced into the vessel under pressure, so that while dust-free air might leak out, ordinary air could not leak in. Viewed as described there was a blue track along the beam which, though much fainter than the track seen with ordinary air, was visible without difficulty when the eyes had been rested in the dark. Several tests were applied to show that the effect observed was not due to residual dust. No change was produced by further filtering, and Aitkens' method for counting dust particles failed to reveal presence of a single one. The blue track was examined spectroscopically to eliminate the possibility of the effect being due to a fluorescence of the air. A two-hours' exposure with the arc source brought out faintly the cyanogen band (λ388), which is photographically the most conspicuous feature of the arc spectrum; while a three-days' exposure with a quartz-mercury lamp showed only the mercury lines. Other gases were used in the vessel in place of air. With oxygen the appearance was indistinguishable from that with air, with carbon dioxide the intensity was greater than with air, and with hydrogen very faint indeed. The scattered light is almost completely polarised in the manner indicated by theory. The experiments described are preliminary to the quantitative measurements which are now in progress.

The state of affairs in Russia makes the discovery of platinum in the Serrania de Ronda, in the south of Spain, announced in a special memoir issued by the Geological Institute of Spain, specially interesting. The rocks in this region were studied because of their similarity to the platinum-bearing rocks in the Ural Mountains. The Ural deposits yielded in pre-war days about 300,000 oz. (troy) per annum—about 95 per cent. of the world's yearly output. In 1916 this figure had fallen to 86,000 oz. Platinum has also been found in some quartz deposits in the Ober Rosbach district of the Taunus Mountains (Germany), and these deposits are being worked (*Nature*, April 25, 1918). The chief source of the Entente supplies is now presumably the mines in Colombia, where the metal was first discovered in 1735. Their output is comparatively small: in 1916 it was 25,000 oz.,

rather more than double what it was before the war. Other possible sources of supply are the river gravels in South-East Borneo, where further prospecting is required, and various districts in North America, more especially Sudbury, Ontario, and the stream placers in Alaska, which are being investigated by the U.S. Geological Survey. Considerable activity is also reported in the Similkameen and Tulameen districts in British Columbia, which are the chief localities of platinum production in Canada (*Australasian Manufacturer*, December 8, 1917). New South Wales has produced some 2,000 oz. in the course of the last five years. The demand for the metal has, of course, increased enormously during the war on account of its use in the contact process for the manufacture of sulphuric acid, in aeroplane engines, etc. The average price in 1908 was £5 2*s.* 6*d.* per oz. troy; in New York in 1916 the price rose to £20. Wherever possible alloys have been introduced to take its place. "Palau" is a gold-iridium alloy used for laboratory purposes; "Rhotanium," a gold-palladium alloy used for setting jewels and also in the laboratory, is attacked only by hot concentrated nitric acid; "Amaloy," an alloy of nickel, chromium, tungsten, etc., is used in dental work and for surgical instruments on account of the resistance it offers to atmospheric corrosion (*Nature*, February 21, 1918).

In the Annual Address to the Ohio Academy of Science in April 1917, Prof. J. R. Withrow mentioned another class of alloys which resists the action of sulphuric and nitric acids of all strengths. These are the silicon-iron alloys sold under various trade names, such as Durion, Tantiron, and Ironac. Though not possessing quite the resistance of stoneware, they have made possible the huge increase in the output of nitric acid necessitated by the war. Pipes and castings for fittings can be made quickly and replaced at once, so that the stills can be made to carry heavier charges and be run at greater speeds than before, any resulting damage being made good quite easily. In the same address, which was entitled "The Relation of War to Chemistry in America," the speaker showed how the falling off in (published) chemical research was reflected in the number of *Chemical Abstracts* issued by the American Chemical Society. The abstracts are collated from some 600 journals published in all parts of the world. The numbers are as follows: 1913, 25,971; 1914, 24,388; 1915, 18,449; 1916, 15,784, and these figures, of course, refer to the period before the United States entered the war. Forty-eight journals had apparently ceased publication: England and Austria 1 each; Belgium 7; Germany 8; France 31.

We have received three *Bulletins* (Nos. 4, 5, and 6) from the Advisory Council of Science and Industry of the Commonwealth of Australia. The first deals with the "Factors influencing Gold Deposition in the Bendigo Goldfield." It contains a very complete account of the structure of the reefs and of the theories of the causes of the gold shoots, but it has not been possible to draw any very definite conclusions to help the prospector. The second, on "Problems of Wheat Storage," also fails to reach any very helpful results. The Committee appointed to investigate the problem has made tests on the quicklime process for treating damaged wheat. It is found that, when freshly burnt hot quicklime is mixed with the wheat, the bacteria on the outer layers of the grains are considerably reduced in number and the surface is cleansed from organic nitrogenous compounds, so that an appreciable improvement is effected. The multiplication of insect pests is largely determined by conditions of temperature and moisture. For example, grain is apparently weevil-proof if it contains less than 10 per cent. of moisture. The Committee recommend that this and other points should be investigated by some one able to devote his whole attention to the work, and it is reported in the daily

press that the Commonwealth Government is endeavouring to obtain the services of Prof. Maxwell Lefroy, for one year, for this purpose.

Bulletin No. 6 deals with the question of Power-Alcohol, and it will be convenient here to consider at the same time a most excellent monograph written by Mr. Robert N. Tweedy, entitled "Industrial Alcohol" (Co-operative Reference Library, Plunkett House, Dublin, 1917. Price 1*s.* net). The subject is of great importance for two reasons: first, because the world is using the existing limited supply of mineral oils faster than their rate of production, and secondly, because the production of denatured alcohol is an important and profitable industry which, owing to the blindness of officialdom, has been almost entirely neglected in this country. The problem naturally falls into two parts—that of production and denaturation, and that of utilisation. In Germany the potato is the chief source of the supply, and in 1913 some 70,000,000 gallons of alcohol (90 per cent. absolute) were distilled from potatoes, 3,000,000 tons being used for the purpose out of a total crop of 50,000,000 tons grown on over 8,000,000 acres of land. The industry in that country owes its success in part to a bonus on alcohol used for other than beverage purposes (which is provided by a tax levied on the latter), and in part to efficient co-operation between the farm, distillery, and market. In France the chief raw material is beet. In 1912 18,000,000 gallons of denatured spirit were produced; but the industry was handicapped by the onerous regulations governing the denaturation. These regulations made the spirit expensive and unsuitable for lighting and internal-combustion engines. Apparently they have been withdrawn since the war began, and it is the intention of the French Government to make alcohol a cheap national motor fuel. Even in the U.S.A. steps are being taken to develop the alcohol industry: in the United Kingdom it hardly exists. In 1914 only 7·7 million gallons of denatured spirit were manufactured, and there is no record that any of it was used for light, heat or power. In 1915 we imported 120,000,000 gallons of petrol, and the equivalent quantity of alcohol might easily have been produced at home. Five million tons of potatoes only would be necessary, and 600,000 acres, properly cultivated to yield 12 tons per acre, would have provided the crop for food and for the alcohol. Beet could be grown instead if it were desirable. The Australian Committee has gone very completely into all the possible sources of alcohol in that Continent, and concludes that, while potatoes would be too expensive, large crops of Sorghum (*S. saccharatum*) could probably be grown yielding 100–180 gallons per acre; while Cassava, which gives a higher yield, might be equally suitable. Synthetic alcohol is possible, and one process is being worked successfully by a Swiss hydro-electric company, which is able to sell pure spirit at 1*s.* 8*d.* per Imperial gallon.

On the Continent the price of the spirit compares favourably with coal, gas and oil, so that it is used for domestic purposes in addition to its use in various industrial operations. For power purposes a special engine is required if the best results are to be obtained, though petrol engines can be adapted to run on alcohol as was demonstrated in Germany early in the war, when the Russians had overrun Galicia and shortage of petrol was threatening disaster to the German transport service. With petrol at 40·5*d.* per gallon and alcohol at 30*d.* the cost per horse-power hour is the same using the same engine and assuming that with petrol it has a thermal efficiency of 20 per cent., while when altered for alcohol it has an efficiency of 22·5 per cent. Specially designed alcohol engines will give 30 per cent. or more with a corresponding decrease in fuel cost. The increased thermal efficiency is due to the increased compressions (as high as 180–200 lb. per sq. in.) which can be employed without danger of pre-ignition. With petrol 80 lb. per

sq. in. is about the limit. After the war petrol will hardly go below 2*s*. per gallon, which is about the pre-war cost of alcohol, but it is made quite clear in Mr. Tweedy's book that alcohol could be sold at a much lower price than this if the industry was handled sympathetically and not overburdened by excise restrictions. The advantages of alcohol as a fuel, as compared with petrol, are (*a*) smoother running, (*b*) absence of carbonisation, (*c*) absence of unpleasant exhaust, (*d*) greater safety from fire owing to its miscibility with water. On the other hand, it is not suitable for high-speed engines and will not start from the cold, so that either an auxiliary petrol tank and carburettor are required or some heating device must be used. During the transition stage from petrol to alcohol it may be desirable to overcome the difficulty by mixing alcohol with some other liquid fuel, such as benzine or sulphuric ether. This would both overcome the difficulty in starting from the cold and also enable existing engines to be used without alteration.

ESSAYS

SCIENCE AND INDUSTRY (Prof. R. A. Gregory)

SCIENCE provides the means for great industrial advances long before they are actually used. Vested interests and political ineptitude always place obstacles in the way of national progress. Mr. A. A. Campbell Swinton, Chairman of the Council of the Royal Society of Arts, pointed out recently in an address to the Society that in 1882 this country was as far advanced in everything pertaining to the application of electricity as any other country in the world ; but the development of this new scientific industry was then impeded by Parliamentary interference, and for six years the supply of electric power was practically at a standstill. Other countries went ahead until legislative amendments in 1888 enabled the electrical industry to make a start here under reasonable conditions. The application of mechanical power to road locomotion was similarly retarded for fifty years by hostile legislation. "When, moreover, a new beginning was made," says Mr. Swinton, referring to automobilism, "the first start did not take place in England, its original home, where it was prohibited by law, but in France, where legislation was more enlightened. In this way, owing entirely to the politicians, we lost an opportunity of becoming pioneers throughout the world of a completely new and what proved to be a gigantic industry, which might have brought to our manufacturers much wealth and to the working-classes much lucrative employment."

A Sub-Committee of the Reconstruction Committee has lately submitted to the Prime Minister a report in which the institution of a national system of electric power distribution in this country is recommended. The scheme follows lines laid down by Lord Kelvin forty years ago. Speaking at the Institution of Civil Engineers in 1878, Lord Kelvin said: "The economical and engineering moral of the theory appeared to be that towns henceforth would be lighted by coal burnt at the pit's mouth, where it was cheapest. The carriage expense of electricity was nothing, while that of coal was sometimes the greater part of its cost. The dross at the pit's mouth (which formerly was wasted) could be used for working dynamo-engines of the most economical kind, and in that way the illumination of great towns would be reduced to a small fraction of the present expense. . . . It might be expected that, before long, towns would be illuminated at night by an electric light produced at the pit's mouth or by a distant waterfall. The power transmissible by the machines was not simply sufficient for sewing-machines and turning lathes, but, by putting together a sufficient number, any amount of horse-power might be developed."

Had Lord Kelvin's advice been taken, the country would have saved many millions of tons of coal per annum, and there would have been enormous developments in the application of electric energy to industry. The Reconstruction Sub-Committee proposes to divide the country into sixteen districts, in each of which there would be several large inter-connected super-stations for generating electric power. The stations would be either near the pit's mouth, where coal

dross could be used for engines of the most economical type, or in places where plenty of condensing water is available, where coal transport is cheap, and where they would be near the centre of gravity of the probable demand. The Sub-Committee has no difficulty in showing that "the present system of electric power distribution throughout the country, which is undertaken by over 600 authorities in as many separate districts, is technically wrong and commercially uneconomical." From the point of view of practical engineering, there is no reason why early steps should not be taken to supersede this inefficient system, but opposition must be expected from the vested interests of individual capitalists and from local authorities possessing electric undertakings or profiting by the rates payable by supply stations. It requires a broad mind and an unusual spirit to be able to think nationally, as it does to think Imperially, yet these qualities are always essential to the establishment of schemes designed for the common good.

The advantages to be derived from such a system of national power supply are so great that nothing should be permitted to stand in the way of their realisation. The Sub-Committee points out that large quantities of electrical power will be required for the development and carrying on of new processes not at present undertaken in this country. In addition to industrial applications, a national system of electric power supply would greatly facilitate the electrification of railways with its attendant advantages and bring within reach of the community as a whole the great benefits of an increase in the use of electricity for domestic purposes.

The application of the electric furnace to the metallurgy of iron and steel, and the use of electro-chemical methods for the production of many substances of industrial importance, have led to very remarkable developments during the past few years; and the prospects for the future are most promising. Beginning with the production of various ferro-alloys which cannot be made in fuel-fired furnaces, and the highest classes of carbon and alloy tool steels which compete successfully with crucible steels, the electric furnace has become within the last few years an essential element in steel manufacture. Owing to its high temperature, the refining of steel can be carried to a further stage than is possible with the Bessemer and open-hearth furnaces, with the result that a purer and more trustworthy metal is produced. This is especially the case with rail steel; and railway companies are prepared to pay considerably more for the increased trustworthiness.

Early in the nineteenth century Sir Humphry Davy succeeded in decomposing caustic potash and caustic soda by means of the electric current, and in obtaining from them the metals potassium and sodium. The experiment was made to settle a disputed question in pure chemistry, but it was the starting-point of an immense industry. At the present time, the chief process of commercial importance for making sodium is that of electrolysis of caustic soda, all chemical methods of manufacturing the metal being superseded by it. Caustic soda itself, together with chlorine for bleaching purposes, is obtained from common salt by electrolytic methods on a large scale; and its manufacture is an important industry.

In 1833 Faraday obtained the metal magnesium from a compound of the element by means of electrolysis; and now the magnesium ribbon and powder used for flash-light and other purposes are almost entirely made by the same method. But the most important application of electricity to industrial chemistry is the electrolytic production of that most useful metal, aluminium, which is destined to compete with iron and steel in its importance. Aluminium is now manufactured exclusively by electrolysis of a fused mineral containing it, though a

few years ago it was obtained wholly by purely chemical methods. Unlike the examples already mentioned, the actual process of producing aluminium by electrolysis was not derived directly from a scientific laboratory, yet it and all other electrolytic methods would never have come into being but for the discovery by men of science of the chemical effects of the electric current.

Industrial chemistry has, in fact, been revolutionised by the application of electrical methods; and the foundation of the new branch was laid chiefly by the genius and research of Davy and Faraday, being practically based on the laws enunciated by the latter. Metals, such as copper and iron, are obtained in the highest state of purity by electrolysis; in the United States alone, more than twenty million pounds' worth of copper are electrically refined every year. Silver, gold, and lead are also refined on a large scale by electrical methods. Electro-plating with gold, silver, nickel and other metals; electrotyping, which is used in every printing works to obtain copies of type and engravers' blocks; the electrolytic reproduction of medals and similar articles, and a hundred other commercial uses have been found for methods which when first discovered were considered to be of interest only to the world of science.

The manufacture of aluminium by electrolysis is now an established industry in most of the chief countries of Europe as well as in the United States. From the position of a rare metal, aluminium has risen to a yearly tonnage exceeded only by iron, lead, copper, zinc, and tin; and it may confidently be expected to take a higher rank in the near future. Other products of applied electric power are carborundum and alundum. Early this year announcement was made that satisfactory results in the electrolytic production of zinc from Australian ore and concentrates has been obtained from a plant erected at Risden, Tasmania. The plant has a daily capacity of fifteen tons, which, it is said, can be increased tenfold, and the necessary electric power is obtained from a hydro-electric installation belonging to the State. At present Prof. H. C. H. Carpenter is, however, of the opinion that neither electrolytic zinc nor electro-thermally distilled zinc can compete commercially in this country with that obtained by distillation with coal in externally-fired retorts heated by gas.

Owing to the relative absence of water-power, Great Britain has not been to the fore in electro-chemical and electro-thermal development; but there is good reason for hope that the future will see much greater advances in this direction than would have been anticipated a few years ago. It is becoming more evident every year that abundant water-power is not an indispensable adjunct to electro-chemica enterprises, and that other sources of power may possess decided advantages. The Report on Electric Power Supply points out a practicable way in which advance in this direction may be secured.

A Chinese proverb states that he who holds the iron of the world will rule the world. This is only a half-truth; for China itself has probably as large deposits of iron ore as any country in the world, but it has not the scientific knowledge required to make the best use of them. The spirit of the proverb, however, was probably one of the main causes of the origin of the present war, for the aim of the great German ironmasters is to obtain a monoply of that vast deposit of iron ore known as "Minette," which covers so large an area of Central Europe. In 1870 the contribution of Germany to the world's output of steel was practically nothing. As a result of the treaty by which she obtained the province of Alsace and the greater part of Lorraine, she secured a considerable proportion of the Minette iron ore deposits which had previously been entirely in French territory; and the avowed intention is to increase her holding of the French ore-fields, which

are richer in iron than those on the German side of the frontier. In 1870 Germany's contribution to the world's output of steel was 0·2 million tons, and that of the United Kingdom 0·3 million tons; in 1910 it was 13·5 millions, while ours had increased to less than half that amount. As Prof. Carpenter has pointed out, Germany could never have built up her immense industry in iron and steel, and the present war would have been impossible, had not two Englishmen, Sidney Thomas and Percy Gilchrist, invented in 1878 the basic-Bessemer process by which the phosphoric iron ores, which had previously been considered to be worthless, were rendered available for steel production, while incidentally the basic lining of the converter, when ground into fine powder after use, forms the valuable agricultural fertiliser known as basic slag. The statues of Thomas and Gilchrist erected in Dusseldorf show that German steel manufacturers recognise the debt they owe to these two men.

As with the basic process, the combination of scientific knowledge with practical purpose has resulted in many other developments of modern metallurgy. Rare elements which were formerly scarcely known by name outside chemical laboratories are now used as essential constitutents of alloys of the highest industrial value. Ten years' research by Sir Robert Hadfield upon the influence which different percentages of manganese exert upon the properties of steel led to the discovery of that remarkable metal, manganese steel, which has shown the way to the production of dozens of other alloys possessing qualities required by the arts of peace as well as the arts of war. To elements quite unknown a few years ago, or regarded merely as chemical and mineral curiosities, we owe not only the new steel alloys which now play so large a part in ordnance, naval construction, and engineering, but also the incandescent gas mantle, the metal filament electric lamp, and many other industrial products.

The raw materials of every industry are given additional values by the processes of manufacture to which they are submitted. If the cost of the material used is deducted from the selling value of the article at the factory, we get what is known as the net output, which represents the value added by industry. By dividing this net output by the number of workers engaged in producing it, the net output per head is obtained. Now, according to the Census of Production, 1907, the average net output per worker in the industries of the United Kingdom was £102 per annum. This sum includes all establishment charges and interest on capital as well as the wages of the worker, so that probably the average wage should not be taken as more than about £1 a week.

The problem is how to increase this amount and the net output of the worker of which it is a part. It would be no advantage to increase the value of the net output by raising the selling price of the goods in the home markets, and international competition prevents this being done in other markets. Two other courses are possible—namely, (1) the individual worker works harder or longer, or (2) he is enabled to produce more by means of machinery. A table given in the Census of Production, and reproduced in the recent Report on Electric Power Supply in Great Britain, shows conclusively that in practically all trades the average net output per worker increases with the increase of horse-power of engines in factories per hundred persons employed. In other words, the average wage goes up with the use made of machinery. Individual workers may be thrown out of employment by the introduction of new machines, and if a very large number of men throughout the country remained unemployed because of the use of machinery, it would obviously not be a cause of much national satisfaction to know that the net output per head of the industries using the machinery

was higher than when the manual labour was used. Experience shows, however, that taking the country as a whole its prosperity is measured by the output per head; and this is increased by the use of machinery. Upon this point the Report to which reference has been made states:

"In the United States the amount of power used per worker is 56 per cent. more than in the United Kingdom—if we eliminate workers in trades where the use of power is limited, or even impossible, we shall probably find that in the U.S.A. the use of power, where it *can* be used, is nearly double what it is here. On the other hand, not only are the standard rates of wages higher in the U.S.A., but living conditions are better. There is little doubt that in the U.S.A. the average purchasing power of the individual is above what it is in this country, and that this is largely due to the more extensive use of power which increases the individual's earning capacity. The best cure for low wages is more motive power. Or, from the manufacturer's point of view, the only offset against the increasing cost of labour is the more extensive use of motive power. Thus, the solution of the workman's problem, and also that of his employer, is the same—viz. the greatest possible use of power. Hence, the growing importance of having available an adequate and cheap supply of power produced with the greatest economy of fuel."

Modern industry requires the use of a greater number of skilled research workers and of men with technical knowledge for responsible positions. The Census of Production shows that the net annual output per head is generally highest in those industries which employ the highest proportion of persons receiving salaries as distinct from wages; and it diminishes in passing to industries where the percentage of wage-earning employees increases. Thus, taking the nine leading industries, but not including coal-mining, the highest annual output per head is £185 in the chemical industries, where 12 per cent. of the persons employed receive salaries and 88 per cent. wages. Next in order of annual output per head and proportion of salaried employees come iron and steel factories (£118), and engineering factories, including electrical engineering (£108), and at the bottom of the list are the jute, linen, and hemp factories, with a net output of £61 only, the percentage of wage-earners being 98, of salaried persons 2. These figures indicate that the employment of skilled technologists means increased productivity; and they point to the importance of improved training of artisans in technical schools. If research methods are to be more generally applied to industries, greater skill and accuracy will be required from the general body of workers, so that it is not merely the duty of the universities and colleges to supply highly trained research workers, but the technical schools have also the important task of educating the artisan for the new type of work required under the new conditions.

The way to increase the number of highly skilled technologists is to make their position and prospects better than they have been. Many employers still express their preference for the so-called practically trained man over the man with scientific training, whereas in other countries the college-trained technologist finds ready acceptance in all branches of industry. Whether we like it or not we must face the fact that much of the commerce and the manufacture of the modern world demand the leadership of highly trained, widely informed men; and that these men must be forthcoming if we are to be able to take a leading place among the nations of the world. In the Report of the Board of Education a couple of years ago, it was pointed out that "the provision for full-time education in applied science is regrettably small in bulk compared with the industrial development of the country." We have, in fact, in the United Kingdom only about three

thousand full-time day students of technology to compare with the 17,000 students in the German Technical High Schools, not to reckon nearly 60,000 students in the German Universities. We do not want to adopt here the dominating State influence upon education which exists in Germany, but we must acknowledge that the great advance made in that country in the industrial sciences, and the increase of wealth thus obtained, are due to the use made of highly trained technologists.

These creators of new industries based upon science need, however, intelligent and contented artisans to carry out their developments. Though manual and mental workers are often considered to belong to different classes, and an indefensible social distinction is usually made between them, no such separation can be recognised in scientific fields, where fine manipulation, and skill in the use of instruments, are frequently as valuable as fertility in idea and ingenuity in design. Industrial advance seems, indeed, to depend upon three main factors, in all of which brain and hand are related, though in different degrees. First there is the creative investigator whose work reveals new properties and relationships; then comes the inventor or industrial researcher who seeks to apply knowledge to useful ends; and when a practical process or machine has been devised, the artisan is needed to make it fulfil its technical purpose. Each of these three classes has an essential place in national polity; and the correlation of their interests and activities must be the chief aim of all schemes of reconstruction.

Several recent reports and manifestoes are concerned with the combination of these different groups. The Interim Report on Joint Standing Industrial Councils submitted to the Prime Minister by a sub-committee of the Reconstruction Committee, and referred to as the "Whitley" report, suggests the establishment of district and national councils, which should deal, among other matters, with technical education and training and with industrial research and the full utilisation of its results. The draft constitution of the new Labour Party, submitted to the Nottingham Conference on January 23, has in the forefront of the party objects, "to secure for the producers by hand or by brain the full fruits of their industry"; and the secretary of the party, the Right Hon. Arthur Henderson, in reply to an inquiry from the editor of *Nature*, has stated that "the term 'producers by hand or by brain' would include scientific workers if they are prepared to accept our constitution and programme." As scientific workers were not invited to co-operate in the production of the new constitution and programme, Mr. Henderson's offer does not amount to much; for it is obvious that any party would be prepared to accept them on the same terms. Scientific men will certainly not be disposed to support any system of party politics, and they would be more likely to take part in the new programme if it were made clear that the Labour Party signified a federation or organisation in which brain and hand were united for common welfare rather than for the narrow interests of one particular section of the country's life. We do not want a return to the system of party government; but if all the workers by brain, as well as by hand, combine into one group for the government of the State on efficient lines, party government will practically cease, and the aim of all will be to promote the interests of the organised community and at the same time preserve the freedom of the individual. This—the essential problem of government—should be the object of all legislation: and it needs the closest combination of brain and hand for its solution.

ESSAY-REVIEWS

SOME NEW ASPECTS OF COLLOID CHEMISTRY, by INGVAR JÖRGENSEN, Cand. Phil., D.I.C., on: **Proteinstudier,** I.–V., by Prof. S. P. L. SÖRENSEN, Meddelelser fra Carlsberg Laboratoriet. Vol. 12. [Pp. 364.] (H. Hagerup, Copenhagen, 1917. Price 8 kr. 25 öre. English edition in the Press.)

COLLOID chemistry is one of those unfortunate subjects in which textbooks appear to be in advance of research. A perusal of recent continental works (I. Wo. Ostwald: *Grundriss der Kolloidchemie*, Dresden, 1909. II. *An Introduction to Theoretical and Applied Colloid Chemistry*, translated by Martin H. Fischer; London: Chapman & Hall, 1917. III. H. Freundlich: *Kapillarchemie*, Leipzig, 1909. IV. *Kapillarchemie und Physiologie*, Leipzig, 1914) gives the impression that progress in the subject has been hindered by premature classification and organisation. Too much stress has been laid on the colloidal condition; "colloid chemistry is not the science of colloidal substances, but the science of the colloidal condition of substances" (Wo. Ostwald), and the tendency has been to generalise from experiments with suspensoids. Prof. Sörensen's researches on the proteins constitute a much-needed corrective to these tendencies of the German school.

The fundamental difference between suspensoids and emulsoids lies, according to Sörensen, in the relation between the disperse phase and the dispersion medium in the two classes. The properties of typical suspensoids depend essentially on the surface relations between dispersion medium and disperse phase. The same phenomena are met with also in emulsoids, but here surface action is only of secondary importance, because the disperse phase of emulsoids reacts at the same time in a purely chemical way with the dispersion medium. The fundamental importance of Sörensen's work lies in the way in which it is shown that the experimental results obtained with emulsoids can be interpreted by exactly the same laws which hold for true solutions. The degree of dispersion probably does not play the all-important part which for example Wo. Ostwald assumes for it, and in any case it cannot give a satisfactory explanation of the difference between suspensoids and emulsoids. The explanation that this difference should be due to a solid disperse phase in suspensoids and a liquid disperse phase in emulsoids, is insufficient in view of the fact that colloidal systems are known with a suspensoid character but with the disperse phase liquid (G. S. Walpole, *Biochem. Journ.* 8, 1914, p. 170). It is possible that in emulsoids the degree of dispersion plays an important part in regard to whether the capillary or purely chemical properties are dominant. If the degree of dispersion is small the capillary properties may play an important part, but in emulsoids with a high degree of dispersion the purely chemical properties are dominant and determine the character of the system. In the opinion of Sörensen it is thus impossible to draw a sharp line of distinction between true solutions, emulsoids, and suspensoids. The emulsoids in a way occupy an intermediate position between true solutions and suspensoids. Suspensoids are characterised by having a viscosity not very different from that of the pure dispersion medium. There is a distinct difference of charge between disperse

phase and dispersion medium ; only a small quantity of electrolyte is required for coagulating, and this coagulation, as a rule, is irreversible. Emulsoids, on the other hand, have great internal friction and the property of foam formation ; considerable quantities of electrolytes are required for coagulation, which, as a rule, is reversible.

The new method in colloid chemistry which is indicated by Sörensen's researches is the close study of a single substance from colloid-chemical, physical-chemical, and also from purely chemical standpoint. Although Sörensen's work in some respects is a continuation of Pauli's, and often runs parallel with some of his researches on proteins, he has obtained his important results by confining himself to the detailed study of one substance, namely, egg-albumen, while Pauli worked with a large number of substances.

Sörensen expresses his disagreement with Wo. Ostwald, who, in the second edition of *Grundriss der Kolloidchemie*, issues a warning against drawing parallels between the conditions in true and in colloidal solutions. It is absolutely essential for the understanding of the character of emulsoid systems to compare the properties of true and colloidal solutions, and Sorensen emphasises that the study of true solutions may benefit from investigations on a well-defined protein solution, because methods may be used in the latter case, *e.g.* the employment of semi-permeable membranes, which can only be used in exceptional cases in investigations on true solutions. The proof of the applicability of Gibbs' phase-rule to the equilibrium condition between crystalline egg-albumen and its mother liquid, and the elaboration of Donnan's formulæ in respect to the osmotic pressure of egg-albumen solutions containing salt, more than justifies the claim made by Sörensen.

In this review it is impossible to go into all the details of the research dealt with in Sörensen's work. His book is divided into five chapters. Chapter I. describes the methods for preparing the material and tests for its purity. Chapter II. deals with acid and base binding power of egg-albumen, particularly the binding power with regard to sulphuric acid at various ammonium-sulphate concentrations. Chapter III. deals with the crystallising out of egg-albumen by addition of ammonium sulphate. Chapter IV. treats of the equilibrium condition between the crystallised egg-albumen and the surrounding mother liquid. Chapter V. discusses the question of the osmotic pressure of egg-albumen solutions, and its relation to the composition of the solution.

The success of Sorensen's researches is due in the first instance to his using a well-defined material, which allows the reproduction of the same conditions in all series of experiments. Lack of realisation of this important point has been the cause of many uncertain and contradictory results obtained in earlier investigations. The egg-albumen which has been used in many researches has not been purified to any great extent, and consequently the results obtained are of little value. Even if the egg-albumen has been purified by recrystallisation and subsequent dialysis the material is still undefined, because the dialysis is never complete, and the quantity of electrolytes which remain behind to a great extent determines the properties of the egg-albumen, and their amount must therefore be determined quantitatively.

The method used for crystallisation is essentially the same as that used by F. G. Hopkins and S. N. Pinkus (*Journ. of Physiol.* vol. 23, p. 130, 1898). By crystallisation and washing are removed in the first case ash compounds and "mucoids" (nitrogen substances which are not coagulable) and "conalbumen" (coagulable but non-crystallisable nitrogen-containing substances). The trace of ash which pure egg-albumen leaves on incineration is not due to impurities but to

the phosphorus content of the egg-albumen (E. G. Willcock and W. B. Hardy, *Proc. Chem. Phil. Soc.* vol. 14, p. 119, 1907). Further purification was effected by a number of recrystallisations, generally six. The ammonium sulphate used in the crystallisation is removed by dialysis. Egg-albumen as an amphoteric compound binds both acid and basic compounds, particularly the latter, as egg-albumen is more acid than basic. Consequently it is impossible by simple dialysis to remove every trace of the compounds of ammonium sulphate, particularly the ammonia. Sörensen therefore allows the dialysis to progress so that all sulphuric acid is removed, which can be done by addition of ammonia at intervals whereby the sulphuric acid bound to the egg-albumen is transformed into ammonium sulphate, which dialyses away. In this way all sulphuric acid can be completely removed, but it is impossible to remove the ammonia completely by dialysis, because the albumen combines with the ammonia and only the ammonia in excess dialyses and later the ammonia freed by hydrolysis of the ammonium salt. But as it is possible to determine small quantities of ammonia with great accuracy the albumen solution can nevertheless be well defined, and if a quantity of acid equivalent to the remaining ammonia is added to the system there results a system containing albumen and a known small quantity of ammonium sulphate, but neither ammonia nor acid in excess. By addition of acids, salts, or ammonia one can of course obtain systems having very different but nevertheless quite definite composition.

Egg-albumen in crystallising out contains water, so that the factor by which the nitrogen has to be multiplied is not 6·45 but 7·86, the quantity of water in a gram of egg-albumen being 0·22 gm.

The most striking and probably the most important part of the results of Sörensen's researches with the egg-albumen prepared in this manner is that dealing with the application of Gibbs' phase-rule to the equilibrium condition between the crystalline egg-albumen and its mother liquid. The generally accepted axiom in colloid chemistry is that the phase-rule does not hold for colloid substances, and probably this is true for suspensoids where the conditions are very complex, but in this respect emulsoids can be regarded in the same way as true solutions. If an egg-albumen solution is precipitated with ammonium sulphate and left until equilibrium is established, one has a heterogeneous system with three components: (1) water, (2) egg-hydrate, and (3) ammonium sulphate, and three phases: (1) solid crystalline egg-albumen, (2) the watery phase containing ammonium sulphate and egg-albumen, (3) water vapour. The system should then, according to the phase-rule, have two degrees of freedom—that is, if, for example, temperature and ammonium sulphate concentration were chosen, the system should be completely determined. Or, in other words, with crystallisation at a definite temperature and a definite ammonium sulphate concentration, there should be a definite concentration of egg-albumen in solution. As this is not the case, as, for instance, is seen from Galeotti's work, it is generally assumed that the phase-rule does not hold for colloidal substances. This false conclusion is due to the fact that the egg-albumen used by earlier workers did not have always the same quantity of acid or ammonia in excess, and the same holds for the content of ammonium sulphate which very seldom contained equivalent quantities of ammonia and sulphuric acid. Consequently, the number of components is not three, but four—namely, water, egg-albumen, ammonium sulphate and sulphuric acid (or ammonia). With this increase in the number of components the number of degrees of freedom becomes three, and this is confirmed by all Sörensen's experiments, for, at a definite temperature, definite ammonium

sulphate concentration, and definite hydrogen-ion concentration (which is determined by the fourth component), the system is completely determined.

Another difficulty in the application of the phase-rule to emulsoids consisted in the fact that the equilibrium condition seemed to be dependent on the original protein concentration. For example, Chick and Martin (*Bioch. Journ.* vol. 7, p. 380, 1913) find that egg-albumen is more completely precipitated the greater the original concentration. The error committed by Chick and Martin lies in assuming that the ratio between ammonium sulphate and water is not altered during the precipitation, and therefore they carried out no determinations of ammonium sulphate in the filtrate From Sörensen's experiments it is evident water-containing egg-albumen is precipitated, so that the ratio between ammonium sulphate and water is different in the filtrate from that in the original mixture. It is clear that the greater the original concentration of egg-albumen, the more water will be taken away from the solution, and, consequently, the greater the concentration of ammonium sulphate will become in the filtrate, and, as an increase in the ammonium sulphate concentration brings about greater completeness in the precipitation of egg-albumen, the experiments of Chick and Martin are quite in accordance with the phase-rule.

Thus, Sörensen, who in his proof of the applicability of the phase-rule to emulsoid systems gives many interesting details which space does not permit one to deal with, has justified his contention that the study of colloids is best promoted by applying the same principles and points of view as are used in the study of true solutions. This is, perhaps, still more brought out in the last chapter of his book, which contains what is probably an epoch-making treatment of the osmotic pressure of emulsoids.

For the first time this much-disputed question is made clear. It is found that an egg-albumen solution of a given composition has a constant well-defined osmotic pressure the magnitude of which is dependent on the egg-albumen concentration, the ammonium sulphate concentration, and the hydrogen-ion concentration, and varies with these factors according to definite laws. Consequently the osmotic pressure of a correctly defined egg-albumen solution is as definite a quantity as the osmotic pressure of a crystalloidal solution of definite composition.

In this review it has only been possible to deal with a few isolated questions, and the scope and beautiful exactness of the work can only be appreciated by a detailed perusal of the book. The same holds for the masterly mathematical treatment, for example, of amphoteric electrolytes and the application of Donnan's formulæ to the case of albumen solutions. It is clear that these researches form the beginning of a new era in colloid chemistry, and their stimulating influence on all biological sciences cannot as yet be estimated, but must, at any rate, be profound. Similarly, one may perhaps expect that this change of outlook in colloid chemistry will lead agriculturalists and soil chemists to occupy themselves with the emulsoids of the soil.

MECHANISM AND VITALISM, by JOSHUA C. GREGORY, B.Sc., F.I.C.: on **The Organism as a Whole, from a Physicochemical Viewpoint.** By JACQUES LOEB, M.D., Ph.D., Sc.D. [Pp. viii + 379, with 51 illustrations.] (New York and London: G. P. Putnam's Sons, 1916. Price 12*s*. 6*d*. net.)

IN his *Zoonomia* Erasmus Darwin warns inquirers into diseases that "animation" includes more than mechanism and chemistry. This warning clearly and simply

defines the issue between mechanism and vitalism. Mechanism regards the organism as nothing more than a connected collection of physical and chemical processes; vitalism assumes an extra factor that is not physicochemical. This issue is clearly defined and remains so however uncertain the nature of this extra factor and however vitalists may differ among themselves. To believe that organism = chemistry + physics + X is vitalism. X may be regarded as unknown or described in any way whatever—it is vitalism in all cases. Mechanism deletes the X. The problem can be defined with certainty as X or no X; with any attempt at solution certainty disappears.

The discussion is fundamental and far-reaching. Mind and consciousness appear in the living thing. Psychology and, through it, the whole range of thought thus become deeply involved. "Any fact," writes Sir Martin Conway, "may be chosen as the centre of all knowledge," and "any study, if pursued to the end, leads on to all other studies." But some parts of knowledge are more intimately connected with the whole than others—as London is a better centre for exploring or knowing the world than the Arctic wilds. So the mechanico-vitalistic issue, with all its implications and lines of research, bears with particular immediacy on the problems presented to thought. There is, no doubt, a close connection between this immediacy of bearing and the difficulty of the problem. It is improbable that the nature or even the simple existence or non-existence of so momentous an X will be disclosed to simple inspection. Neither is it surprising to discover that the more prolonged the research the less soluble the problem appears. Mechanism and vitalism have not yet composed their differences. It may not be desirable that humans should settle their greater problems promptly; it is quite certain that they cannot.

Mechanism begins with the obvious plenty of chemistry and physics in the organism. It becomes more and more confident as it discovers more and more physico-chemico mechanism. It tends, by an almost inevitable mistake, to suppose that the more complete the mechanism the less the room for an extra-mechanical agency. It commits itself to such statements as that in the work that prompted this discussion, "As soon as we can show that a life phenomenon obeys a simple physical law there is no longer any need for assuming the action of non-physical agencies." Imagine an observer on a distant planet who can observe our railway trains but cannot perceive the humans who direct them. He would see the working of the locomotive without noticing the driver. This illustrates the vitalistic notion of the organism, though all such illustrations are, of course, crude. He could interpret the movements of the trains very thoroughly through physicochemical processes. The machinery of the engine would seem self-explanatory; the burning of the coal operating on the wheels through the drive of the piston propelled by expanding steam would present a purely physico-chemical circuit—as it actually is. The shovelling of the coal into the furnace, if he could observe the motions of the shovel, might strike him as curious, but he would probably conclude that this mechanical causation was beyond the limits of observation—just as the mechanist has constantly to suppose that other chemical or physical processes lie beyond those he can observe. The more this observer studied railway affairs the more impressed he would become with their mechanical nature. He would note that trains started from definite spots at definite times, pursued definite routes at definite rates and had definite stops. In the apparent absence of intelligent agents he would be increasingly impressed by the "clock-work" aspect, and more and more inclined to describe the whole railway system as a mechanism pure and simple. He would tend, in fact, to think, like the

mechanist in biology, that the more mechanism he could detect the less the possibility of an extra-mechanical agent. He might correct this notion by reflecting that complexity and completeness of mechanical organisation are often due to intelligent—to extra-mechanical—agents. This is the vitalist's reply to the statement quoted above and to the mechanist's formidable list of the physico-chemical accomplishments of the organism. There is more mechanism in the weaving of cloth in Britain to-day than in primitive weaving without looms. The mechanism of travel is more perfect, more diffused, and infinitely more elaborate to-day than when the first navigator paddled his tree-trunk with a branch. The range of mechanism, in the sphere of human invention, increases with the range of intelligence. The vitalist justly remarks that, by analogy, the very complexity and extent of mechanism in the organism indicates an extra-mechanical agency. On this point, as on others, the difficulties of deciding the problem grow with research. The mechanist continues to discover more physics and more chemistry in the organism—nicer and more complex adjustments in the living thing. This success constantly arouses the hope that the organism will ultimately be exhaustively analysed into physics and chemistry, and the always pertinent vitalistic reply that the more complex and delicate the mechanism the more probable the existence of the *X*—the extra-mechanical factor.

The mechanist secures no victory by concentrating the controversy on the egg. Loeb remarks that "the cytoplasm contains the rough preformation of the embryo," and, in commenting on some vitalistic views, refers to the importance of the structure and heterogeneity of the unfertilised egg. The study of enzyme action has given a further disclosure of the mechanism, or possible mechanism, of development. Suppose, for the sake of argument, that the fertilised egg-cell is a connected collection of enzymes. Development then proceeds from their systematised co-ordinated activity—as they operate at their appropriate times and in their appropriate places. The problem presented by a host of physico-chemico activities co-operating as a whole towards a certain result—the structure and functioning of the organism in its life-activities—is not seriously altered by throwing the whole burden on a big collection of tiny enzymes. The enzymes are the whole of the organism according to the mechanist—at all events the materials on which they act and the forces they apply fall within the physico-chemico sphere. The vitalist can easily reply that the enzymes behave like chemists who serve the extra-mechanical agency—even if he is compelled to add they are chemists of its own making. They act so appropriately, so in conjunction, that they suggest control and controller as surely as the mechanism of a battleship suggests an artificer. Modern battleship gunfire is *almost* completely mechanical, and it is "almost" precisely because it is not *quite*. It would not be difficult for the planetary observer to overlook the factor represented by the gun-control officer—it is easy to overlook the organising agency in the impressive organisation of the enzymes rendered liberally automatic.

An absolutely exhaustive analysis of the organism might settle the question for or against mechanism. Such exhaustive analysis is not yet, to say nothing of the difficulty of deciding when the limit of analysis is reached. Meanwhile, no multiplication of the organism's purely physico-chemico performances can decide the issue. Loeb resolves a number of animal tropisms into physico-chemico circuits. Heliotropism, he claims, can be connected in many cases with the Roscoe-Bunsen law that the chemical effects of light vary with the product of the time and illumination intensity. "Apparent instincts," he remarks, "in some cases obey simple physicochemical laws with almost mathematical accuracy."

Artificial parthogenesis, intimately connected with Loeb's name, has revealed a chemical mechanism in the fertilising action of the spermatazoön. Other chemical mechanisms stand declared in the action of various salt solutions on developing eggs, and in the chemistry of immunity. The vitalist can admit physico-chemico "sets" in organisms to respond to stimuli—and plenty of them. Is it so certain, besides, that if our planetary observer so perfected his instruments that he could observe human action, he might not be tempted to regard the effects of rain on the populations of cities as negative aqua-tropism, and the influence of the summer sun as positive heliotropism? The perfection of human contrivance secures the perfect setting of mechanisms: the gramophone is "set" to deliver a tune when a catch is released. In the sphere of human invention the greater the distribution and perfection of delicately "set" mechanisms, the more evident the extra-mechanical agency. There still remains the possibility, however, that the vitalist is the victim of a false analogy and that the mechanist is right.

When the human mind is confronted with an *impasse* only resolvable by an indefinite analytical regress, and perhaps not then resolvable, it seeks for some strategical point of view that will make it independent of imperfect knowledge. Vitalism has endeavoured to demonstrate that no physiological or physicochemical machinery could be solely responsible for the morphogenetic or physiological processes of the organism or for the characteristic behaviour of the animal. If it could be demonstrated that any reactions of living things are of such a sort that purely physico-chemico processes could not, from their very nature, completely account for them, the vitalist would occupy a strategic point that would enable him to disregard the argument that complete knowledge might fill all present gaps with a purely physico-chemico filling. If the nature of physico-chemico forces be such that they could not produce some of the results evident in the organism, vitalism can obviously dispense with the complete description of life-activities and affirm that an extra-mechanical factor is present. The argument with a most general appeal is drawn from animal behaviour and particularly from animal behaviour in the higher part of the animal scale. This involves a factor that *appears*, whatever the reality may be, to be extra-mechanical and to control, in some measure, the activity of the organism. This factor is, of course, consciousness, which, wherever any particular observer may place its point of origin in the evolutionary series, must be assumed to exist in, at any rate, higher animals like mammals and birds. Many writers attempt to separate the mechanico-vitalistic problem entirely from consciousness. Thus, Prof. Schafer distinguishes, in his Presidential Address to the British Association of 1912, between the "soul" and the "problems of life" that "are essentially problems of matter." Now, the psychical element appears in connection with life. The morphogenetic processes converge on the production of an animal that can "behave," and consciousness appears in connection with this behaviour. It seems very arbitrary to make consciousness, which appears quite continuous with the whole vital process, a strict dividing-line between the purely physicochemical and the mechanical plus an extra-mechanical agency. The lower down the scale of life the emergence of consciousness is placed, the more arbitrary does the distinction become. Amœba and paramœcium cannot be positively credited with consciousness, but neither can it be positively denied that there are good grounds for such crediting. It would be unsafe to put any lower limit to genetic psychology. In any case it is at least as unsatisfactory to regard, with Sir Noel Paton (in "A Physiologist's View of Life and Mind" in the *Hibbert Journal* for January 1915), consciousness "only as an epi-phenomenon linked to the more complexly developed living

things" as to admit a more or less mysterious extra-mechanical agency in all life. It seems simpler and more coherent to admit the vitalist contention that in consciousness, or mind, the extra-mechanical factor in life fully reveals itself.

Of course the vitalist has still to face the possibility of a final resolution of mind into physico-chemico terms. In any case his position is strengthened if he can discover in any organic processes, such as embryogenesis, that are not usually considered to involve consciousness, results that can be definitely placed outside the scope of physics and chemistry acting alone. A fundamental difficulty at once bars the way to this strategical position. Our experience is far too limited to assign any limit to physico-chemico possibilities. Driesch suggests, in his Problem of Individuality, that a machine may be defined as a given specific combination of specific chemical and physical agencies. He argues that when Hydra grows a new head after decapitation, or when sections of planarians regenerate the whole animal, the mechanistic explanation does not suffice, because a machine is a specific arrangement of parts, and this is altered if a part be removed. Such an argument obviously lays its emphasis on the fixed arrangement of rigid parts characteristic of human-made machines. It emphasises the element of "machinery" in the ordinary or engineering usage of the term. Loeb ascribes regeneration to the restriction placed on the flow of circulating bodies by the isolation of parts. "We shall see that growth takes place in certain cells when certain substances in the circulation can collect there. The mysterious influence of the whole on these parts consists often merely of the fact that the circulating specific or non-specific substances—we cannot yet decide which—will on the whole be attracted by certain spots and that this will prevent them from acting on other parts of the organism. If such parts are isolated the substances can no longer flow away from these parts and the parts will begin to grow." This directs attention to the enormous possibilities of the chemical element in mechanism. "Machinery" plus chemical growth stimulators or producers, if lines of flow or supply be supposed to be given, need not necessarily be put out of action by the removal of parts, so long as the mechanism provided is equal to the task of restoration. Driesch refers to the development of the ovary from the Anlage—one cell producing a number of cells substantially similar to itself. "Machinery" would hardly be capable of such achievement, but can the same be said of a "constellation" of mobile chemical substances? A mother cell could give, it is quite conceivable, to many daughter cells a sample of each of her enzymes or of whatever chemical substances her mechanism might contain. The realisation of the enormous potentiality for mechanistic organisation involved in the chemical process or in a connected system of mobile chemical substances prevents any safe declaration that certain results must be outside the purely mechanistic scope. Our own utilisation of mechanism in our human-made machinery is too limited to enable us to set any limit to Nature's powers. Our knowledge of Nature's methods is insufficient for any assertion regarding their limits to be made. Research continues to discover new mechanisms and new possibilities of mechanism so rapidly and continuously that there must be an element of dogmatism in all attempts to occupy a strategic position with respect to all non-conscious processes by roundly stating that they fall outside purely mechanistic possibility.

It may be possible, by moving upwards into psychology or into animal behaviour, to show that the extra-mechanical factor in the organism is explicitly displayed in consciousness or in animal activity. It may be possible to come down on the mechanico-vitalistic controversy with a decision drawn from philosophy. It does not yet seem possible, by a definite conclusion within the

biological sphere, to provide thought with a definite inductive datum to build upon. Philosophies must still continue to decide from their own general nature whether they will follow the mechanistic or the vitalistic lead. Biology must still leave their claims unsettled.

MODERN SPIRITUALISM, by W. ARNISON SLATER: on

Psychical Investigations, by J. ARTHUR HILL. [Pp. 288.] (Cassell & Co., Ltd., 1917. Price 6*s.* net.)

On the Threshold of the Unseen, by SIR WILLIAM F. BARRETT, F.R.S. [Pp. xx + 336.] (Kegan Paul, Trench & Co., Ltd. Price 6*s.* 6*d.* net.)

The Reality of Psychic Phenomena: Raps, Levitations, etc., by W. J. CRAWFORD, D.Sc. [Pp. 246.] (London: John M. Watkins, 1916. Price 5*s.* net.)

Proceedings of the Society for Psychical Research. Part LXXIII. Vol. XXIX. March 1917. Price 1*s.* 6*d.* net. "The Ear of Dionysius," by THE RIGHT HON. G. W. BALFOUR. (Printed for the Society by Robert Maclehose & Co., Ltd., University Press, Glasgow.)

SIR ARTHUR CONAN DOYLE, speaking recently of spiritualism, referred to "the wonderful literature which had sprung up around it during the last few years. If no other spiritual books were in existence than five which had appeared in the last year—Sir Oliver Lodge's *Raymond*, Mr. Arthur Hill's *Psychical Investigations*, Prof. Crawford's *Reality of Psychic Phenomena*, Sir William Barrett's *Threshold of the Unseen*, and Mr. Gerald Balfour's *Ear of Dionysius*—those five alone would in his opinion be sufficient to establish the facts for any reasonable inquirer." *Raymond* was noticed in this review last July, and the names of the other four books stand at the head of this article. If we cannot all agree that they are sufficient to establish the facts, that may be because we are more impressed by the complexity of the subject than Sir Arthur appears to be; but we can readily admit that together they give a very good idea of the position of modern spiritualism. Nor can any one, without a careful study of some of them, claim a right to express an opinion on the subjects with which they deal.

Mr. Arthur Hill's book is interesting chiefly as an account of his own meetings with mediums, though incidentally it gives a pleasant picture of local spiritualism in the North and Midlands, among industrious and honest people for whom it supplies the place of a religion. From the speculative point of view the book is notable as a record of cases in which a medium gave information about matters apparently outside his own normal knowledge, and not at the time known to the sitter, but afterwards found to be correct. There are other chapters on various aspects of the subject, not without value in themselves, but as an introduction to the history and claims of spiritualism most people will find the book next on our list to be more generally useful.

Sir William Barrett gives us a well-written survey of the whole ground, treating it by a method which is a quite successful blend of the historical and the logical. We have accounts of the most celebrated mediums and their feats, with all the controversies that raged round them. And there are clear descriptions of all the types of phenomena commonly called occult, from planchette-writing and the

divining-rod, through phantasms of the living and the dead, telepathy, poltergeist phenomena, direct voice and direct writing, materialisations, and so forth, to the very latest classical conundrums, appreciable only by the learned. Probably this is the best book of all for one beginning the study of the subject. Its general tone is candid and persuasive without being dogmatic.

Sir William Barrett deals with levitation among other spritualist manifestions, but Dr. Crawford's book is concerned solely with this topic. Levitation is said to occur when a heavy body is raised without the use of any known force, and kept suspended without any visible support counteracting its weight. Dr. Crawford has been fortunate enough to find an unpaid medium who can produce levitation practically at the experimenter's word of command, and he has taken the opportunity to carry out some experiments on more or less scientific lines. He finds that, when the medium is seated on a weighing machine, the table being raised in the air some feet away from her, her weight is increased by nearly the weight of the table, sometimes a little more, sometimes a little less. This taken alone would be interesting, and we might accept the suggestion that, when the added weight is less, a small part of it is borne by other members of the circle acting as accessory mediums. But this, even if it explained anything, would not account for the cases in which the medium's increase in weight is greater than the weight of the table, nor for those in which the medium herself is levitated. And although Dr. Crawford finds no pressure on the floor under the table, yet a scale-pan a few inches above the floor does register a pressure, and sometimes one greater than the whole weight of the table, especially if this is only partly raised, one or two legs remaining on the ground. Again, if the table is levitated from the platform of the weighing-machine, its weight now is found to be on the weighing machine and not on the medium. Sometimes the unseen "operators," as Dr. Crawford calls them, appear to find great difficulty in lifting even a light table; yet the whole strength of four men is not enough to hold the table down if it is determined to levitate; and a single man sitting on it is tilted off with the greatest ease. Dr. Crawford is very ready with explanations: in some cases there is too much light, or of the wrong colour, on a part of the apparatus; or the table is only just small enough to stand on its platform, which is supposed (no reason given) to create difficulty for the spiritual operators. He has also set forth what he calls a theory of levitation, supposing that a flexible or rigid (sometimes it seems to be both at once) rod is formed out of matter taken from the medium and joins her body to the levitated table. Though rigid enough to transmit the weight of this through her body to the weighing machine, it is imperceptible to the touch, nor does she seem to feel any discomfort from the weight so transmitted through her shins, although the table is borne at the end of a lever some feet long. This cantilever theory, with all the accessory explanations, is not by any means convincing mechanically. The one point that emerges from Dr. Crawford's measurements is that they are hopelessly inconsistent and incomplete. When all these experiments have been repeated with other mediums by other investigators, when there is practical agreement as to the facts, a time may come for spinning theories, but not yet.

The root of the trouble is here. If in any ordinary inquiry you came upon results like these, the same or another investigator would take the whole thing up afresh from the beginning, with more accurate instrumental equipment, self-recording apparatus, and so forth, and at least make certain of the facts. But in this case that method is not applicable, because the new investigator might quite likely find no facts at all to work upon. Yet, until this question can be cleared up,

until the atmosphere of caprice and uncertainty which now surrounds these phenomena can be dispersed by further discovery, no real progress in psychical investigation is possible. And so long as this atmosphere is assumed to be the reflection of actual human caprice on the part of certain assumed human "operators" who get "annoyed" when certain experiments are tried, so long no real inquiry will even be attempted. Whatever else the spiritualist hypothesis may do, it certainly acts as a bar to real investigation. We want to know why these phenomena are so irregular and so uncertain, why they require darkness or a red lamp, why we may not investigate them by any method we can think of, such as surprising the "operators." If we content ourselves with the answer that the operators like it so, or do not like it, we shall naturally not get very far. In the early days of electricity it was found that, while glass or sealing-wax could easily be electrified and made to attract small particles, this could not be done with metal rods. If the inquirers had been content with the explanation that the spirits did not like metals, our knowledge of electricity would soon have reached a limit. Dr. Crawford tells us that his "operators" were generally (not always) anxious to aid his investigations, the implication being that they approved and wished to demonstrate his doctrine of spiritualism. From this point of view it is unfortunate that the most impressive manifestations seem to be given to those who already believe and are not in need of further proof. He gives it as one of the prime conditions, "before we can expect anything worth having in the way of results," that the medium and sitters must be "imbued with the seriousness and wonder of the phenomena presented." Fortunately for our wounded, if we are taking an X-ray photograph of an injured limb, it is not found that the believer in radiography has any advantage over the incredulous: the photograph comes out just the same. Nor does the efficacy of a vaccine or serum depend in any degree on the faith of the patient, but protects equally the just man and the anti-vivisectionist. And if Dr. Crawford's "operators" were really sane human beings anxious to prove their and his thesis, we should expect them not only to welcome the opportunity to demonstrate before unbelievers, but also eagerly to fall in with any experimental tests whatever, especially those designed to test their own good faith. If one-tenth of the energy now being wasted in getting useless messages from the other world could be devoted to finding out exactly what it is that happens, and how it happens, psychical research would begin to justify itself, and there would be a speedy end of the complaints that scientific men refuse to consider the subject seriously.

In this respect Dr. Crawford has made a very good beginning which ought to be followed up. But we trust he will see fit to remove from his next edition the pages devoted to a "spirit photograph." It is almost incredible that such passages should have been allowed to creep into a book professing to be a sober and scientific record of facts. It is literally true that if unfounded hypothesis and ill-regulated imagination be taken out of this account, which is given with an air of the most solemn respect, there is nothing left but a shapeless smudge on a photograph.

It is unfortunate that from so many of these books one gets an impression of credulity, eagerness to embrace favourable evidence and to ignore what is unfavourable, a lack of the rigid independence and love of truth for its own sake, which are necessary not merely to inspire confidence but to obtain any success in the search for truth itself. For instance, Sir William Barrett speaks of Dr. Crawford "devising elaborate and ingenious apparatus to test the phenomena." Surely this is at least adequate for a weighing machine, a spring balance, and an

electric bell! Sir William himself is only too ready to pile up hypothesis on hypothesis, incomprehensible forces, particles of thought stuff, exoneural action of mind, and so forth; while his attempt to explain the necessity of mediums, on the ground that, for instance, luminous vibrations are impossible without some medium which can be made to vibrate, would have been better omitted, as a piece of special pleading or even verbal jugglery, calculated to impress the ignorant but not worthy of a serious discussion. Unfortunately such lapses do not impress the ignorant alone, but are partly responsible for the neglect and aloofness of which spiritualists complain.

Mr. Gerald Balfour's contribution forms a part of vol. xxix. of the *Proceedings of the Society for Psychical Research.* It gives a series of automatic writings by two or three automatists during the last seven years, all seeming to bear on the same point. These scripts (though written by living persons who may or may not be in a trance) purport to be controlled by the well-known scholars, G. H. Butcher and A. W. Verrall, who have devised a literary puzzle, in the hope of proving to us on earth their continued existence and activity. At intervals of months or years there have appeared in the scripts of certain automatists fragmentary references to an eye, an ear, a boot, to Sicily, to Ulysses, and so forth. Afterwards came clearer indications, a quotation from Swinburne's *Garden of Proserpine*, mention of Polyphemus, word-play about satire and jealousy. At first and for a long time, no connecting clue between the various ideas could be traced; but, as links were gradually added, connections became visible, and finally all the threads are gathered up and the key found in a rather abstruse quotation. The theory is that the puzzle is so esoteric that only very learned scholars could have invented it, and special touches are claimed as definitely characteristic of the supposed inventors, or one of them, who do of course in the script declare themselves to be its authors.

The subject of automatic writing has attracted a good deal of attention in recent years; it is interesting, and fully deserves thorough investigation. It may be the case, as Mr. Balfour claims, that many of the references are beyond the power and knowledge of the automatist. But the step from this to an admission that the puzzle is the work of two dead scholars is considerable. In the first place, allowance must be made for the fact that more ingenuity is needed to solve a conundrum than to invent it. To solve the one in question in the early stages when only a few points are given would be impossible. But if one started with the last quotation given by Mr. Balfour from the *Encyclopædia Britannica*, and gradually followed separate lines of thought thence arising, until one got to points so divergent that there was no apparent connection—this does not seem beyond average capacity. Apart from this, the proof, such as it is, depends on the chief automatist's honesty and her lack of learning—her inability to invent such a scheme and her inability to lie about it.

All these cases ultimately rest on a probability, a presumption of human weakness: it is *unlikely* that this person could do so-and-so; but it never amounts to absolute impossibility. On the other hand it seems at least *unlikely* that learned and ingenious men, desirous to communicate with us, cannot hit upon any method of doing so which is not open to so many misunderstandings, and so liable to fraudulent imitation. And if any one asks whether these automatic writings, with their erratic interruptions, their fragmentary incoherences, their feeble jocosities, their surprising lapses of memory—whether these writings bear any resemblance to serious attempts by serious men to prove that they still exist in another world, the only possible answer is: not the slightest. And that is the conclusion of the

whole matter. There does seem to be evidence of something happening which is not fully explained by our present knowledge of nature and the human mind. But the more deeply one studies the subject, the more firmly one becomes convinced that departed spirits have absolutely nothing to do with it.

Let us wind up with a couple of quotations :

"A pencil and some pieces of paper were lying on the centre of the table; presently the pencil rose on its point, and after advancing by hesitating jerks to the paper, fell down. It then rose and again fell. A third time it tried, but with no better result. After this a small wooden lath, which was lying upon the table, slid towards the pencil, and rose a few inches from the table ; the pencil rose again, and propping itself against the lath, the two together made an effort to mark the paper. It fell, and then a joint effort was again made. After a third trial, the lath gave it up and moved back to its place, the pencil lay as it fell across the paper, and an alphabetic message told us, 'We have tried to do as you asked, but our power is exhausted.'"

That is a report by Sir William Crookes of what happened in his own house, in the light, with only a few private friends present, when he asked for a written message. The other story is taken from Mr. W. W. Bagally's little book on *Telepathy*, but is not given in his words :

"Many people will remember the Zancigs, who gave a thought-reading performance at the Alhambra some years ago: Zancig went about among the audience, who showed him small articles, which Mme. Zancig on the stage then described. On one occasion they were performing at Birmingham, and Sir Oliver Lodge (who knew them personally) with some friends entered the gallery. At that moment Zancig was in the gallery going among the seats. Just as he was taking something from a lady, he raised his eyes, saw Sir Oliver Lodge, and bowed to him ; then to his wife, 'What is this?' Instead of 'A silver pencil-case,' she replied, 'An Oliver.'"

A NEW AMERICAN UNIVERSITY, by PHILIP E. B. JOURDAIN, M.A. : on **The Book of the Opening of the Rice Institute**: being an Account in Three Volumes of an Academic Festival held in Celebration of the formal Opening of the Rice Institute, a University of Liberal and Technical Learning founded in the City of Houston, Texas, by William Marsh Rice, and dedicated by him to the Advancement of Letters, Science, and Art. [Vol. I.: Pp. xiv + 1–264, with 2 portraits and various inserts. Vol. II.: Pp. x + 265–680, with 5 portraits. Vol. III.: Pp. x + 681–1100, with 7 portraits and numerous diagrams.] (Houston, Texas.)

THESE magnificently produced volumes celebrate the opening of the latest American University. The history of the growth of a noble idea and its materialisation are well sketched by the first President of the Rice Institute, Mr. Edgar Odell Lovett, in a paper in the first volume, and the various important inaugural lectures, delivered by eminent men summoned from all parts of the world to Houston, Texas, in October 1912, form the second and third volumes. To come to details, the first volume also contains the usual preliminaries to an academical festival : list of delegates, addresses of welcome and responses, programmes of concerts, toasts and responses, and accounts of religious services given in the city auditorium. Some of these accounts were reproduced in the *Rice Institute Pamphlet* (1915, **1**, 1–132 ; 1916, **3**, 231–310), and, in the case of the important scientific and other lectures in the second and third volumes, we shall give refer-

ences to the reproductions in rather more accessible form in this *Pamphlet*. The portraits and other reproductions are very fine indeed: in the first volume there are two photogravures of the founder, a view of the University, the general architectural plan, the invitation to the festival, and facsimiles of some of the letters received. The second and third volumes contain finely executed portraits of the authors of the various inaugural lectures, and also one of the subject of one of these lectures, Henri Poincaré. A half-tone reproduction of the same portrait of Poincaré is also given in No. 2 of Vol. I (1915) of the *Pamphlet* (facing p. 133).

The second volume contains: Rafael Altamira y Crevea, "The Problem of the Philosophy of History" (265–87; 1915, **1**, 256–78), "The Theory of Civilization" (288–320; 1915, **1**, 279-311), and "The Methods of Extending Civilization among the Nations" (321–46; 1915, **1**, 312–37); Émile Borel, "Molecular Theories and Mathematics" (347–77; 1915, **1**, 163–93), "Aggregates of Zero Measure" (378–98; 1917, **4**, 1–21), and "Monogenic Uniform Non-Analytic Functions: The Theories of Cauchy, Weierstrass, and Riemann" (399-429; 1917, **4**, 22–52); Benedetto Croce, "The Breviary of Æsthetic" (430–517; 1915, **2**, 223–310); Hugo de Vries, "Mutations in Heredity" (518–70; 1915, **1**, 339 91), "Geographical Botany" (571–95), "Modern Cytological Problems" (596–614), and "The Ideals of an Experiment Garden" (615-9); Sir Henry Jones, "Philosophical Landmarks, being a Survey of the Recent Gains and the Present Problems of Reflective Thought" (620–80; 1915, **1**, 195–255).

The third volume contains: Baron Dairoku Kikuchi, "The Introduction of Western Learning into Japan" (681–725; 1915, **2**, 55–99); John William Mackail, "The Study of Poetry" (726–77; 1915, **2**, 1–52); Wilhelm Ostwald, "The System of the Sciences" (778–867; 1915, **2**, 101–90), and "Principles of the Theory of Education" (868–98; 1915, **2**, 191–221); Vito Volterra, "Henri Poincaré" (899–928; 1915, **1**, 133–62); Sir William Ramsay, "The Electron as an Element" (929–46; 1915, **1**, 392–409), "Compounds of Electrons" (947–61; 1915, **1**, 410-24), and "The Disruption of the so-called Elements" (962–80; 1915, **1**, 425–43); Carl Størmer, "The Corpuscular Theory of Aurora Borealis" (981–1035); Vito Volterra, "The Generalization of Analytic Functions" (1036–84; 1917, **4**, 53–101), and "On the Theory of Waves and Green's Method" (1085–100; 1917, **4**, 102–17).

It may surprise some that such a large space is given to pure mathematics in these lectures. But it seems particularly suitable that, in a large and rather sparsely inhabited province of the United States where we should expect particular attention to be paid to the practical sciences, a far-seeing President and Committee should have laid stress on the great truth that science in general can only proceed if the logical instrument for exact thought and exact expression is diligently cultivated. It is surely not a mere accident that two of those men among modern mathematicians should be chosen as lecturers whose work is in the foremost line of advance of pure mathematics and has also a very close connection with mathematical physics.

I will now attempt to pick out some of the points of scientific interest in some of the lectures.

Sir Henry Jones attempts to "indicate the manner in which the natural sciences . . . must not only extend your mastery over the outer world, but reverberate within your inner selves, enriching and enlarging the powers of your rational nature." The intercourse of Japan with the West began in 1543, and then it was through the Portuguese. Not long afterwards came the English, the Dutch, and the Spanish; but Western medicine, surgery, and mathematics seem to have been introduced by the Jesuits. Baron Kikuchi's short summary is

especially interesting from the point of view of the parts which various nations have played in developing the intercourse up to 1912. Mr. Mackail deals in succession with the function of a University, the nature of Poetry, the Modern World, Poetry and Science ("The fancied opposition of science to art and letters, and more particularly to poetry, is injurious to the general interest of mankind. . . . The creative instinct, the imaginative impulse, which find expression in poetry, are powerfully reinforced by the discoveries of science and by the growth of the scientific spirit. . . ."), Poetry and Business, and Poetry and Democracy. The lecture by Prof. Størmer contains a summary of his researches on aurora borealis which were begun in 1904, and the results of which have been published from 1904 to 1912 in various periodicals.

Two of the lectures by M. Borel and two of those by Prof. Volterra were noticed in the "Recent Advances" of the last number of SCIENCE PROGRESS (1918, **12**, 544). M. Borel's lecture on "Molecular Theories and Mathematics" starts from the reflection that "it was the study of physical phenomena which suggested the notions of continuity, derivative, integral, differential equation, vector, and the calculus of vectors; and these notions, by a just return, have become part of the scientific equipment necessary to every physicist . . .," and examines the influence which molecular theories may have on the development of mathematics. Indeed, "the points of contact between molecular physics and mathematics are numerous," "mathematicians can only gain by investigating [the analogies] more closely," and "the task . . . cannot long be deferred of creating an analysis adapted to theoretical researches in the physics of discontinuity." Prof. Volterra's lecture on "Henri Poincaré" emphasises the very modern aspect of Poincaré's scientific work. At the present day scientific work is published chiefly in the form of memoirs in scientific journals, so that work is often published as it progresses. "The proceedings of the academies, short and precise reviews, have appeared. A man reports in a few words every discovery as soon as he has made it. Time presses; one fears that the next minute the discovery may be lost . . . this development has created a particular state of mind among scientists, and has changed their lives, their ways of working, and even of thinking. There are great advantages in this modern scientific life. Research has become almost collective. The energies of the investigators are summed; their discoveries follow each other rapidly; competition spurs them on. Their number increases from day to day. But how many objections we can oppose to these advantages!" Indeed, the whole aspect of scientific life has quite changed since the tradition created by Gauss's practice of writing "*pauca sed matura*." It may be remembered that Weierstrass once remarked that the method adopted by the Paris Academy of Sciences for announcing discoveries seemed to him to injure the work of Poincaré. However, it does not really seem that the objections urged by Weierstrass had weight against Poincaré's best work; as regards his "philosophical" work, certainly much of it gives one the impression of a kind of lively lack of interest in the subject, and consequent carelessness, but the work that Poincaré loved preserves a power of stimulating his readers, and, like many Frenchmen, he thought so quickly and accurately in his chosen domain that one can hardly imagine that his work would have been improved by years of silent meditation before it was published. Prof. Volterra gives a very clear account of certain of Poincaré's mathematical investigations: on the theory of linear differential equations and "Fuchsian" functions, on mathematical physics, and on dynamics and astronomy. In particular, that investigation is described rather more in detail which concerns the equilibrium of a rotating fluid mass.

We have noticed the stimulating effect of much of Poincaré's work, which is partly due to the modern methods of publication. There are some remarks made by Sir William Ramsay, in his reply to a toast after a public luncheon given on the occasion of the opening of the Rice Institute, which seem to bear on the subject. Speaking of the danger of having too large classes of students in a University, he recommended that the number of assistants to a Professor should not be increased but the number of entirely separate departments should be increased. Learned men cannot, he points out, be made like needles or wire or nails, but each student must come into personal contact with his teachers. It seems that this Platonic view ought, perhaps, to be rubbed into our British educational authorities rather than into such authorities in America. The importance of personal contact has never been lost sight of in America: we need only remember the wonderfully broadminded conditions under which Sylvester held his professorate at Johns Hopkins University.

What seems to be an even greater need at the present time is the provision of means whereby an intending investigator may keep abreast of the huge flood of literature on scientific subjects. It is—partly at least—owing to the modern conditions spoken of by Prof. Volterra that men of science publish their work in short communications at different times and perhaps in different periodicals. The personal element is preserved, in many cases, because we are present, so to speak, at the discovery of what is discovered; but it becomes necessary to have a complete and critical index, a thread to follow in the maze of scientific literature. This seems to be one of the things that can be undertaken by a prosperous University, and by such a University alone. Hitherto the work of this kind has almost wholly been left to Germany. Now, apart altogether from certain national prejudices which undoubtedly appear in many German accounts of other nations' work, and thus affecting both the completeness and the value of the criticism, we are faced with the problem that, for reasons which reduce to financial ones, no European country except Germany ever has been able to undertake such a work on a large scale, while Germany itself will probably be unable to do its part for some time to come. One of the distressing results of the intolerable (to others) government of Germany is that what is good in Germany —good intentions of some of its scientific men—has its power weakened.

It only remains to point out one or two printer's or other mistakes in these noble volumes. The word "the" is sometimes superfluous—at least to British ears: thus "the Nations" (p. 321) and "the mathematical analysis" (p. 984). In the note on p. 273 it is confusing that the title of a book is translated, although the book has not been translated into English, but this confusion is not always made (for example on p. 336). In the notes on pp. 1041 and 1042 the text of the Italian is not translated, as if "*Vedi*" and "*e seg.*" were part of the title of a book or a paper. On pp. 399 and 428 the word "vigorous" appears instead of "rigorous"; and on p. 400 the word "exactly" is wrongly used: Fourier cannot be said to have proved his theorem *exactly* (that is, rigidly); what the original seems to have meant is that this theorem was exactly what Fourier proved—or rather made very probable.

REVIEWS

MATHEMATICS

Theory of Functions of a Complex Variable. By A. R. FORSYTH, Sc.D., LL.D., Math.D., F.R.S., Chief Professor of Mathematics in the Imperial College of Science and Technology, London, and sometime Sadlerian Professor of Pure Mathematics in the University of Cambridge. [Third Edition. Pp. xxiv + 855.] (Cambridge: University Press, 1918. Price 30s. net.)

THIS book is well known as almost an encyclopædia of the theory of functions of a complex variable, and the fact that it is now in a third edition bears testimony to the great extent to which it has been used as a textbook. The Preface to the first edition was written in 1893, and the short description of the contents of the book given there (pp. vi–vii) still remains true: the first part (Chapters I–VII) contains the theory of one-valued functions, beginning with complex integration and going on to the theorems of Weierstrass and Mittag-Leffler; the second part (Chapters VIII–XIII) contains the theory of many-valued functions, principally after Briot and Bouquet, while a chapter has been devoted to the proof of Weierstrass's results about functions that possess an algebraic addition-theorem; the third part (Chapters XIV–XVIII) contains a treatment of Riemann's method; the fourth part (Chapters XIX, XX) treats conformal representation; and the fifth part (Chapters XXI, XXII) contains an introduction to the theory of automorphic functions. In the second edition, the Preface to which was written in 1900, no substantial alterations were made to the part on the theory of one-valued functions, "but new references are given for the sake of readers who may wish to acquaint themselves with the most recent developments" (p. ix). The other chief alterations in this second edition were in the proof of the existence of various classes of functions upon a Riemann's surface, several additions to Chapter XVIII on algebraic functions and their integrals, and a transference to the second volume of Prof. Forsyth's *Theory of Differential Equations* of the sections that discussed the properties of certain binomial differential equations of the first order. In this third edition, of which the Preface was written in 1917 some changes have been made, for example, in the establishment of the fundamental functions in Weierstrass's theory of elliptic functions, a note has been added giving some applications of the theory of conformal representation to some branches of mathematical physics (pp. 639–52), and a note stating the results of the discussion referred to above of certain differential equations.

It seems unfortunate that in this third edition very little has been done to bring matters up to date, particularly in the first part. Between 1900 and 1917 much has been done towards the putting of Cauchy's theory on a sound basis; yet here there is no mention of Goursat's remarkable work, no indication is given of modern investigation on "contours," and, indeed, the only marked addition seems to be a reference on p. 49 to the book of Lindelöf (1905) on the calculus of residues. One might expect some reference to the work on analytic continuation

done between 1894 and 1917 by Borel, Hadamard, Mittag-Leffler, and others, but the nearest approach to this is a reference—it is hardly more—on pp. 69–70 to some papers by Mittag-Leffler and Poincaré. Even in this third edition Prof. Forsyth is unpardonably obscure about the values of a function "at" an essential singularity (pp. 66, 115, 117), and no account is given of Picard's proof by modular functions of his very important theorem on "exceptional values." But sometimes, as on p. 111, a decided attempt is made, even in the first part, to bring references to investigations up-to-date, while the added note already mentioned, on the applications of conformal representation, is quite modern.

As is so often the case with mathematical textbooks, so it is here: the book vastly improves when the first few chapters are left behind, and would be still further improved if they were left out. Prof. Forsyth's useful book does not now, it seems, make many attempts to be an encyclopædia or to be quite accurate; perhaps this is why it is a good textbook.

PHILIP E. B. JOURDAIN.

Infinitesimal Calculus. By F. S. CAREY, M.A., Professor in the University of Liverpool [Section I.: Pp. xiv + 144 + v, with diagrams, 1917; Section II.: Pp. x + 145–352 + iv, with diagrams, 1918.] (London and New York: Longmans, Green & Co. Price 6*s*. net and 10*s*. 6*d*. net respectively.)

THIS book is one of "Longmans' Modern Mathematical Series," and the first section "deals with those parts of the Infinitesimal Calculus which have been recently introduced into the syllabus of some examinations for higher school certificates, while the two sections taken together correspond fairly closely to the curriculum of students reading for the first part of an honours course in mathematics or for the ordinary degree in arts, science or engineering" (p. v). The first section passes through "those domains of number and function with which the student is probably already acquainted, while the functions which are likely to be unfamiliar to him have been reserved for the second section" (p. v). In symbolism there are some good innovations in this book: there is a convenient notation for open and closed ranges of real variables by square and round brackets (p. 14), and arrows with a single (upper or lower) barb are used to express convergence of the general term of a sequence down or up to a limit (p. 24). This notation is a decided advance on the fully-barbed arrow now so generally used to replace signs often involving the notion of "equality to infinity." The most striking difference of this book from others is that "no attempt has been made in the first section to deal with the definite integral, nor has the usual notation for the indefinite integral been introduced until a comparatively advanced stage" (p. vi), because of the impossibility of justifying the use of the usual symbol for an indefinite integral as a representation of inverse differentiation (see p. 110, cf. p. 122) until the nature of a definite integral has been explained.

"The book is not written for any particular group of students; it is designed for those who wish to use the Infinitesimal Calculus as an instrument in the attainment of further knowledge" (p. vi); and yet more attention is paid to logic than in the usual textbook: in fact, if we describe, as it seems that we may, the object of any textbook to be to give, for teaching purposes, a judicious compromise between history and logic, this book must be accused of being logical. Though it is on "infinitesimal" calculus, infinitesimals are banished (cf. p. 38). This is a good book, and it is pleasing to see (p. 245) a treatment of integrals giving mean values—an important notion in the theory of functions.

PHILIP E. B. JOURDAIN.

Leçons sur les Fonctions Monogènes uniformes d'une variable complexe. By ÉMILE BOREL, Professeur de théorie des fonctions à l'Université de Paris. Rédigées par GASTON JULIA. [Pp. xii + 165.] (Paris: Gauthier-Villars et Cie., 1917. Price 7 fr. 50 c.)

IN this, the most recent of M. Borel's "Collection de monographies sur la théorie des fonctions," we get an insight into much of M. Borel's life-work in mathematics. In his Thesis of 1894 he took up the question of the possibility of defining a "continuation" of an analytic function beyond a "natural limit" of essential singularities, the impossibility of which then seemed to have been shown by Poincaré. In fact, Poincaré had constructed analytical expressions with certain singularities, and had given an argument to prove that the theory of analytic functions could not be extended beyond those kinds of regions defined by Weierstrass. In this book, the Preface contains a short summary of the history of the author's researches; it was not till 1912 that he proved that he had succeeded in completely defining more extended regions than those of Weierstrass, in which monogenic functions can be defined by Cauchy's integral which possess the characteristic properties of the analytic functions of Weierstrass.

It seems, however, not quite justifiable to say (p. viii) that Weierstrass's limitation is, "consequently, purely arbitrary." In fact, Weierstrass's theory concerns power-series and their continuations, and for functions so defined his regions certainly cannot be generalised. It is interesting and important that other functions with the properties mentioned above can be defined, but it is hardly legitimate to speak of the theory of Weierstrass as if Cauchy's theory had gained a victory over it in a new Franco-Prussian conflict. And the author seems to depreciate the merits of Weierstrass, and to attribute to his eminent pupil Prof. Mittag-Leffler opinions (p. vii) which it is incredible that he should hold. Further, it must be remarked that not the least merit of Weierstrass's work on analysis is the thorough treatment of such subjects as irrational numbers, in which, by defining real numbers as classes of rational numbers, he anticipated one of the main points in modern logical theory.

The first chapter contains the preliminaries of a theory of one-valued functions of a complex variable: Goursat's proof of Cauchy's theorem, a treatment of Weierstrass's regions and the elements of the representation of analytic functions in such regions. This chapter is almost wholly good: the only exception seems to be the often repeated and mistaken views (pp. 3–5) on "finite definability." The second chapter contains the application of Cauchy's integral to the development in a series of polynomials of a function defined in a region of Weierstrass (Runge's method, and also a modification of some work of Mittag-Leffler). The third chapter contains some remarkable consequences of the development just referred to, and the extension of the theory of analytical continuation. The other two chapters concern the new theory. It is essential that we consider a part of the complex plane containing points everywhere dense in a part of this area; and, by Chapter IV (*cf.* pp. 124–6), we may limit our consideration, without loss of generality, to those points with rational co-ordinates. Then the regions (C) aimed at are defined: they are not "perfect" in Cantor's sense, the aggregate of points which is complementary to the part considered of the plane of complex numbers is of measure zero, and there are perfect regions which are analogous to the perfect regions, limited by circular arcs, inside and approximating to a region of Weierstrass (pp. 125, 131). Then monogenic functions are defined in the regions C. In the proof of a theorem analogous to Cauchy's, the hypothesis of the continuity of the derivative is used, although it is "doubtless superfluous" (p. 135). The

integral of Cauchy is generalised for the regions C (p. 140), and thus the monogenic function is proved to have derivatives of all orders, to be continuable along certain lines, and to be representable by a series of Mittag-Leffler's polynomials (*cf.* p. 141). The "new monogenic functions possess the fundamental property of analytic functions, uniqueness of continuation" (p. 144). This is a most important book: we may remark that it contains a full exposition of the theories described in the last number of SCIENCE PROGRESS (1918, **12**, 544), and referred to in a review of *The Book of the Opening of the Rice Institute* in this number.

PHILIP E. B. JOURDAIN.

ASTRONOMY

(1) **The Dimensions of a Globular Cluster.**

(2) **Globular Clusters and the Structure of the Galactic System.** By HARLOW SHAPLEY. (Reprints from the *Publications of the Astronomical Society of the Pacific*, No. 172, December 1917, and No. 173, February 1918, respectively.)

(1) IN the Astronomy section of "Recent Advances in Science" in the last number of SCIENCE PROGRESS mention was made (p. 552) of the methods by which Mr. Harlow Shapley, of the Mount Wilson Observatory, has been able to make reliable estimates of the distances of stellar clusters. These clusters are at such great distances that direct measurements of their distances do not give reliable results. The best determinations of stellar parallaxes have probable errors which are many times larger than the parallaxes deduced by Shapley for various clusters. The concordant values which he has obtained by different lines of reasoning give confidence, however, that his values are at least of the correct order of magnitude.

In the first of the papers under review he has given detailed results for one cluster, Messier 3, which gives one some idea of the enormous size of many of these objects. This cluster is one of the brightest in the northern sky, being visible under good conditions to the naked eye near the southern edge of the constellation Canes Venatici. Its parallax is estimated to be $0''\cdot000074$. This corresponds to a distance of nearly three thousand million times the distance of the sun from the earth, so that the light now reaching us from the cluster left it 44,000 years ago. In a photograph taken with the Mount Wilson 60-inch reflector more than 20,000 stars can be counted, excluding the central area where they are clustered too thickly to be counted. With the new 100-inch telescope many more stars would appear on a photograph. The diameter of the cluster as ordinarily photographed is about thirty million times the distance of the earth from the sun, and to cross the cluster light must travel 470 years. Many of the stars are immensely superior in brightness to our sun, which, if situated in the cluster, would not appear in the photograph, although the latter contains stars as faint as the twentieth magnitude. Sirius would only appear as a very faint star of the seventeenth magnitude.

It would appear either that such clusters as Messier 3 are separate stellar universes or that we must be prepared to revise the ideas we have hitherto held as to the actual dimensions of our stellar universe.

(2) The second paper gives a summary of numerous conclusions which Shapley has drawn from his series of researches on Globular Clusters which have previously been referred to in the Recent Advances section. Inasmuch as the precise grounds upon which the conclusions are based are not detailed, but are

to be given at length in several contributions from the Mount Wilson Observatory which will appear in the course of a few months, criticism of the results at present would be inadvisable even if possible. These results are of such fundamental importance that more detailed reference will be made to them when full particulars are available. It may be stated, however, that they revolutionise our conception of our stellar universe. Various lines of argument lead Shapley almost naturally to the conclusion that the globular clusters are all members of our galactic organisation, and that they outline its extent and arrangement. The immense distances at which many globular clusters are situated have been referred to under (1). Adopting this view of the stellar system, it follows that all known sidereal objects—including globular clusters and such structures as the Magellanic Clouds—are parts of one enormous unit whose volume is more than one hundred thousand times that commonly assigned to the stellar universe. Various lines of argument had appeared to lead to the conclusion that the spiral nebulæ were separate stellar universes, and an analogy between their structure and the structure of the nearer part of our galactic system was pointed out. But such estimates of their distance as have been made (see SCIENCE PROGRESS, p. 551, April 1918) are of the order of distance deduced for the globular clusters. If, then, the latter are members of our universe, so also must the nebulæ be. It appears, therefore, as though the "island universe" theory has been seriously shattered by Shapley. A more definite conclusion must be withheld until his detailed investigations have been published.

H. S. J.

PHYSICS

The Theory of Electricity. By G. H. LIVENS, M.A. [Pp. 717+vi.] (Cambridge University Press, 1910. Price 30*s*. net.)

THIS is a general textbook on the mathematical aspects of modern electrical theory. It is based mainly on the original Faraday-Maxwell theory. This theory has, of course, been generalised to meet the growing body of experimental knowledge and to embrace the concept of the electron and the electro-dynamics of moving systems. In this generalisation the author follows the views of Sir J. Larmor as expounded in his lectures and papers and his book, *Æther and Matter*, on the ground that although "this form of the theory has been almost entirely abandoned in recent accounts of the subject, it remains the only one which appears to be completely satisfactory from the point of view of mathematical and physical consistency, and in its generality it is unapproached by any other form."

The book forms a very comprehensive account of the subject, the author's aim being to emphasise the dynamical aspects of electrical and magnetic phenomena, and to give a rigorous formulation of underlying physical principles as well as translate them into mathematical theory. There is a mathematical introduction giving the essential parts of vector analysis, as required later, the theorems of Green and Stokes being expounded for moving circuits as well as stationary ones, and a section inserted giving a short account of Poincaré's discussion of potential integrals as regards convergence and possibility of differentiation. The main part of the book is divided into two parts on the stereotyped lines of books on Mechanics, Chapters I.—VIII. treating the "Statics and Kinetics" of electrical and magnetic phenomena and Chapters IX.—XV. dealing with "Dynamics." The treatment in the first part follows well-recognised

lines, but it is characterised by great care in the development of the ideas, as, for instance, in the passage from point charge to continuous distribution, and thence to surface distribution and double sheets. All the well-known geometrical distributions of charge and their fields are treated with a free use of the Legendre and Bessel functions, without overburdening the pages with purely analytical mathematics, and there is a very good account of the application of conformal representation and inversion to electrostatic problems. After developing the Faraday-Maxwell theory of electrostatic action the author gives a very full chapter on the theory of polarised media, using Larmor's mode of presentation, with a section on Lorentz's theory as to the mechanism of dielectric polarisation. A chapter on Magneto-statics includes not only much classical material, but introduces sections dealing with Langevin's and Weiss's treatment of paramagnetism and ferromagnetism. Under "Kinetics" the author treats the phenomena of metallic conduction and electrolysis, gives Lorentz's electron theory of the mechanism of metallic conduction and some account of electric currents in gases, with the methods of determining the charge and mass of an electron.

The second part, dealing with "Dynamics," begins with a general account of the equations of the electromagnetic field, introduces the "retarded" potentials, and discusses very thoroughly the complete constituents of an electric current in the most general sense. Two chapters treat some special electromagnetic fields and the electrodynamics of linear currents; and two more deal with electromagnetic oscillations and waves. In these, in addition to the usual theory of the Hertzian oscillator, an account is given of Love's extension to the more practical case when damping is taking into account. These matters naturally lead to some discussion of radiation in general. Two concluding chapters deal with general electrodynamic theory both for stationary and moving media on the basis of the principle of least action, and constitute, in the writer's view, the most valuable part of the book, inasmuch as they present in a compact form material hitherto scattered over many books and papers. The book as a whole is a very valuable addition to the literature of the subject, not only on account of the wide field covered, but also because of the care exhibited in the selection and treatment of the material. The form of the theory adopted is so general that it can include the most recent speculations on such matters as the nature of radiation and the relativity view of time and space without being dependent for consistency on the truth of such views. In its 700 pages is summarised a body of knowledge which will put the reader, as it were, right on the threshold of present-day research in theoretical electricity and magnetism. There is a collection of over three hundred examples on which the mathematically-minded student can try his prowess.

J. R.

Lecture Notes on Light. By J. R. ECCLES, M.A., Assistant Master at Gresham's School, Holt. [Pp. viii + 217.] (Cambridge: University Press, 1917. Price 12*s.* 6*d.* net, with Diagrams; 5*s.* net, without.)

THERE is still a difference of opinion as to the manner in which students should record the information given to them in their lectures. A few teachers prefer the class to give them an undivided attention during the lecture and to rely on the text-book for a permanent reference. In the vast majority of cases notes are taken, and the rate at which the ground is covered must be regulated to enable this to be done. Even then the weaker brethren produce but a sorry record, and

for their benefit a certain amount of dictation or blackboard writing is essential. At Gresham's School all the notes were dictated until the author tried the plan of distributing the printed notes on which this book is based. It is claimed that the experiment has proved a success—" fifty per cent. more ground is covered, and the boys understand the work just as well." Against it there is the objection that the acquirement of the art of note-taking is a not unimportant part of an elementary science course. Moreover, even if notes be distributed, each teacher's course should have an individuality of its own which would lose much by being tied down to a precise scheme drafted by another.

In the cheaper edition of the book the left-hand pages are left blank so that the students themselves may draw on them the diagrams which form so important a part of an elementary light course. The complete edition, with diagrams, is intended as a key for the master's use. This plan is novel so far as physics books are concerned, and its advisability must depend on the capabilities of the class. The notes themselves are clear and concise, covering the ground of a school course in geometrical optics of perhaps a little more than matriculation standard. A little adverse criticism is necessary. The fact chosen to disprove the corpuscular theory is unfortunate: ". . . if the corpuscles travelled with this enormous velocity they would possess a considerable momentum, of which there is no evidence." This is, of course, incorrect, even though that which exists is only a second order quantity; the true criterion might easily have been given. Four of the diagrams showing the paths of light rays through telescopic systems (pp. 184 and 186) are very misleading in that the rays leaving points on the images are not the same as those arriving at them, so that, apparently the rays are bent in passing through a focus. Experience in marking examination papers shows how prevalent this error is. The descriptions throughout are written with the personal pronoun "we did . . .," "we took . . .," "we drew . . .," etc.; even at school boys should be taught to write impersonally. A word of praise must be given to the way in which the book has been produced; the paper, printing, and diagrams are altogether admirable.

D. O. W.

A Textbook of Physics. By J. DUNCAN, Wh.Ex., M.I.Mech.E., and S. G. STARLING, B.Sc., A.R.C.Sc. [Pp. xxiii + 1081, with 985 figures in the text.] (London: Macmillan & Co., 1918. Price 15*s.* net. Also issued in Parts: Dynamics, 5*s.*; Heat, Light, and Sound, 6*s.*; Magnetism and Electricity, 4*s.*; Heat, 3*s.* 6*d.*; Light and Sound, 3*s.* 6*d.*)

THIS textbook is intended primarily for the use of students of engineering, and the subject-matter has been chosen consistently with that end in view. The first 300 pages form a complete treatise on elementary mechanics. Stress is laid on graphical methods, and the examples are such as are of special interest in engineering work. The section on heat includes an account of combustion and of calorimeters designed to measure the thermal value of various fuels; there is a good description of oil and steam engines, boilers, indicators, turbines, and even of the Gaede molecular air-pump. Viscosity has, however, been overlooked, and the treatment of surface tension and osmotic pressure is very brief. In light special attention is given to surveying instruments; in electricity to dynamos, motors, and alternators. Taking the book as a whole, the material has been selected with excellent judgment, is up to date, and is such as is not to be found in any other book of its kind.

Unfortunately it is impossible to take quite the same favourable view of the manner in which the fundamental principles have been treated. It must be remembered that, in the great majority of cases, the intermediate course furnishes the only training in pure science that the engineer receives. It becomes, therefore, even more important in his case than in that of the chemist or physicist to give some insight into methods of scientific thought and to attempt to inculcate, if only by example, something of the spirit and inspiration of science. It is essential to impart some knowledge as to how and why the governing principles originated and developed; of the difficulties overcome and remaining to be solved; and above all of the theories, however incorrect they may ultimately prove to be, by which we endeavour to connect our ideas. In this way science is realised as a living organism; facts are seen in their several aspects; the imagination is stimulated and the mind is brought into touch with the hidden processes of nature, whose external effect alone it is usually possible to study. No doubt these things are not essential for success in examinations or for the acquirement of a considerable facility in working routine problems, but they form a very necessary part of a true scientific equipment.

Judged from this standpoint the book is far from satisfactory, as a few examples will suffice to show. On the first page there is this statement: "The fundamental units—to which are referred all measurements in any scientific system—are those of length, mass, and time." That is all that is said about them. The student is not invited to stop and think why such units are required, why there are only three, why this particular three, or of the possibility of choosing others. Again, mass is defined simply as "quantity of matter." It is not discussed at all, and there is not the vaguest suggestion as to how it can be measured apart from weight. Such teaching can only be characterised as shallow and vague. Apart from three pages containing a specially uninteresting and uninformative account of the kinetic theory of gases, there is no mention at all of the kinetic theory of matter, so that in dealing with thermal conductivity, after stating that the heat "is passed through from layer to layer" of a body, the authors are compelled to say that "the process has not yet been investigated thoroughly." True, the exact part played by the electron in the process is uncertain, but in these days of low-temperature research, Brownian motion, and the revelations of the X-ray spectrometer, it is not unreasonable to expect something a little more satisfying. The definition of conductivity in terms of the heat flowing through a unit cube is usual, but none the less objectionable. Convection is treated in the old-fashioned way as a separate mode of heat transfer instead of a case of accelerated conduction. On the other hand, in the section on thermodynamics there is a genuine attempt to explain what is meant by a reversible cycle. Kelvin's absolute temperature scale is given again; it is never really understood by elementary students, and, being quite unnecessary, might well be relegated to the advanced course or, better still, to the historian.

The other sections of the book are less open to criticism. In giving the Young-Helmholtz colour theory it should be pointed out that, successful as it has been in connecting many of the phenomena of colour vision, it is nevertheless now known to be incorrect. In the account of the theory of electrolysis the all-important fact that the charge on a monovalent ion is equal to that on an electron is not mentioned, and, indeed, there is no attempt at all to give the reader any notion of the mechanism of the electric current. Electrostatics is placed most remarkably in the middle of current electricity.

With the exception of the first part (dynamics) the book can safely be recom-

mended to those who are content with a concise matter-of-fact treatment of the subject. The diagrams are numerous and exceedingly clear; there is an ample selection of problems (with answers) at the end of each chapter and a complete course of practical work. The paper is good, but the binding over light for so heavy a book.

D. O. W.

CHEMISTRY

Theoretical and Applied Colloid Chemistry. By DR. WOLFGANG OSTWALD. Authorised Translation by DR. MARTIN H. FISCHER. [Pp. xv + 232, with 45 illustrations.] (New York: John Wiley & Sons. London: Chapman & Hall, 1917. Price 11*s*. 6*d*. net.)

THIS book contains a selection from a number of lectures delivered by the author in America during the winter of 1913-14. The object of these lectures is essentially to claim for colloid chemistry the right to be regarded as an independent science subject. There can be no question but that the plea is amply justified, although colloid chemistry has not as yet attracted the attention which it deserves. That this is Dr. Ostwald's view likewise is indicated by the sub-title to the book, viz. "the world of neglected dimensions."

Naturally in a brief survey of a more or less popular nature, the treatment cannot be expected to be profound. Out of a wealth of material the author has made an excellent choice. The most striking thing about these lectures is the experimental illustration of many of the points discussed. It is indeed remarkable that so much can be done within the limited range of a lecture demonstration. This aspect of the work will be appreciated by all those who have occasion to lecture upon the subject or who desire to bring colloids into a systematic course of practical chemistry.

The lectures reproduced are five in number. The first three deal with the fundamental properties of the colloidal state, the concept of disperse systems, the classification of colloids, and the properties of colloids as a function of the degree of dispersion. The fourth lecture deals with "some scientific applications of colloid chemistry," that is, with the application of the science of the colloid state to other sciences, *e.g.* to physics, analytical chemistry, photochemistry, preparative organic chemistry, mineralogy, geology, soil chemistry, agricultural chemistry, biology, and medicine. The fifth lecture treats of a number of "technical applications of colloid chemistry." How vast the field is here will be gathered by simply enumerating the more important technical operations, processes, and manufactures in which colloid chemistry plays a significant rôle: the manufacture of lubricants, use of straw infusions in brick-making, incandescent light filaments, coloured glasses, ultramarine, photographic materials, inks, ceramics and hydraulic cements, iron-carbon alloys, dyeing, tanning, cellulose industries, parchment manufacture, mercerisation, artificial silk (viscose), celluloid, varnishes, bakelite, rubber coagulation, vulcanisation, manufacture of soap, starch, glue, mucilages, food products such as margarine, etc. This list, though by no means complete, covers the entire round of chemical technology. No stronger argument could be urged for a fuller realisation of the fundamental importance of colloid chemistry.

The author has succeeded in presenting a well-balanced account of the possibilities of colloid chemistry in an exceedingly readable and interesting form.

W. C. McC. LEWIS.

Lecithin and Allied Substances: The Lipins. By HUGH MACLEAN, M.D., D.Sc. (Monographs in Biochemistry.) [Pp. vii + 206.] (London: Longmans, Green & Co., 1918. Price 7*s*. 6*d*. net.)

AN ether extract of a tissue or organ is likely to contain a mixture of a number of different substances which may be classified as follows: (1) Neutral fat or fatty acid; (2) cholesterol and pigments not chemically related to fats; (3) substances, such as lecithin, containing fatty acids, nitrogen, and phosphorus; (4) substances containing fatty acids, nitrogen, and a carbohydrate complex, but no phosphorus. The two latter groups, to which Thudechum gave the names of Phosphatides and Cerebrosides respectively, are frequently classed together as lipoids, but this term is by some authors made to include cholesterol and similar bodies. Overton, on the other hand, uses the term lipoid in a general sense to denote all cellular components soluble in organic fat solvents, such as chloroform or ether. To avoid confusion the present author proposes to do away with the word lipoid altogether and to replace it by Lipin, which term is to include only groups (3) and (4). The classification adopted in this monograph is practically that of Thudechum. The *Phosphatides* fall into three groups according to their nitrogen phosphorus ratio as follows: (1) Monoamino monophosphatides, (*a*) Lecithin, (*b*) Kephalin; (2) Diamino monophosphatides—Sphingomyelin; (3) Monoamino diphosphatides—Cuorin. The *Cerebrosides* are only two in number, namely, Phrenosin and Kerasin.

The subject-matter is divided into eight chapters; two of these are devoted to the chemistry methods of extraction and purification of the phosphatides, on all of which questions the author's own researches enable him to speak with authority. Next follows a chapter on the Cerebrosides, and then one dealing with the subject of Protagon, which, after a full description, is dismissed as "nothing more than a mixture of cerebrosides and sphingomyelin with traces of other bodies." Chapter VI. gives an account of a number of alleged lipins such as carnaubon, jecorin, and other "insufficiently characterised substances." The latter is an excellent description for many of the substances described by workers in this extremely difficult field of chemical physiology. Many of the results obtained by different workers are ambiguous or conflicting, and with regard to the biological significance of the lipins, practically nothing is known. Starting with such unpromising material, the author has nevertheless managed to create order out of chaos, and he is to be congratulated on having written a very clear and intelligible account of the present state of our knowledge of this intricate subject.

P. H.

GEOLOGY

Volcanic Studies in Many Lands. Being Reproductions of Photographs taken by the Author. By TEMPEST ANDERSON, M.D., B.Sc., Hon. D.Sc. The Text by T. G. BONNEY, Sc.D., LL.D., F.R.S. Second Series. [Pp. xv + 88, with 82 plates.] (London: John Murray, 1917. Price 15*s*. net.)

THE late Dr. Tempest Anderson was for many years our most successful photographer of volcanic phenomena. Early in 1903 his first volumes of *Volcanic Studies* was published, containing the more interesting results of eighteen years' work. Soon afterwards he retired from medical practice, and was thus able to extend his travels much farther afield. At various times he visited and photographed volcanoes in Mexico, Guatemala, New Zealand, Samoa, Hawaii, Java, and Luzon. It was during the return voyage from the East Indies that he caught

enteric fever, and died on August 26, 1913. His collection of photographs was left to the Yorkshire Philosophical Society, and, under their auspices, this supplementary volume has been issued. The text which accompanies the plates is written by Dr. Bonney, who has also acted as editor.

Recent eruptions of European volcanoes—of Stromboli in 1904, Vesuvius in 1906, and Etna in 1908—provide several illustrations. But the most interesting photographs in the volume are perhaps those of the Souffrière in St. Vincent, Mount Pelée in St. Martinique, and Krakatau. Those of the Souffrière and Mont Pelée were taken in 1907, five years after the great eruptions, and show the forms of the craters and the effects of recent denudation. Krakatau was visited during Anderson's last journey in 1913. The photographs then taken illustrate the cliffs formed during the eruption of 1883, and the return of rich vegetation during the interval of thirty years.

The text differs from that of its predecessor in its more personal tone. Dr. Bonney's interesting accounts are founded partly on studies of the eruptions by various investigators, and partly on the notes made at the time by Dr. Anderson and his companions in travel. They are brief, but they contain all that is essential for the understanding of the plates. The only noticeable omission is that of reference to the eruption of Taal Volcano (Luzon) in 1911, in which, owing to the absence of timely precautions, more than 1,200 persons lost their lives.

To the geologist the illustrations will be even more interesting than those of the earlier volume. They represent volcanic phenomena in more distant and somewhat inaccessible lands; they enable us to realise the rapid effects of denudation on recently ejected materials. As photographs, however, they are, as a rule, inferior. In several plates the detail in the shadow is obscure. In every photographer's collection there are negatives taken under poor conditions on the principle that a photograph taken then may be better than none at all. But Dr. Anderson was an artist as well as a student of volcanoes, and if he had lived to make the selection some of the pictures here reproduced would probably have remained in the privacy of his own collection. There are, however, many of which any photographer might be proud, and several which, if the right of reproduction is allowed, will enrich our textbooks of the future.

The introductory memoir by Mr. George Yeld, which is reprinted with additions from the *Alpine Journal*, gives the impression, so vivid to all who knew Dr. Anderson, of an enthusiastic labourer in his chosen branch of science, and of a man who, both in his private and professional life, never lost an opportunity of doing a kindly action.

C. D.

BOTANY

The Anatomy of Woody Plants. By EDWARD CHARLES JEFFREY. [Pp. x + 478, with 306 illustrations.] (Chicago: The University of Chicago Press, 1917. Price $4 net.)

THIS work scarcely falls into the category of textbooks, and, indeed, its chief value and interest is due to the method of presentation. The anatomical details of the vascular plants are described, but not so much for their own sake as in illustration of the trend of evolution or as evidence in support of some hypothesis. The method adopted is largely comparative, and particular stress is laid on the historical data furnished by the fossil record.

The first half of the book is occupied by an account of the tissues and organs from the anatomical standpoint, whilst the remainder is devoted to a consideration

of special groups and chapters on: Anatomical structure and climatic evolution, Evolutionary principles exhibited by the Compositæ, together with an account of the methods of anatomical technique employed at Harvard University.

In some respects the most interesting chapters are those concerned with the fibro-vascular system. These occupy nearly a quarter of the text, and much detail is here incorporated in the account of the author's views regarding correlation between the appearance of parenchymatous storage elements and the secondary wood, and the phenomenon of annual rings. The formation of wood parenchyma and the occurrence of tangential pitting are held to be concomitants of the seasonal differentiation of the secondary xylem. Phylogenetically the wood parenchyma is considered to have been derived from tracheids, and to have first originated at the tangential face of the summer wood. The diffuse occurrence of wood parenchyma throughout the annual ring, as also of the tangential pitting, is thus viewed as a subsequent extension, leading to the extreme condition in which the parenchyma is confined to the ends of the annual rings and the periphery of the vessels. The radial parenchyma is similarly regarded as a derivative of tracheal elements, and interesting views are advanced, in these and later chapters, as to the relationships of the types of radial parenchyma in woody and herbaceous Angiosperms.

In dealing with the Lycopsida, Prof. Jeffrey advocates the view which regards the pith as of cortical origin. This seems the more remarkable as emanating from one who lays such stress on the primitively microphyllous character of the group in question. The account of the Filicales only occupies fifteen pages in spite of the large amount of comparative work in this field during recent years. In a volume of this character the whole series of fossil Osmundaceous forms might appropriately have been considered, however briefly. All the more as the sole example described, though perhaps most favourable to the author's special views, is scarcely representative of the primitive structure of the group. The earlier *Zalesskya* and *Thamnopteris*, like the Lepidodendrons and Botryopterideæ, strongly support the evolution of a medulla of stelar origin, although there can be little doubt that in more advanced types cortical incursions associated with leaf-gaps augmented the pith thus formed. Prof. Jeffrey, however, appears to regard the medulla as in all cases derived in the latter manner.

The section dealing with the Coniferales is in many respects excellent and affords the author an opportunity of elaborating his well-known views on the primitive character of the Abietineæ. The anatomical structure in this group has received so much attention from Prof. Jeffrey and his school that such a generalised account should prove welcome to the student.

The author is at pains to insist on the elementary character of the work and, as such, opinion will naturally differ as to what should have been included and what omitted. There is, however, an obvious inequality of treatment, for whilst on the one hand certain chapters are somewhat discursive and contain some redundant repetitition, others, notably those on the epidermis and the sporangia, are extremely brief and scarcely commensurate with the importance of their subjects.

The value of the work as an introduction to more specialised study would have been enhanced by the inclusion of literature references, all the more on account of the controversial character of some of the views expressed. The volume is essentially one that will prove most valuable in the hands of the critical reader, and for such the originality of treatment will more than compensate for any shortcomings that may be apparent.

The quality of the illustrations maintains a high standard, both the photographs and drawings being exceptionally clear and well chosen. In particular, the utilisation of perspective diagrams is an admirable safeguard against the growing tendency to view structure in the light of sections and to ignore interpretation in terms of the solid.

E. J. SALISBURY.

Rubber Cultivation in Trinidad and Tobago. Report of the Special Committee of the Board of Agriculture, Trinidad. [Bulletin of the Department of Agriculture, vol xvi. 1917, pp. 95-152.]

RUBBER cultivation having been taken up more actively in Trinidad than in any other West Indian island, this report of the industry is of particular interest, especially as regards the conditions of rubber growing there compared with those in the East. A Committee, including Mr. W. G. Freeman, Acting Director of Agriculture, was appointed in 1916 "to investigate and report upon the present position and prospects of rubber cultivation in Trinidad," and the report now issued is a full and valuable account of the industry.

Two species of rubber trees are chiefly grown in Trinidad, *Hevea brasiliensis* (Para rubber) and *Castilloa elastica* (Central American rubber), and the report shows that, as in the East, Para rubber is by far the most profitable kind to grow. Both species were introduced into Trinidad about forty years ago, but unfortunately preference was given at first to *Castilloa*, although no experimental evidence was available at the time to justify this. It was not until the arrival of the late Mr. Carruthers with his wide knowledge of the rubber industry in the East, that the merits of Para rubber in Trinidad were recognised. The report states that the rate of growth and the yield of latex in some plantations of Para rubber in Trinidad are equal to those obtained on average estates in the East, although the rainfall is often considerably less. The cost of production is reasonable and leaves a good margin of profit with rubber at 2*s*. 6*d*. a pound. Hitherto the trees have been planted too closely, and if the best results are to be obtained the plantations will have to be thinned out until not more than a hundred trees are left to the acre. Although the cost of weeding is less in Trinidad, other labour charges are rather higher than in the East. In order to reduce the cost of production it is suggested that the trees be tapped at intervals of four days, as preliminary experiments show that the yield is as high under this system as when the trees are cut every alternate day, notwithstanding that the highest yields in the East result from tapping every day or on alternate days. Most of the plantations in Trinidad are interplanted with some other crop, Robusta coffee being considered the most suitable for Para rubber. Until the present this kind of rubber has been less subject to root and bark diseases than in the East, but this immunity is somewhat threatened by a leaf disease which has been troublesome in Guiana and has recently appeared in Trinidad.

Castilloa, on the other hand, has been a disappointment. The report states that the planting of this variety as a pure crop is not profitable, and that its further planting, even as shade for cacao, is of doubtful benefit. Where already established, *Castilloa* can be tapped with profit, although the yield is much less than from *Hevea*. Unfortunately an efficient method of tapping *Castilloa* rubber has not been discovered, and frequent tappings of adjacent areas of bark do not give a steady flow of latex as in *Hevea*. At present the best way of tapping *Castilloa* is to cut the bark with a cutlass two or three times a year; most of the latex coagulates on the trunk and the rubber is collected as scrap.

Now that it is clear that Para rubber is the best variety to grow in Trinidad, this planting industry can be extended on a proper basis, and its development will make for stability in agricultural conditions in this important island. The exploitation of the tropics will soon be renewed with increased vigour, and the publication of this report shows that Trinidad is alive to the great possibilities of tropical agriculture under scientific direction.

F. T. BROOKS.

Soil Biology: Laboratory Manual. By A. L. WHITING, Ph.D. [Pp. x + 143.] (London: Chapman & Hall; New York: John Wiley & Sons. Price 6*s*. net.)

THIS book, as indicated in the sub-title, is essentially a laboratory handbook, and by far the greater part is devoted to the description of experiments, or "practices" as they are here termed, to be carried out by the student. Of these there are thirty-three in all, of which number most deal with soil bacteria and their products; two are concerned with the soil fungi; one with algæ; and one with protozoa. The directions for carrying out each experiment are followed, in every case, by a few references to appropriate literature and several questions.

In the second part, which comprises about one-third of the whole, the methods employed in soil biology are described. These include tabulated formulæ and general information respecting culture media, stains for bacteria, quantitative and qualitative determination of nitrogen compounds and other substances, methods of pot culture, etc.

There is a very full table of contents which in a great measure atones for the absence of an index. The manual is well suited for students already acquainted with bacteriological and chemical methods, but is not intended for those who have not such preliminary training. Even, however, at the risk of the volume being less handy for laboratory use, rather more detailed directions might perhaps have been given so as to have rendered the admirably conceived exercises available to a wider range of students.

E. J. SALISBURY.

The Dissemination of Parasitic Fungi and International Legislation. By E. J. BUTLER, M.B., F.L.S. Memoirs of the Department of Agriculture in India. Vol. IX., No. 1. [Pp. 73.] (Calcutta: Thacker, Spink & Co. Price 2*s*. net.)

MR. BUTLER, after emphasising the importance of dissemination by means of the mycelium carried in the living plant tissue in certain cases (*e.g.* Potato Blight), deals with the more frequent mode of dispersal by means of spores. These latter are produced in prodigious numbers by many fungi and, by their capacity for withstanding desiccation and their small size, are eminently suited for conveyance by air currents or carriage by animal agency. These means of dispersal are probably the only ones effective over long distances, although other agents come into play within a short distance of the source of infection.

It is chiefly with the long-distance spread that Mr. Butler is concerned. Evidence, mainly circumstantial, is advanced to support the view that fungal spores are seldom carried long distances (more than about 50 miles perhaps) by wind agency.

Thus, the chestnut-free belt of the Catskill Mountains appears to offer an impassable barrier to the chestnut-bark disease; the known history of the spread

of gooseberry mildew indicates distribution from infected nurseries rather than carriage by wind; the blister blight of tea failed to reach Darjeeling for at least forty years after its appearance in Assam; *Pythium palmivorum* spread centrifugally from Godavri at the rate of from 1 to 3 miles per annum, but the spread was always continuous without intervening uninfected areas. Other instances are afforded by the vine and oak mildews, the carnation rust, the coffee-leaf disease, and the hollyhock rust.

The geographical distribution of various genera supports the same contention that long-distance carriage by air currents rarely occurs. Of the examples cited we may quote that of *Uromyces*, of which genus 119 species occur in Europe and 249 in America; only four or perhaps six are common to both regions, and these are parasitic on cultivated plants.

Such evidence suggests that man has played an important part in the transmission of fungal pests, on or in living plant material, over long distances—a rôle that increases with the increased facilities for transport. Mr. Butler points out that once a parasite has been introduced into a new area its extermination is extremely difficult, so that some check on the transport of infected material is becoming more and more essential.

In this connection the recommendations of the International Phytopathological Conference held at Rome in 1914 are considered, especially in their relation to India. Briefly, the object of the Convention drawn up was to control the sanitary condition of imported horticultural produce. Certificates would be granted by Government inspectors that the exported plants were free from the diseases enumerated in a list furnished by the importing country.

The author aptly points out the danger of any attempt to standardise remedial measures which might accrue from official interference. The criticism is advanced that the Convention makes no discrimination between continuous and discontinuous dissemination. Also in view of the marked variation in virulence of the same parasite in different countries, the proviso that only such diseases may be listed by any country as are very harmful or destructive is open to grave objection.

Whilst broadly the author approves of the application of the Convention to India, he offers pertinent suggestions and criticisms of very general application which should be read by all interested in this extremely important subject.

E. J. SALISBURY.

AGRICULTURE

Annuaire Internationale de Statistique Agricole, 1915 et 1916. Institut International d'Agriculture. Service de la Statistique Générale. [Pp. xlix + 949.] (Rome: Imprimerie de l'Institut, 1917. Price 10 frs.)

VARIOUS additions and extensions have been made in the preparation of this new census, which now displays the state of the world's agricultural industry from 1907 to 1916 inclusive, making it still more valuable as a standard work of reference.

The table of contents is given in five languages, followed by ten pages of introduction to the 836 tables of statistics, which make the volume up to a thickness of a little more than three centimetres. Preceding them is a notice calling attention to the notation by which the compilers distinguish between official data, official estimates, and private data, and to the quinquennial averages.

The tables embody statistics of area and population, the classification of areas as productive and non-productive, and the further sub-division of the productive areas according to the type of cultivation. The various crops of the world are

then dealt with one by one, giving area, yield, and production of finished material such as olive oil, silk cocoons, or ginned cotton; it is not yet possible to include tea, cocoa, rubber, jute, or gutta-percha in this section, because the available data have been found too imperfect to justify publication. It is to be hoped that the industries in question will help to remedy this omission.

International trade statistics are abstracted in regard to the principal crops, and data for consumption, price, ocean freight, and rates of exchange follow; this section especially is more fully developed than in the 1913-14 issue. The last section deals with production and prices of fertilisers.

The list of authorities cited is in itself most useful as a reference for the economist; taking the countries of the world one by one, and commenting where necessary on the statistical value of the data provided, it covers a hundred pages.

L. B.

ZOOLOGY

Organic Evolution. A Textbook. By PROF. R. S. TULL, PhD. [Pp. xviii + 729, with 253 figures and 30 plates.] (New York: The Macmillan Company, 1917. Price 16*s*. net.)

IT is strange, as the author points out in his introduction, that, in spite of the large number of books that have been written on evolution and allied subjects, very few have come from palæontologists, yet they deal with the material that provides the most direct proof of the phenomenon. A book, therefore, coming from such a well-known authority as Prof. Tull is doubly welcome, and it is one that will prove of great interest and use to all students of biology. The reviewer, unfortunately, has not merely to praise or condemn a book as a whole, but if the former, as is undoubtedly the case with the present volume, also to indicate points that do not seem in keeping with the general high level of the work. On p. 199 *Volvox* is referred to as a form showing the beginning of the differentiation of the germ cells from the soma, but even more might be made of it, for according to recent authorities still further specialisation is to be found among the cells of the "soma." The account of the "air bladder" (*sic*) given on p. 318 suggests that its action depends entirely upon the contraction of the body muscles. This, however, is misleading, for it does not take into account the extraordinary rete mirabile and gas gland with which that structure is provided nor the fact that it does not contain air but a mixture of oxygen and nitrogen secreted and re-absorbed as required. No histologist could let pass the footnote on p. 539 that "All bone consists first of cartilage." A further statement cannot pass without challenge, and that is that the lower jaw and tooth found in association with the Piltdown skull does not belong to it and "is not even human but is that of a fossil chimpanzee." This is a view which, for some unaccountable reason, has found favour in America. The probability of such a fortuitous association is infinitely remote, and, moreover, the jaw, which fits the skull, as near as can be ascertained, quite well—again implying a marvellous coincidence—is utterly unlike that of any chimpanzee, as a brief examination of the actual specimen will show. If the jaw does not belong to the skull, we have in the Piltdown relics a still more remarkable find, for, in addition to a peculiar type of human cranium, we have a jaw of an entirely new genus of anthropoids.

Apart from these matters for criticism, which are, after all, only of small moment in view of the wide ground covered by the book, we certainly have one of the most useful general accounts of evolution that have been published for some

years. It is plentifully supplied with good illustrations, which still further illumine the already lucid text. The author has adopted the praiseworthy plan of adding at the end of each chapter a short bibliography, which, with the copious index, increase the utility of the book considerably. The whole subject-matter is treated from the point of view of a palæontologist, and therein lies its chief value, for its author is not merely an authority on this subject but also wields a facile pen. During the past few years several books have appeared on the fossil history of certain groups of vertebrates, mostly by American authors, but this is the first giving a survey of modern knowledge derived from palæontology and applied to the whole animal kingdom. The author is undoubtedly to be congratulated on the production of a book that will instruct and delight a wide circle of readers, including all interested in general biology.

C. H. O'D.

A Year of Costa Rican Natural History. By A. S. Calvert and P. P. Calvert. [Pp. xix + 577, with 142 illustrations and a large map.] (New York: The Macmillan Company, 1917. Price $3.)

Professor and Mrs. Calvert spent a year in 1909–10 in Costa Rica, mainly with the object of collecting and studying the dragonflies of the region. This they did with very considerable success, and were able to add a number of new species and an amount of knowledge about the distribution and mode of life of a large number of others. The country is apparently rich in Odonata, and the authors made the most of their opportunities. That they did not confine themselves to these insects alone, but observed the general flora and fauna of the district, is evident from reading these pages. Perhaps the group that was treated least was the vertebrates, and the references to them are few. A brief account of the topography and means of communication makes the various journeys undertaken easy to follow. The book on the whole is written in a clear manner and interest is maintained throughout, and many points of topography or species of animals and plants are brought home vividly by an excellent series of photographs.

A number of appendices give the itinerary, the weather conditions at Cartago which was made the headquarters of the trip, a list of papers based in whole or in part on the material collected by the authors, and a selected literature list of the works dealing with the natural history of Costa Rica. These, together with an excellent index, render the book useful for reference, and indeed it should prove indispensable to any naturalists subsequently collecting or travelling in the country.

The book does not pretend to make any large contribution to theoretical biology, although such matters as distribution, mimicry, the colonial ants, etc., are bound to be introduced. It is, however, a straightforward and quite readable account of the observations made by two very skilled naturalists during their year's stay in a country rich in animal and plant life, and its contents are admirably expressed by the title, *A Year of Costa Rican Natural History*. The year passed tranquilly in collecting and journeying, but the exit was dramatic enough for a tragedy, for the authors got back to Cartago a day or so before the terrible earthquake that devastated that city in 1910. They were actually there at the time, and give a graphic account of their experiences. Fortunately for the reader as well as themselves they and their collections were unharmed, and they left the city when the earthquake was passing away.

C. H. O'D.

Moth Borers affecting Sugar-cane in Mauritius. By D. D'EMMEREZ DE CHARMOY. Department of Agriculture, Mauritius, *Bulletin*, No. 5, Scientific Series. [Pp. 27, with 6 plates and 1 chart.] (Mauritius: The Government Press, Port Louis, 1917.)

THE publications of the Department of Agriculture, Mauritius, are issued in three series—General, Scientific, and Statistical—as occasion demands. The present bulletin is the second of an entomological nature, its precursor, "Insects Injurious to Stored Grains," being No. 2. of the Scientific Series. In it the author gives the results of three years' investigation of the borers affecting sugar-cane in Mauritius. Four species of Lepidoptera are inculpated—the Pink Borer (*Sesamia vuteria*, Stoll.), the Spotted Borer (*Diatrea sacchariphaga*, Boyer), the White Borer (*Grapholita schistaceana*), and the Brown Borer (*Alucita sacchari*, Bojer). With the exception of the last-named species, which sometimes causes damage to cuttings, these insects are of considerable economic importance and are responsible for serious annual losses to the sugar-cane crop. *A. sacchari* also is the only indigenous species; *D. sacchariphaga* was introduced from Ceylon in 1848 and for a few years proved a regular scourge to the sugar industry. It has become less destructive of late years, however, and, although the damage it does to the mature canes is still substantial, it has now restricted itself within limits which cause no alarm. *S. vuteria* is at present the most harmful species and, like *G. schistaceana*, was introduced unnoticed subsequent to *D. sacchariphaga*. M. Charmoy has carefully worked out the life histories of these moths and has conducted trials, in the experimental fields of the Department, with several methods for the establishment of control measures. Maize was found to be a highly efficient plant trap for the Pink Borer, and has been adopted extensively with good results. The author has also made a close study of the natural parasites of these pests, and large numbers of two species of Chalcids (*Trichogramma australicum*, Gir., and an unidentified species of *Telenomus*, which is considered one of the most valuable natural enemies of *S. vuteria*), parasitic in the eggs of the borers, have been bred in the laboratory and distributed throughout the island.

H. F. C.

The Objects and Work of the Royal Italian Oceanographic Committee. By GIOVANNI MAGRINI. [Pp. 118, with 15 plates and maps.] (Published by the Royal Italian Oceanographic Committee, Venezia. Prem. Officine Grafiche di C. Ferrari. 1916.) This publication in the English language constitutes the twenty-first memorial of the Committee.

THIS work contains an account of the institution and aims of the Royal Italian Oceanographic Committee, founded in 1910 for the exploration of the Adriatic and other waters within what may be called the Italian sphere of influence. The Central Institute for Marine Biology is situated at Messina, overlooking the Strait. This building, of handsome appearance and sound design, is fully described with plans and illustrations. A number of quarterly cruises were undertaken between August 1909 and April 1914, all in the Adriatic, and in addition there were six special biological cruises in the Libyan, Tyrrhenian, Ionian, and Albanian seas. A good deal of the material collected on these cruises has already been investigated and a long list of publications is given. The physico-chemical researches were carried on at the Chemical Institute of the University of Padua under the direction of Prof. Bruni, and tide researches were

undertaken by the Italian Mareographic Commission. The present volume is in the nature of a prospectus and summary of the work contemplated and already performed. For the details reference should be made to the works mentioned in the bibliography. There is a short description, with plans, of the vessel built for the oceanographic work by the Co-operative Society of Sampierdarena. All this work has obviously been terminated, we trust only temporarily, by the war; but enough has been outlined in the volume under review to show that the Italians are keenly alive to the importance of oceanographical and marine biological research.

J. T. J.

MISCELLANEOUS

The Limits of Pure Democracy. By W. H. MALLOCK. [Pp. xx + 397.] (London: Chapman & Hall. Price 15*s*. net.)

IN these days, when democratic government is considered by many to be the panacea for all ills, it is as well that the meaning of a word in such common use should be fully understood by those who profess to be adherents to democratic forms of government. The whole aim of the book is to prove that democracy is possible only in small communities, and that oligarchy is the ideal, and indeed, the only possible form of government in large states. For example, the theory that democracy is government *of* the people, *for* the people, *by* the people, a theory that never fails to produce instant cheers, may be reduced to the following propositions: that democracy in any concrete case—in the case of France, for instance—is government which is exercised over the French people—that is, exercised by the people of France; and that it is exercised by the people of France with a view to their own advantage. The chief attraction of these otherwise barren platitudes, says the writer, appears to be the word "people," though the word in itself has no real meaning whatever. The writer cites several arguments to support his theory that a pure democratic will is possible only as to very simple questions. Even in an argument in the taproom of a public-house, two or three individuals with a better knowledge of the affair concerned or with shrewder intellects are able to sway the opinions of the remainder of the debaters.

The fifth chapter of the first section (which deals with Political Democracy) is perhaps the most interesting and considers revolutionary oligarchies. That trade unions have developed into oligarchies in England is well known, but that Lassalle and Proudhon admitted that purely democratic wills must be merged in those of the leaders is much less widely known. The Gallic tribes which existed in the times of Cæsar were democracies in times of peace, and oligarchies in times of war.

The second section of the book deals with democracy as applied to technical production. Mr. Mallock scrutinises the meaning of the word "industry," and questions the Marxian doctrine that manual labourers are the sole and equal producers of wealth.

The next section deals with the distribution of wealth. The enormous amount of unearned income which haunts the brain of many a socialist is shown to be about threepence a day per inhabitant of the United Kingdom. Many of the other figures are also very interesting.

In Section IV. the second chapter dealing with the various socialist experiments is also interesting; most of these experiments were in the United States, but all were unsuccessful. Some, like the Rappites, developed into wealthy plutocracies,

while others, like the Brook Farm, were entire failures. The most celebrated of all these experiments was probably the "New Australia" community in Paraguay, and the story of its birth and eventual collapse is highly amusing in some places.

The writer discusses in the next two sections the minimum wage and the effects of the false statistics of Marx and Henry George. He attributes the desire for absolute equality to artificial inequality such as existed in France prior to the first revolution. Mr. Mallock also discusses "Equality of Opportunity," one of the chief demands of all revolutionaries. He also suggests reforms for all these grievances.

In the seventh and last section Mr. Mallock discusses among other things the Russian revolution, to which, unfortunately, the writer has had but small space to devote, as the book was nearly complete at the time: such as he has written, however, is terse and well expressed.

C. C. ROSS.

Britain's Heritage of Science. By ARTHUR SCHUSTER, F.R.S., and ARTHUR E. SHIPLEY, F.R.S. [Pp. xv + 334, with 15 portraits.] (London: Constable & Co., 1917. Price 8*s.* 6*d.* net.)

IF it be true that good wine needs no bush, then, indeed, this book needs no recommendation. From such apparently unpromising sources as the *Encyclopædia Britannica* and the *Dictionary of National Biography* the authors have produced a valuable and fascinating record of the work of British scientists. Nor is it merely a catalogue of achievement; space is found for personality and anecdote, and the larger part of the book, devoted to Physical Science, might almost claim to be a history of the science itself, so overwhelmingly large is the British share in its development. Mathematics is not included directly, nor is the work of living men discussed save in the few cases where the omission would have conveyed a false impression of the part which Great Britain has played in the recent progress of science. Some abridged chapter headings will, perhaps, best indicate the scope of the book: The Ten Landmarks of Physical Science—dealing with the main stream of research from Roger Bacon through Gilbert, Newton, Dalton, Faraday, Kelvin, and Clerk Maxwell; The Heritage—academic and non-academic—through the seventeenth, eighteenth, and nineteenth centuries; Industrial Applications; Scientific Institutions; Biology in the Middle Ages; and then chapters devoted severally to Botany, Zoology, Physiology, and Geology.

With such a wealth of material at hand it is difficult to make a selection for quotation or comment. One must suffice—Newton's own account of his discoveries quoted from an MS. among the Portsmouth Papers:

> ". . . The same year, in May, I found the method of tangents of Gregory and Slusius, and in November had the direct method of fluxions, and the next year, in January, had the theory of colours, and in the May following I had entrance into the inverse method of fluxions. And the same year I began to think of gravity extending to the orb of the moon, and . . . from Kepler's rule . . . I deduced that the forces which keep the planets in their orbs must be reciprocally as the squares of their distances from the centres about which they revolve. . . . All this was in the two Plague years of 1665 and 1666, for in those days I was in the prime of my age for invention, and minded mathematics and philosophy more than at any time since."

Newton was twenty-five years old at the commencement of this period and in his fourth year at Cambridge, which he entered in 1661, "not being considered fit to be a farmer," as his father was.

D. O. W.

The Life of Sir Colin C. Scott Moncrieff, K.C.S.I., K.C.M.G., R.E., LL.D., etc. Edited by his niece, MARY ALBRIGHT HOLLINGS. [Pp. xii + 374, with illustrations, genealogy, and index.] (London: John Murray, 1917. Price 12*s*. net.)

THE significance of the life here recorded is best summarised in Sir Colin's question: "Suppose you had a fresh visitation of famine, would you be more ready than you were last time to fight it?" Our sheltered nations till lately had forgotten that all food comes from the plants in the soil, and thought that mechanism was Progress; yet "of course the railways are invaluable for carrying food to where it is wanted, but only irrigation creates food," as he writes when nearing the end of a life spent chiefly between naked earth and glaring sky.

As an account of the kind of work done by one of those men who have brought honour to the Empire by unselfish, far-sighted labour for her subject races, on which labour its security depends, this volume has much more than a personal interest; it is a book to be read by every novice starting for civil service abroad. The story is told very largely by Sir Colin's own words, skilfully woven, and this restrained treatment makes the effect the more striking. A career which participated in the Indian Mutiny, in the Madras Famine of 1877-8—those pages should be read together with Kipling's story of another Scott in "William the Conqueror"—and in the Delhi Durbar of 1903; which completed its full service in the Indian Irrigation Department before taking charge for ten years of the waters of the Nile, and so laying the foundation of Egypt's modern prosperity; which after ten years more in the Scottish Office presided over the Indian Irrigation Commission; which knew Transcaucasia, Lombardy, South Africa, and America, and lasted from 1836 to 1916, was filled with human interest.

In writing to General Gordon in 1876 he modestly says: "I cannot pretend to any scientific knowledge," but although it is true that his life work was primarily the administration of technical processes with notable engineering courage, rather than the deliberate study of first principles, yet one technical discovery, not specially emphasised in this volume, is alone sufficient to entitle him to scientific status—if a President of the Engineering Section of the British Association needed it—namely, his clear recognition of the importance of humble drainage equally with water-supply. We find this in his Egyptian Public Works Reports from 1882 to 1892, and it is not unreasonable to grudge the ten years he gave to the Scottish Office; for, had those further years been spent in Egypt, the developing irrigation projects of America, India, and Egypt, too, might have been saved from temporary failure to recognise that "it would be more difficult to drain the water off the land than to pour the water over the land" (p. 175).

The work of Sir Colin Scott Moncrieff stands in such fundamental relationship to the last half-century of effort in irrigation, that we wish it might be found possible to prepare a companion volume to the present *Life*, dealing more technically with aspects, developments, and consequences of his work. Much of the material is already available in official reports, lectures, or books, and most of his assistants are still with us. In any case we can commend this book to all who desire to learn how good principles may be put into good practice.

L. B.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Frequency Curves and Correlation. Addendum, with Diagram and Errata. By W. Palin Elderton, F.I.A. London: Printed for the Institute of Actuaries by Charles and Edwin Layton, 1917. (Pp. 22.) Price 1*s*. 6*d*. net.

This pamphlet is published with the idea of bringing up to date the author's book on *Frequency Curves and Correlation*, which was published in 1906. There are three sections, respectively on "Uncommon Frequency Types," "Calculation of the Co-efficient of Correlation in a Two-row Table," and "The Correlation Ratio," and there is also a list of Errata in the book mentioned. It is to be noticed that special attention is given to recent work by Prof. Karl Pearson.

Analytic Geometry and Calculus. By Frederick S. Woods and Frederick H. Bailey, Professor of Mathematics in the Massachusetts Institute of Technology. London: Ginn & Co., and Boston, New York, and Chicago. (Pp. xi + 516.) Price 12*s*. 6*d*. net.

Theory of Maxima and Minima. By Harris Hancock, Ph.D. (Berlin), D.Sc. (Paris), Professor of Mathematics in the University of Cincinnati. London: Ginn & Co., and Boston, New York, and Chicago. (Pp. xiv + 193.) Price 10*s*. 6*d*. net.

Les Sciences Mathématiques en France depuis un Demi-Siècle. By Émile Picard, Secrétaire Perpétuel de l'Académie des Sciences. Paris: Gauthier-Villars et Cie., Editeurs, Libraires du Bureau des Longitudes, de l'École Polytechnique, 55, Quai des Grands-Augustins. (Pp. 25.)

Mathematics for Engineers. Part I. Including Elementary and Higher Algebra, Mensuration and Graphs, and Plane Trigonometry. By W. N. Rose, B.Sc., Eng. (Lond.), Lecturer in Engineering Mathematics at the University of London, Goldsmiths' College. London: Chapman & Hall, 11, Henrietta Street, W.C.2, 1918. (Pp. xiv + 510.) Price 8*s*. 6*d*. net.

Recent Progress in Magneto-Optics. Indian Science Congress (Section—Physics and Mathematics). Presidential Address by Dr. Wali Mohammad, M.A. (Punjab), B.A. (Cantab.), Ph.D. (Gottingen), Fellow of the Allahabad University, Professor of Physics, Aligarh. Delivered at Lahore, January 1918. Printed by M. Muqtada Khan Shirwani, at the Institute Press, Aligarh. (Pp. 20.)

Experimental Inorganic Chemistry. By Alexander Smith, Professor of Chemistry, and Head of the Department, Columbia University. Sixth Edition. London: G. Bell & Sons, 1918. (Pp. vii + 171.) Price 3*s*. 6*d*. net.

Introduction to Inorganic Chemistry. By Alexander Smith, Professor of Chemistry and Administration Head of the Department, Columbia Univer-

sity, New York. Third Edition. London: G. Bell & Sons, 1918. (Pp. xiv + 925.) Price 8*s.* 6*d.* net.

A Laboratory Outline of College Chemistry. By Alexander Smith, Professor of Chemistry and Head of the Department, Columbia University. London: G. Bell & Sons, 1918. (Pp. v + 206.) Price 3*s.* net

This is intended for use either with Prof. Smith's "Introduction to Inorganic Chemistry," or his "General Chemistry for Colleges," the three volumes forming an excellent working library for the chemical student. Cross references to the three volumes are given, and the book should prove of great value to those teaching or learning inorganic chemistry.

Lectures in the Principle of Symmetry and its Applications in all Natural Sciences. By F. M. Jaeger, Ph.D., Professor of Inorganic and Physical Chemistry in the University of Groningen, Holland. Amsterdam: Publishing Company "Elservier," 1917. (Pp. xii + 33, with 170 diagrams.)

What Industry owes to Chemical Science. By Richard B. Pilcher, Registrar and Secretary of the Institute of Chemistry of Great Britain and Ireland, and Frank Butler Jones, B.A. (Cantab.), A.I.C., Assistant, Laboratory of the Department of Explosive Supplies, Ministry of Munitions. With an Introduction by Sir George Beilby, LL.D., F.R.S., Past President of the Institute of Chemistry and of the Society of Chemical Industry. London: Constable & Co., 10, Orange Street, Leicester Square, W.C., 1918. (Pp. xiv + 150.) Price 3*s.* net.

Cellulose. An Outline of the Chemistry of the Structural Elements of Plants with reference to their Natural History and Industrial Uses. By Cross & Bevan (C. F. Cross, E. J. Bevan, and C. Beadle). New Impression with a Supplement. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. xviii + 348.) Price 14*s.* net.

This is a new impression of the 1916 edition, with a Supplementary Chapter dealing with the more important developments in the theory and applications of the subject during the last two years.

Organic Compounds of Arsenic and Antimony. By Gilbert T. Morgan, D.Sc., F.R.S., F.I.C., M.R.I.A., A.R.C.Sc., Professor of Applied Chemistry, City and Guilds Technical College, Finsbury, formerly Professor in the Faculty of Applied Chemistry, Royal College of Science for Ireland, and Assistant Professor in the Imperial College of Science and Technology; Corresponding Member of the Royal Dublin Society. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. xx + 376.) Price 16*s.* net.

The Manufacture of Intermediate Products for Dyes. By John Cannell Cain, D.Sc. (Manchester), Editor of the *Journal of the Chemical Society*, Examiner in Coal-Tar Colouring Matters to the City and Guilds of London Institute; late Member of the Technical Committee of British Dyes, Ltd., and Chief Chemist of the Dalton Works, Huddersfield. London: Macmillan & Co., St. Martin's Street. (Pp. xi + 263.) Price 10*s.* net.

Edible Oils and Fats. By C. Ainsworth Mitchell, B.A., F.I.C. London: Longmans, Green & Co., 39, Paternoster Row, E.C., and New York, Calcutta, and Madras, 1918. (Pp. xii + 160.) Price 6*s.* 6*d.* net.

The Book of the Rothamsted Experiments. By A. D. Hall, M.A. (Oxon.), F.R.S., late Director of the Rothamsted Experimental Station, First Principal of the South-Eastern Agricultural College, now Secretary of the Board of Agriculture. Second Edition. Revised by E. J. Russell, D.Sc., F.R.S., Director of the Rothamsted Experimental Station. Issued with the authority of the Lawes Agricultural Trust Committee. London: John Murray, Albemarle Street, W., 1917. (Pp. xl + 332.) Price 10*s*. 6*d*. net.

The Potato. By Arthur W. Gilbert, Ph.D., Professor of Plant-Breeding, New York State College of Agriculture at Cornell University, assisted by Mortier F. Barrus, Ph.D., Professor of Plant Pathology, New York State College of Agriculture at Cornell University, and Daniel Dean, formerly President of New York State Potato Association. New York: The Macmillan Company, 1917. (Pp. xii + 318.) Price $1.50.

Tidal Lands. A Study of Shore Problems. By Alfred E. Carey, M.Inst.C.E., Fellow of the Royal Geographical, Geological, and Chemical Societies, and F. W. Oliver, F.R.S., Quain Professor of Botany in University College, London. London: Blackie & Co., 50, Old Bailey, E.C., and Glasgow and Bombay, 1918. (Pp. xiv + 284, with 29 plates and 54 figures in the text.) Price 12*s*. 6*d*. net.

School Entomology. An Elementary Textbook of Entomology, for Secondary Schools and Agricultural Short Courses. By E. Dwight Sanderson and L. M. Peairs. New York: John Wiley & Sons; London: Chapman & Hall, 1917. (Pp. vii + 356.) Price 7*s*. net.

Studies in Insect Life and other Essays. By Arthur Everett Shipley, Sc.D., F.R.S., Master of Christ's College, Cambridge. London: T. Fisher Unwin, Adelphi Terrace. (Pp. xi + 338, with 11 illustrations.) Price 10*s*. 6*d*. net.

A Check List of North American Amphibians and Reptiles. By Leonhard Stejneger and Thomas Barbour. Cambridge, U.S.A.: Harvard University Press, 1917; London: Oxford University Press, Amen Corner, E.C. (Pp. iv + 125.) Price 10*s*. 6*d*. net.

Records of the Indian Museum. Vol. XIII., Part VI., December 1917. (Pp. 307—418.) Price 2 rupees, and Vol. XIII., Part XI., February 1918. Zoological Results of the Abor Expedition, 1911–12. (Pp. 581—600.) Price 2 rupees.

The Megalithic Culture of Indonesia. By W. J. Perry, B.A. Manchester: at the University Press, 12, Lime Grove, Oxford Road; London: Longmans, Green & Co., 1918. (Pp. xiii + 198.) Price 12*s*. 6*d*. net.

Contributions from the Walker Museum. Vol. II., No. 4. (1) The Evolution of Vertebræ. (2) The Osteology of some American Permian Vertebrates, III. By Samuel Wendell Williston. Chicago, Illinois: The University of Chicago Press. (Pp. 75 + 112, with 4 plates.) Price $0.52, post paid.

Aeronautics in Theory and Experiment. By W. L. Cowley, A.R.C.Sc., D.I.C., Whitworth Scholar, and H. Levy, M.A., B.Sc., F.R.S.E., Carnegie Research Fellow, late 1851 Exhibitioner and Ferguson Scholar. London: Edward Arnold, 1918. (Pp. xi + 284.) Price 16*s*. net.

Directions for a Practical Course in Chemical Physiology. By W. Cramer, Ph.D., D.Sc., M.R.C.S., L.R.C.P. Third Edition. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1917. (Pp. viii + 119.) Price 3*s.* net.

The Spleen and Anaemia. Experimental and Clinical Studies. By Richard Mills Pearce, M.D., Sc.D., Professor of Research Medicine, with the Assistance of Edward Bell Krumbhaar, M.D., Ph.D., Assistant Professor of Research Medicine, and Charles Harrison Frazier, M.D., Sc.D., Professor of Clinical Surgery, University of Pennsylvania. Philadelphia and London: J. B. Lippincott Company. (Pp. x + 419, with 16 illustrations, colour and black-and-white.) Price 21*s.* net.

Tumours, their Nature and Causation. By W. D'Este Emery, M.D., B.Sc. (Lond.), Director of the Laboratories, King's College Hospital, Captain R.A.M.C. (T.F.). London: H. K. Lewis & Co., 136, Gower Street, W.C. (Pp. xx + 146.) Price 5*s.* net.

The South African Institute for Medical Research. Nos. VI., VII., VIII., IX., and X. Published by the Institute, Johannesburg. Printed by W. E. Horton & Co., Johannesburg.

An X-ray Atlas of the Skull. By A. A. Russell Green, M.B., B.S. (Lond.), M.R.C.S. (Eng.), Captain R.A.M.C. (T.), M.O. in charge of X-ray Dept., 2/1 Southern General Hospital, Radiographer to Birmingham Skin Hospital, Radiographer to Birmingham Board of Guardians. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. x + 27, with 5 coloured plates and a table showing relations between displacement of shadows and distance of bodies throwing those shadows.) Price 10*s.* 6*d.* net.

Medical Contributions to the Study of Evolution. By J. G. Adami, M.D., F.R.S., F.R.C.P. London: Duckworth & Co., 3, Henrietta Street, Covent Garden, W.C., 1918. (Pp. xviii + 372, with 7 Plates and 20 Figures in the Text.) Price 18*s.* net.

An Enquiry into the Analytical Mechanism of the Internal Ear. By Sir Thomas Wrightson, Bart., M.Inst.C.E., with an Appendix on the Anatomy of the Parts Concerned by Arthur Keith, M.D., F.R.S. London: Macmillan & Co., St. Martin's Street, 1918. (Pp. xi + 254, with 9 plates.) Price 12*s.* 6*d.* net.

Elements of Constructive Philosophy. By J. S. Mackenzie, Litt.D. (Camb.), Hon. LL.D. (Glasgow), Emeritus Professor of Logic and Philosophy in University College, Cardiff, formerly Fellow of Trinity College, Cambridge. London: George Allen & Unwin; New York: The Macmillan Company. (Pp. 487.) Price 12*s.* 6*d.* net.

Seventy-ninth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England and Wales (1916). Presented to Parliament by Command of His Majesty. London: Published by His Majesty's Stationery Office, 1918, Cd. 8869. (Pp. xcii + 492.) Price 5*s.* net.

Annals of the Durban Museum. Vol. II., Part I. Edited by the Curator, E. C. Chubb. Issued December 28, 1917. Printed by John Singleton & Sons, Durban, for the Durban Museum. (Pp. 46.) Price 5*s.* net.

The Year-Book of the Scientific and Learned Societies of Great Britain and Ireland. A record of the work done in Science, Literature, and Art during the Session 1916-17, by numerous Societies and Government Institutions. Compiled from Official Sources. Thirty-fourth Annual Issue. London: Charles Griffin & Co., Exeter Street, Strand, W.C.2. (Pp. viii + 334.) Price 9*s*. net.

The Athenæum Subject-Index to Periodicals, 1916. Science and Technology, including Hygiene and Sport. Issued at the Request of the Council of the Library Association. London: The Athenæum, Bream's Buildings, Chancery Lane, E.C.4; New York: Messrs. B. F. Stevens & Brown, 16, Beaver Street. (Pp. 162.) Price 10*s*. net.

ERRATUM

In the January number of SCIENCE PROGRESS the denominator of the formula on p. 507 should read:

$$d^{2+(4d_a d^{-1})}$$

instead of

$$d^2 + (4d_a d^{-1})$$

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

RECENT ADVANCES IN SCIENCE

PHILOSOPHY. By Hugh Elliot.

The recent tendencies of philosophical speculation have been strongly in the direction of spiritualistic, as opposed to scientific, explanations. Philosophy is a general name for a wide range of different topics, which have little in common beyond the quality of being very obscure and inaccessible to the ordinary weapons of logic. It thus comprises within its purview some subjects on which so much knowledge has already been accumulated as almost to justify their incorporation into the body of natural science. It comprises some other subjects which are condemned by increasing knowledge and are passing into the sphere of acknowledged superstition. It is upon this less reputable department of philosophy that public attention has lately centred ; and we shall be obliged to devote the greater part of this Review to the subject of Spiritualism.

Not that philosophy has been wholly stagnant during the war. Notable advances have been made in Psychology, owing to the great wealth of new material furnished by the study of shell-shock. This peculiar manifestation of hysteria has provided abundant opportunities for hypnotic experimentation, and for testing the theories of Freud and Jung, which had so great a vogue before the war. The doctrines of psycho-analysis and suppressed consciousness have passed with exceptional rapidity through the ordinary stages of a new sphere of thought. A period of incubation and neglect was succeeded by a hail of applause, a sudden leap of the new ideas into fashion. This was followed by intensive criticism, which, fastening upon the most vulnerable features of the theory, seemed at first to discredit the whole. And finally there has supervened a calmer atmosphere, which, while totally rejecting much of the new psychology, accepts a large part as constituting real and important progress. It has now come to be generally held that Freud enormously over-estimated the influence of sexuality in determining hysterical manifestations. When once we cut out this preoccupation with sex, we find much of the highest value

in his doctrines, which confirm and supplement the writings of Janet.

It is, however, not in these higher grades of philosophy that the public have been interested. Philosophy at all times has been the arena of a never-ending battle between emotion and intellect. Man is born with powerful desires and cravings; when his mind is undisciplined, he accepts his desires as the criterion of truth. He ardently wishes a certain theory to be true, and he forthwith affirms that it *is* true. You produce facts which show that it cannot be true, and he refutes you by calling you a materialist, and by refusing to admit the facts into his mind. The infinite tragedy of the war—a tragedy so great that a human mind can no more conceive the miseries of it than it can conceive the distance of a fixed star—has brought in its train a powerful set of desires, perfectly natural, perfectly intelligible, but strongly reinforcing the emotional obstacles to the perception of truth. Now truth is the single purpose of science; and men of science must not allow the common people to believe that things are true merely because they happen to be ardently desired. Yet this, up to date, is the sole basis of spiritualism, which has recently acquired such immense vogue.

Of the many works lately published on this subject, only one, *Spiritualism and Sir Oliver Lodge*, by Charles A. Mercier (The Mental Culture Enterprise), has any claim to be considered scientific. It is indeed one of the most brilliant expositions of the subject that has yet been published. Dr. Mercier adopts a purely logical attitude in his criticism, which is mainly directed against Sir Oliver Lodge's *The Survival of Man*. Of special importance, coming from Dr. Mercier, is his experience that the pursuit of the occult, and especially of telepathy and so on, "leads to a morbid frame of mind, and tends to render those who are at all predisposed to insanity an easy prey to the disease." In this experience he is not alone, for he quotes in support the view of Dr. G. M. Robertson, Superintendent of the Royal Asylum of Morningside, Edinburgh. Spiritualism appears to have special fascination for the weaker and more infirm members of the community, and this aspect of it is therefore important. It is needless to refer to Dr. Mercier's arguments in general, beyond remarking that they are unanswerable, and that probably no attempt will even be made to answer them.

The other works which we have to mention fall into an altogether different category. *Immortality*, an Essay in Discovery co-ordinating Scientific, Psychical, and Biblical Research, by Burnett H. Streeter, A. Clutton-Brock, C. W. Emmet, J. A. Hadfield, and the author of *Pro Christo et Ecclesia* (Macmillan), is chiefly of interest on account of the authors' names. They have adopted what they call the "group method" of attacking spiritualistic problems; and by "living contact of mind with mind," they have achieved a belief in personal immortality, which apparently is stronger than when they considered the matter separately. It is, of course, well known that belief may be enormously fortified by suggestion, and the individual sceptic who has survived the bolus of Sir Oliver Lodge will scarcely succumb to the system of polypharmacy here practised upon him. The book bears valuable testimony to three propositions: (1) That the writers all believe in Personal Immortality; (2) that they are of opinion that the veil which hangs between this world and the next is not impenetrable; (3) that these doctrines are by a fortunate coincidence wholly in accordance with their natural sentiments. Although these propositions are here well established, and although the book is written with much grace and classical knowledge, the reader who was previously unconvinced that the authors' views were correct will in all probability still remain unconvinced.

A further work, *Telepathy, Genuine and Fraudulent*, by W. W. Baggally (Methuen), devotes Part I (Genuine Telepathy) to cases in which the author has not yet discovered the *modus operandi*; and Part II (Fraudulent Telepathy) to those in which he has discovered the *modus operandi*. He feels "bound to state that, in spite of initial improbability," his experiences have convinced him that the telepathic faculty does exist. We, on the other hand, also feel bound to state that they have not convinced *us*.

Finally, there has been published a work called *Creative Psychics*, by Fred Henkel (Golden Press, Los Angeles, California), which is addressed to "all who foster . . . sex unfoldment as fruitions of the procreative urge; who see in Creative Generation, Regeneration, and Art Generation; who value the divine importance of developing the inherent creative and procreative powers." Now we cannot honestly confess that we do foster sex-unfoldment as fruitions of the procreative

urge. We might foster it if we knew what it meant; but as at present advised we disclaim it altogether. For a similar reason we are unable to see in Creative Generation, Regeneration, and Art Generation; and, as to the development of the procreative powers, we have never regarded metaphysics in the light of an aphrodisiac. This book, however, deals with metapsychics, not metaphysics; it is a branch of philosophy upon which we need not touch further in SCIENCE PROGRESS.

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

THE titles in these references are abbreviated in partial conformity with the Royal Society's *Catalogue of Scientific Papers, Subject Index*, vol. I: *Pure Mathematics* (Cambridge, 1908). Where the abbreviations used in this *Catalogue* are not formed consistently with one another, or where the periodicals mentioned here are not listed in the *Catalogue*, that usage is still further departed from—but always in a perfectly obvious way.

The deaths have been announced of the following mathematicians: Cyparissos Stéphanos of Athens, Léon Autonne of Lyons (January 12, 1916) at the age of fifty-seven; G. Frobenius of Berlin (August 3, 1917) at the age of sixty-seven; F. R. Helmert of Berlin (June 15, 1917), at the age of seventy-three; and Franz London of Bonn (February 17, 1917) at the age of fifty-four. Obituary notices of S. B. Maclaren, Sir W. D. Niven, J. G. Darboux, W. H. Besant, and E. K. Wakeford are given in the *P. L. Math. S.* 1918, **16**, xxxiii-lvii.

Education.—A. J. Kempner (*Amer. Math. Monthly*, 1918, **25**, 201–10) has an elementary note, which will be of interest to teachers, on the greatest and least values of analytic functions, and on the smallest integer $m!$ divisible by a given integer n. A very interesting feature of this *Monthly* is the accounts of the really great work that is being done in most American universities in the formation of undergraduate mathematical clubs, in which discussions take place and lectures are given—often by the students themselves—to show that mathematics is a living thing with a history and even human interests. Particularly valuable are the well-documented suggestions of suitable topics. Naturally D'Arcy Thompson's recent book

On Growth and Form (Cambridge, 1917) is suggestive in this connection (*cf.* the learned review in *Bull. Amer. Math. Soc.* 1918, **24**, 403–7), and on allied subjects we have excellent notes in the last three of the following list of suggestions that have been made: Rhind papyrus, geometrography, arithmetical prodigies, Ptolemy's theorem, paper folding, women as mathematicians and astronomers, binary arithmetic, the logarithmic spiral, "golden section," and a Fibonacci series (*Amer. Math. Monthly*, 1918, **25**, 6–8, 91–6, 136–42, 189–93, 232–8). Here also may be mentioned the useful account by G. W. Evans (*ibid.* 447–51) of Cavalieri's theorem on the mensuration of solids.

Articles of interest in the teaching of various parts of geometry are those by D. M. Y. Sommerville (*Math. Gaz.* 1917, **9**, 153–5), W. H. Roever (*Amer. Math. Monthly*, 1918, **25**, 145–59), and E. B. Stouffer (*ibid.* 159–67).

History.—G. Milhaud (*Rev. de Métaphys.* 1918, **25**, 163–75) inquires what was the discovery to which referred the date of November 11, 1620, written on the margin of Descartes' manuscript called "Olympica." It is, of course, known that this date has been taken to be that of the discovery of the method of co-ordinate geometry, but Milhaud gives good reasons, chiefly based on the "Journal" of Beeckmann printed in the tenth volume of Adam and Tannery's *Œuvres* of Descartes, that the marginal note refers to the discovery of the theory of telescopes. This paper is yet another result of Milhaud's important researches on the early scientific work of Descartes (cf. SCIENCE PROGRESS, 1918, **13**, 1–2).

F. Cajori (*Amer. Math. Monthly*, 1918, **25**, 179–201) gives a useful set of references to early uses of the process of "mathematical induction" (cf. W. H. Bussey, SCIENCE PROGRESS, 1918, **12**, 362), and then inquires how it came to be called by that rather ambiguous name. Wallis (1656) used the name for a process used by him, which was very like the usual process of induction from isolated facts; but the first use of the term for the process of deduction it now denotes seems to have been made by De Morgan in 1838.

There is an excellent and thorough review by A. Dresden (*Bull. Amer. Math. Soc.* 1918, **24**, 454–6) of J. M. Child's shortened and striking edition of *The Geometrical Lectures of Isaac Barrow* (Chicago and London), 1916. "It seems not at all

unlikely therefore," says Dresden, "that we shall have to place Barrow at last on a par with, if not above, Newton and Leibniz among the inventors of the calculus."

G. H. Hardy (*P.C.P.S.* 1918, **19**, 148–56) gives a very penetrating analysis of the paper of 1847 in which Stokes advanced his discovery of uniform convergence. The thoroughness of this examination makes it very important.

Logic, Principles, and Aggregates.—In Hartog's work of 1915 (SCIENCE PROGRESS, 1917, **11**, 453, and **12**, 7), it was proved, though not in all respects satisfactorily, that, for any given aggregate M, there is always one and only one ordinal number (α) such that: (1) For every number γ less than α there is a part of M which can be arranged in type γ; (2) No part of M can be put in type α. Philip E. B. Jourdain (*C.R.* 1918, **166**, 520–3, 984–6; *Nt.* 1918, **101**, 84, 304; *Mind*, 1918, **27**, 386–8) defines completely a method of removing the members of those parts of M which can be well ordered (not necessarily in the order, if there is one, in which M is given) and forming with them well-ordered parts of M which actually exhaust M, though none of the parts of M first mentioned need be presupposed to exhaust M. Thus any aggregate can be well ordered, Zermelo's "axiom" can be proved quite generally, and Hartog's work is completed. As this result gives the solution of a problem in pure mathematics which has been much discussed, especially during the past fourteen or fifteen years, and has many far-reaching consequences, a detailed account of it is given elsewhere in the present number of SCIENCE PROGRESS.

C. D. Broad (*Mind*, 1918, **27**, 284–303) describes a notation for the logic of relations which presents certain advantages—especially in the case of relations of a high degree of polyadicity—over that used by Whitehead and Russell in their *Principia Mathematica.*

An English translation of Hermann Minkowski's famous lecture on space and time, which was first published in 1909, is given in the *Monist* for April, 1918 (**28**, 288–302).

W. M. Thorburn (*Mind*, 1918, **27**, 345–53) continues his elaborate researches to prove that the principle of methodology known as "Occam's Razor," which has come to be of such great importance in logic and the principles of mathematics, was not stated by Ockham in the form with which he is usually

credited (cf. SCIENCE PROGRESS, 1917, **12**, 6, for reference to an allied article by Burns).

Arithmetic and Algebra.—L. J. Rogers (*Proc. Lond. Math. Soc.* 1917, **16**, 315–36) gives two theorems of combinatory analysis and some allied identities. Major P. A. MacMahon (*ibid.* 1918, **16**, 352–4) makes a small contribution to combinatory analysis, and (*ibid.* **17**, 25–41) shows that there is a one-to-one correspondence between combinations which are derived from m, identical sets of n, different letters and magic squares of order n, which are such that the numbers, placed in the cells, have for each row and column a sum m. He then generalises this theory. The problem involved is the enumeration of particular sets of partitions of certain multipartite numbers.

G. A. Miller (*Annals of Math.* 1917, **19**, 44–8) explains a different method of solving E. H. Moore's (1896) problem of possible arrangements of the players at card tournaments, and emphasises the connection of this problem with the theory of substitution groups.

E. T. Bell (*ibid.* 1918, **19**, 210–16) finds many theorems which express sums $\Sigma f(n)$ as functions of $[x]$, which denotes the greatest positive integer contained in x, where x assumes successively each of a given class of values, not necessarily integral, and n runs through all members of a given set of integers.

S. Wigert (*Acta Math.* 1917, **41**, 197–218) shows that, for values of z in the neighbourhood of the origin, the function defined by Lambert's series, which is an analytic function whose domain of existence is limited by the imaginary axis, allows of a certain functional asymptotic equation; and then applies this result to obtain a result on an arithmetical function studied by Voronoï and Landau.

W. H. Metzler (*Proc. Roy. Soc. Edinburgh*, 1918, **38**, 57–60) discusses a determinantal equation whose roots are the products of the roots of given equations.

Connected with O. E. Glenn's work of 1917 (cf. SCIENCE PROGRESS, 1918, **12**, 543) are his researches (*Annals of Math.* 1918, **19**, 201–6) on the covariant expansion of a modular form.

Analysis.—H. F. Price (*Amer. Journ. Math.* 1918, **40**, 108–12) considers the "fundamental regions" of Klein for certain finite groups in two complex variables,

É. Picard (*Compt. Rend.* 1916, **163**, 284–9) has a note on certain sub-groups of hyperfuchsian groups corresponding to ternary quadratic forms with conjugate undetermined quantities. Connected with this is his note (*ibid.* 317–9) on functions of two complex variables which remain invariable by the substitutions of a discontinuous group. Allied questions in certain groups of substitutions are dealt with by G. Julia (*ibid.* 599–600, 691–4; 1917, **164**, 32–5).

W. H. Young and (Mrs.) Grace Chisholm Young (*Proc. Lond. Math. Soc.* 1918, **16**, xii–xiv, 337–51), developing various suggestions in their former work on the theory of functions of real variables, give an account of some new theorems in that part of the theory of sets of points in space of any number of dimensions which corresponds to the classification of the limiting points of a set on the straight line into those which are limits on one side only, or on both sides (descriptive property), as well as into those which are, and those which are not, the end-points of intervals containing only a sub-set of the given set of content zero (metrical property). These theorems are of importance when we try to pass from a single variable to two or more variables, and lead to the confirmation of a surmise of Young (1909) that the set of the first category of exceptional singularities of a function of any number of variables has content zero. The same authors (*ibid.* xiv–xv; **71**, 1–16) apply the new theorems to the investigation of the symmetry which must in general hold below the limits of a function at a point, and the discussion of the distribution of the points at which this symmetry is more or less imperfect. Analogy with physical relations suggested the application of the word "crystalline" to such symmetrical relations.

H. B. Fine (*Annals of Math.* 1918, **19**, 172–3) gives a simpler and less general substitute for "Duhamel's theorem" than that of G. A. Bliss (1914), and which, like that of Bliss and unlike the particular case treated by Huntington, shows directly the limit of the sum in question. The theorem is that, if $F(x)$ is the product of continuous functions, the sum formed as if for a definite integral for this product, where the variable in each of the continuous functions may have a different value in an interval of subdivision, is the integral of $F(x)$:

$$\lim \Sigma f_1(\xi'_i) f_2(\xi''_i) \ldots h_i = \lim \Sigma F(\xi_i) h_i.$$

R. Jentzsch (*Acta Math.* 1917, **41**, 219–51) proves the theorem, which gives a more general case of some results of Hurwitz, Lindwart and Pólya, and Lukács, that every point of the circle of convergence of a power-series is a point of condensation of the zero of the "segments" (sums of the first n terms of the series, where n is any finite number). This new theorem is also related to the general researches of Montel, Severini, Carathéodory, and Landau on sequences of one-valued analytic functions. Jentzsch (*ibid.* 253–70) continues these researches. The memoir first named also appeared at Berlin in 1914 as a dissertation.

J. Hodgkinson (*Proc. Lond. Math. Soc.* 1918, **17**, 17–24) applies the method of conformal representation given by Burnside in 1893 to the discussion of certain results obtained by Gauss and Kummer with respect to the hypergeometric series.

P. Humbert (*Proc. Roy. Soc. Edinburgh,* 1918, **38**, 61–9) finds a reduction formula for the functions of the second kind connected with the polynomials of applied mathematics.

A. Ostrowski (*Acta Math.* 1917, **41**, 271–84) investigates solutions of the functional equation $\phi(x) . \phi(y) = \phi(xy)$, where the function only takes real values, the arguments take the values of any assigned corpus, and $\phi(x+y) \leqq \phi(x) + \phi(y)$.

F. W. Reed (*Amer. Journ. Math.* 1918, **40**, 97–107) applies the method of infinitesimal transformations developed by Lie to those types of integral invariants defined and discussed by Poincaré (1890, 1895), Lie (1897), and others.

R. D. Carmichael (*Annals of Math.* 1918, **19**, 159–71), starting from Birkhoff's (1908) work on adjoint systems of boundary conditions associated with adjoint linear differential equations of order n, obtain certain extensions of one of C. Sturm's (1836) theorems of comparison to equations of order higher than the second.

D. Buchanan (*Proc. Lond. Math. Soc.* 1918, **17**, 54–74), in connection with his work of 1915, determines solutions of the differential equations of motion which give orbits asymptotic to an "isosceles-triangle solution of the problem of three bodies."

Geometry.—J. Rey Pastor (*Scientia,* 1918, **23**, 413–22) gives a slight account of the systematisation of geometry by the theory of groups, which was brought about chiefly by Klein's "Erlangen Programme" of 1872.

L. P. Eisenhart (*Bull. Amer. Math. Soc.* 1918, **24**, 227–37) gives an excellent account of Darboux's contribution to geometry, and a long and thorough review (*ibid.* 394–403) of Darboux's last work, the *Principes de Géométrie analytique* (Paris, 1917).

H. S. White (*Bull. Amer. Math. Soc.* 1918, **24**, 238–43) gives an account of the first two volumes of Cremona's *Opere matematiche.*

J. R. Kline (*Annals of Math.* 1918, **19**, 185–200) gives a non-intuitional definition of the notion of "sameness of sense" on closed curves in non-metrical plane *analysis situs.* All his theorems are proved on the basis of R. L. Moore's (1916) system of axioms (cf. SCIENCE PROGRESS, 1916, **11**, 270; 1917, **12**, 194; 1918, **12**, 548).

C. L. E. Moore (*Annals of Math.* 1918, **19**, 176–84) studies motions in hyperspace by a method making use of Lie's infinitesimal transformations.

J. Eiesland (*Amer. Journ. Math.* 1918, **40**, 1–46) continues his work on flat-sphere geometry.

J. V. De Porte (*ibid.* 47–68) writes on irrational involutions on algebraic curves.

J. R. Musselman (*ibid.* 69–86) writes on the set of eight self-associated points in space.

C. L. E. Moore (*Amer. Math. Monthly*, 1917, **24**, 456–62) gives a substitute for Dapin's indicatrix for the study of surfaces, which has the advantage that it can be easily generalised.

L. P. Eisenhart (*Annals of Math.* 1918, **19**, 217–30) discusses some types of surfaces which can be generated in more than one way by the motion of an invariable curve, when the types are not so very simple or well known as, for instance, ruled surfaces and surfaces of revolution.

J. K. Whittemore (*Amer. Journ. Math.* 1916, **40**, 87–96) discusses associate minimal surfaces.

APPLIED MATHEMATICS.

By S. BRODETSKY, M.A., Ph.D., A.F.Ac.S., University, Bristol.

It is necessary first to define the sense in which the term Applied Mathematics is to be used here. Whilst all would agree that certain subjects must be classed under Physics and others under Astronomy, there is in reality no sharp line of demarcation between Applied Mathematics and Physics on the one

hand, and Applied Mathematics and Astronomy on the other. There is further the additional difficulty that many problems in Pure Mathematics are obviously and directly suggested by some natural phenomenon when formulated in symbolical notation. All investigations on the solution of Laplace's equation and its modifications, as well as the other equations of mathematical physics, come under this category, and it is no easy task to decide whether the pure or the applied mathematician may claim them as coming within his sphere.

In this survey of some of the recent work in applied mathematics the writer's principle has been to limit himself to the consideration of such papers as can be definitely excluded from the domains of pure mathematics, astronomy, and mathematical physics. We shall then report on the progress in the development of Statics and Dynamics of Solids and Fluids, including the Theory of Gravitational Attraction, Motion in a Resisting Medium, and the Mechanics of Astronomy considered from the mathematical point of view. We omit Relativity as such, since it is of more immediate interest to the physicist and the astronomer.

Research in any branch of science can be divided into two categories. The first consists of the further development of what may be called the classical problems of the subject, the working out of the further consequences of the well-established principles and methods, and the elaboration of the detailed solutions. The second category of research concerns itself with the discovery of new principles, the development of new methods, and the attacking of new problems suggested by theoretical, practical, and industrial applications. In applied mathematics this dual nature of research is at once made clear by the recent growth of the subject. Whereas on the one hand we have much that is a continuation or an extension of the well-established ideas and processes, we have on the other hand ample evidence of progress towards fresh fields of investigation.

Treating first of the research of a pioneer nature we are confronted by two main themes. The principle of Relativity has necessitated a reconsideration of the fundamental principles of mechanics, and this has brought in its train a certain amount of scepticism as to the accepted facts of universal gravitation. It is assumed in ordinary dynamics that the gravitational effect

of a body is a function only of its mass and position. Comparatively recent experiment has suggested that temperature affects slightly the gravitational force produced. V. Crémieu (*Comptes Rendus*, **165**, 586–9, 670–72, 1917) uses the pendulum method to investigate experimentally whether the gravitation between two bodies is affected by (1) motion of one of the bodies or (2) the presence of lines of gravitational force due to a rapidly moving third body. In each case the result obtained was negative, the order of accuracy being 1 per cent.

C. E. Weatherburn (*Proc. Camb. Phil. Soc.* **19**, 72–85, 1917) considers Lamla's and Laue's equations of motion of hydrodynamics from the standpoint of relativity, obtaining the analogues of some well-known theorems in ordinary hydrodynamics.

The other direction in which much work is now being done is in the dynamics of and through media. This is an immediate effect of the development of the art of flying. In order to achieve success in aeronautics two problems referring to our subject need investigation—motion through a resisting medium and meteorology. The war has reacted profoundly upon this phase of research. Whereas only a few years ago orthodox mathematicians stood more or less aloof from aeronautical mathematics, the case is quite different now. The successful vindication of the researches of Lanchester and Bryan as leading to criteria of stability and controllability of an aeroplane based on exact mathematical formulation, and the imperative necessity to apply the best brains of the country to the solution of the urgent problems of aircraft attack and anti-aircraft defence, have swept away the remains of old prejudice. It is obvious, of course, that much of the work that has been and is being done during the war must remain unpublished, and there can be no doubt that the cessation of hostilities will be followed by the release of an amount of accumulated knowledge and results that will colour the teaching and development of dynamics for many years. Nevertheless, a considerable amount of important work, that has no immediate bearing upon practical military problems, is being published. Among the recent researches that have come to the notice of the writer are the following:

P. Appell (*Comptes Rendus*, **165**, 694–5, 1917; **166**, 22–3, 1918) discusses the results of experiments made by Z. Carrière

(*Jour. de Phys.* **5**, 175–86, 1916) on the two-dimensional motion of a light sphere moving in air under gravity, having a rotation about a horizontal axis perpendicular to the plane of motion. Carrière found that after an initial part dependent on the initial conditions, the motion becomes practically uniform in a straight line inclined to the downward vertical in the same sense as the rotation, the inclination increasing with the ratio of the rotation to the translational motion. Appell suggests that the motion can be explained if the air resistance is taken to act in a line through the centre of the sphere, making an angle with the backward direction of motion dependent on the angular velocity, and states that he intends to publish the mathematical analysis for the assumption that the air-resistance varies as the velocity. In a paper read at the Royal Society but not yet published (*Nature*, **98**, 483, 1917), the writer of these notes shows that the centre of a plane lamina moving in two dimensions in air, the law of resistance being the square of the velocity, moves in a manner similar to that found by Carrière for a light sphere. One may even suggest that probably any rigid body moving in air under gravity will after a time approximate to a type of "terminal" motion along a wavy line inclined to the vertical in a sense and to an extent dependent on the relation between the rotational and the translational velocities. It is well known that a particle tends towards a terminal motion in a vertical straight line.

H. Larose (*Comptes Rendus*, **165**, 545–8, 1917) investigates the steady motion of a uniform flexible and inextensible string in air under gravity, finding the equations of the various forms of a string moving with a constant velocity along itself and an addition velocity in a horizontal direction.

Another problem in resisted motion is worked out by J. Prescott in a paper entitled "On the Motion of a Spinning Projectile" (*Phil. Mag.* (6), **34**, 332–80, 1917). The author takes the air resistance to be proportional to the square of the velocity, the constant of proportionality being one or another according as the velocity is less than or greater than 1,060 ft. per sec., thus introducing an important modification into the method of Bashforth, who made the resistance vary as the cube of the velocity, the constant of proportionality varying as the velocity underwent any considerable change. Assuming first that the resistance is always exactly opposite to the direc-

tion of flight, and then introducing the effect of the shape of the projectile by supposing the resistance to act in the plane of the axis of symmetry and of the direction of motion, at an angle with the former some constant times the inclination of the latter, Prescott calculates the trajectory as well as the drift and the angular deflection of the axis of the projectile.

W. A. Dalby (*Proc. Roy. Soc.* **93**, A, 333–47, 1917) extends his graphical method of drawing trajectories, published in 1915, in which the density of the air was assumed constant. This restriction is now discarded so that the graphical process can be extended to high-angle fire. Numerical results are given.

P. Frank (*Phys. Zeitschr.* **19**, 2–4, 1918) shows that the problem of steering an airship in a variable wind so as to go from one point to another in minimum time leads to an equation of the same type as occurs in the propagation of light in a moving medium.

J. G. Leathem (*Phil. Mag.* (6) **35**, 119–30, 1918) continues his work on curve factors in the conformal representation of hydrodynamical problems in two dimensions (see also *Phil. Trans.* **215**, A, 439–87, 1915, and *Proc. Roy. Irish Acad.* **33**, A, 35–57, 1916). The fact that the lifting power of an aeroplane is greatly improved if the wing is not flat but "cambered," so as to be slightly concave on the lower surface and considerably convex on the upper surface, has long been made use of in practical aeronautics. On the other hand the investigation of the discontinuous stream-line motion past such a wing has long defied analysis. Thus Greenhill in his report on the subject to the Advisory Committee on Aeronautics (Report 19), published as recently as 1910, limits himself explicitly to plane barriers, or combinations of plane barriers. H. Levy (*Proc. Roy. Soc.* **92**, A, 285–304, 1916) showed how to extend the classical work of Kirchhoff and Rayleigh to curved boundaries. Leathem's method consists in discovering conformal transformations applicable to curves by extension of and analogy with known cases.

The practical problem of finding the resistance to motion through a medium is, however, best solved experimentally, and the work of Eiffel, Dines, Bairstow and others has been mainly instrumental in supplying the information upon which the successful conquest of the air has been based. The hydrodynamical calculations are bound to lose in practical value because

of the abstract nature of the problem when simplified into something that can be attacked by present mathematical analysis, since viscosity has to be omitted in general. We must wait till we learn more concerning the nature of the viscous forces in simple types of motion in a gas or liquid. We note that Guillet (*Comptes Rendus*, **166**, 33–5, 1918) has investigated experimentally the viscosity couple in the case of a lamina endowed with oscillatory rotation in its own plane.

Lord Rayleigh (*Phil. Mag.* (6) **35**, 1–12, 157–63, 1918) considers the two-dimensional problem of lubrication, theoretically and experimentally.

The question of the motion of solid particles in a viscous liquid arises in connection with the separation of materials for grinding and polishing glass surfaces in industrial optics. A lecture entitled "Polish" delivered by Lord Rayleigh before the Royal Institution in 1901 has recently been reprinted in the *Transactions of the Optical Society* (**19**, 38–47, 1917). The same volume contains a contribution on the subject by J. W. French—"The Grading of Carborundum for Optical Purposes" (*ibid.* 1–37).

Returning to aeronautical research we have the paper by M. A. S. Riach (*Aeron. Jour.* **21**, 123–41, 1917) on "The Screw Propeller in Air," where the author discusses his blade element theory of the air-screw, combining it with Froude's theory for screws in ships and Lanchester's and Drzewiecki's investigations on aeroplane propellers. J. A. Bothezat, in a paper published at Petrograd (August 1917), examines the various types of screw propellers, ventilators, helicopters, and turbines, summing them up in one composite theory. A general account of the possible lines of progress in aeroplane performance is given by F. M. Green (*Aeron. Jour.* **22**, 3–16, 1918), whilst the interesting paper by R. Mullineux Walmsley and C. E. Larard on "The Training of Aeronautical Engineers" (*Aeron. Jour.* **21**, 403–19, discussion 430–38, 1917) contains much that teachers of applied mathematics may read with profit.

Other papers on applied mathematics published in the last few months are as follows:

Statics.—Mesnager (*Comptes Rendus*, **165**, 551–3, 997–1000, 1103–5, 1917) continues his work on the bending of beams and plates.

J. W. Nicholson (*Proc. Roy. Soc.* **93**, A, 506–19, 1917) calcu-

lates the nodes in vibrating rods formed by the rotation of curves $y=Ax^n$, $0<n<1$, about the axis of x, in connection with a theory on the siliceous deposits on certain sponge spicules.

Ganesh Prasad (*Phil. Mag.* (6) **34**, 138–42, 1917) considers certain laws of density in a symmetrical sphere, that do not conform to Poisson's equation and to Petrini's generalisation.

Guillet (*Comptes Rendus*, **165**, 1050–52, 1917) makes a plea for the measurement of g by the Newtonian method of noting the fall of a body in a short time, as against Galileo's pendulum method. The author urges that the modern refined methods of measuring distances and very short intervals of time make Newton's method more accurate than Galileo's in view of the considerable number of sources of error in a pendulum.

S. Brodetsky (*Quart. Jour. of Math.* **48**, 58–76, 1917) calculates the attraction of equiangular spirals of various laws of density, and considers a dynamical consequence.

Dynamics.—Andrew Gray (*Phil. Mag.* (6) **35**, 181–9, 1918) gives the history of the hodogaph and discusses several points of interest in the problem of two bodies.

C. V. Raman and Ashutosh Dey (*Phil. Mag.* (6) **34**, 129–37, 1917) give an experimental and theoretical investigation of the vibrations of a steel wire under a periodic magnetic force. It is shown that the wire can have vibrations with the periodic force applied at a node, the ordinary forced vibrations being unstable.

E. H. Barton and H. Mary Browning (*Phil. Mag.* (6) **34**, 246–70, 1917) describe experiments conducted on mechanical models illustrative of the types of harmonic oscillations in coupled electric circuits. The method is an extension of the Blackburn pendulum. In one type of experiments two equal pendulums are joined by a stiff connector; in a second type, one pendulum is suspended from a point on the lath of the other. Unequal pendulums are considered in a second contribution ((6) **35**, 62–79, 1918). H. C. Plummer ((6) **34**, 510–17, 1917) objects that the analogy is not sufficiently close to be of use to non-mathematical students of electricity, and the authors reply ((6), **35**, 203–5, 1918). Sir G. Greenhill adds a note on Perigal's experiments ((6), **35**, 140, 1918).

Thybaut (*Comptes Rendus*, **165**, 55–6, 1917) extends into three dimensions Puiseux' work on tautochronous curves under a central force.

R. Moritz in a paper on Hill's Cusped Orbit (*M.N., R.A.S.*, **78**, 48–53, 1917) and E. Lindsay Ince on the General Solutions of Hill's Equation (*ibid.* 141–7) continue the detailed discussion of one of the classical problems of celestial mechanics.

L. Becker (*ibid.* **77**, 655–62, 1917) investigates the law of density of a spheroid that will account for the spiral arms of nebulæ.

Vessiot (*Comptes Rendus*, **165**, 99–102, 1917) shows how to pass from one set of canonical co-ordinates to another in celestial mechanics, without successive transformation of variables.

Mechanics of Fluids.—Lord Rayleigh (*Phil. Mag.* (6) **34**, 94–8, 1917) simplifies and extends Besant's work on the pressure developed in a liquid during the collapse of a spherical cavity, to explain the sound emitted by water on the boil owing to the collapse of air bubbles as they rise through cooler water.

S. Banerji (*Phil. Mag.* (6) **35**, 97–111, 1918) writes on "Aerial Waves Generated by Impact," investigating the wave motion set up in air by the impact of solid bodies, especially the effect of the sizes and densities of the bodies on propagation of the sound in various directions.

T. H. Havelock (*Proc. Roy. Soc.* **93**, A, 520–32, 1917) in a paper entitled "Some Cases of Wave Motion due to a Submerged Obstacle" extends Lamb's two-dimensional work to the case where the obstacle is a sphere.

K. Terazawa (*Sc. Rep. of Tôhoku Imp. Univ.* **6**, 169–81, 1917) examines the "Oscillations of the Deep Sea Surface caused by a Local Disturbance," (1) with the initial displacement given, (2) with the initial impulse given. The solution depends on the function $\gamma(x) = e^{-x^2}\int_0^x e^{t^2}dt$, which is fully investigated by the author and tabulated for a wide range of values of x.

H. Jeffreys (*Phil. Mag.* (6) **34**, 112–28, 449–58, 1917) in two papers "On Periodic Convection Currents in the Atmosphere" considers the effect of eddy viscosity in the equations of motion. He finds that the terms dependent on the vertical velocity may be neglected in some cases, but that in others, as *e.g.* for a small circular island, they must be retained.

ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

Determination of Telescopic Flexure.—An accurate knowledge of the flexure of a telescope used for fundamental meridian

observations is of the greatest importance. Yet it is surprising that although methods have been devised for determining other instrumental errors which would vitiate the accuracy of the results, little progress has been made towards reliable and accurate determination of telescope flexure. The method most commonly employed, that of determining the horizontal flexure by means of collimators, is not very reliable, and involves the assumption, in order to obtain the flexure at any inclination of the telescope, that the flexure varies as the sine of the zenith distance. The use of direct and reflected observations of stars introduces other complications, whose interpretation is not even yet fully understood. Methods have been proposed involving the use of special apparatus attached to the telescope, but they were not convenient inasmuch as the apparatus had to be removed when star observations were made.

A method has been recently devised by Ilmari Bonsdorff of the Poulkowa Observatory, Petrograd, which appears to be free from all objections and has the merit of extreme simplicity. The flexure is determined completely at any zenith distance by an apparatus attached only to the exterior of the cube and having no other contact with the instrument, and which does not need to be removed when star observations are made.

The essential parts of the apparatus consist of two steel tubes solidly fastened to the cube of the telescope, the one above and the other beneath. The tubes are parallel to the telescope axis. To the object end of each tube a horizontal steel cylinder is attached which fits accurately into brass bearings mounted at the ends of a light but rigid aluminium plate, which when in position is just in front of the telescope objective and perpendicular to the telescope axis. The bearings are held against the cylinders by four spiral springs. A mirror, whose position is capable of fine adjustment, is mounted in a brass mount fixed to the aluminium plate. Weights are attached to the eye-ends of the tubes to counterbalance the aluminium plate and mirror.

It is obvious that in whatever position the telescope may be placed, the plane of the mirror will always be parallel to a plane fixed with reference to the cube of the telescope, for the distance between the ends of the two tubes cannot vary and

both tubes being perfectly alike must have the same flexure.[1] Control experiments were made to prove that the apparatus actually behaved in accordance with this conclusion, which was found to be closely justified.

The flexure of the telescope at any zenith distance is determined by bringing the horizontal wire of the ocular micrometer upon its image reflected from the mirror back through the telescope. Great precision in the results can be obtained. The method was seen in application by the writer at the Poulkowa Observatory in 1914. The mean error for one observation was $\pm 0''\cdot 05$ and the flexure in any position was found to be very closely in accordance with the formula $a \sin z$, z being the zenith distance and a a constant which had the value $0''\cdot 405$ for the telescope in question.

When star observations are required to be made the aluminium plate is removed by unhooking the ends of the four spiral springs, counterpoising weights being placed on the ends of the steel tubes. As this operation only takes about one minute, determinations of flexure may be made, if so desired, at any time during an evening's observations.

A detailed account of the apparatus and of results obtained is given in a jubilee volume to Prof. Donner, " Festskrift tillegnad Anders Donner på haus Sextioårsdag den 5 Nov. 1914 af forne Elever."

The Magnetic Field of the Sun.—The deduction from the electromagnetic theory of light made theoretically by H. A. Lorentz and verified experimentally by P. Zeeman, that by passing a beam of light through a magnetic field each spectral component is broken up into several components which are polarised, the number and polarisation of the components depending upon the direction in which the light passes relatively to the direction of the lines of magnetic force, was applied several years ago to the investigation of the general magnetic field of the sun at the Mount Wilson Observatory. The results obtained were given in the *Astrophysical Journal*, **38**, 27, 1913. Using a polarising apparatus with the 75-foot spectrograph of the large 150-ft. tower telescope, it was found that four lines in the third order specimen of a Michelson grating gave Zeeman displacements which agreed in sign and closely in magni-

[1] We may compare the deformation of a parallelogram of jointed rigid rods. If one rod is fixed, the opposite rod must move parallel to itself.

tude with the theoretical values that would be deduced on the supposition that the sun was a uniformly magnetised sphere, the magnetic poles of the sun being at or near the poles of rotation. The approximate vertical intensity of the sun's general field at the poles was found to be about 50 gausses. Evidence was also obtained that the general magnetic field decreased rapidly in intensity in the upper levels of the solar atmosphere. It was a remarkable piece of work which enabled such conclusions to be put forward with confidence, based upon what at first sight appeared slender evidence, the displacements obtained being very small and not exceeding 0·001 Angstrom unit in amount. Further results since obtained have, however, confirmed the earlier conclusions: they are given in a paper by G. E. Hale, F. H. Seares, A. van Maanen and F. Ellerman in the *Astroph. Journ.* **47**, 206, 1918. In this paper, measures of displacements are given for 26 lines in the solar spectrum belonging to iron, chromium, nickel, vanadium, and titanium. Eighteen ether lines, which had previously been found susceptible to the influence of the magnetic field in sun-spots, showed no measurable shift. The explanation of this is not clear. Using the laboratory data for the separation of these lines by a magnetic field of known strength, the field-strengths producing the observed separations were calculated and it was found that the field-strength decreased with increasing line-intensity. The strongest lines are those which originate in the upper levels of the solar atmosphere, and therefore this result is interpreted as indicating a field-strength diminishing rapidly with increasing elevation. Using Mitchell's results for the depths at which the chromospheric lines originate, it is concluded that the part of the field accessible to observation lies within the bounding surfaces of a thin shell in the solar atmosphere about 150 km. thick and that definite values of the calculated field-strength always correspond to definite levels in the solar atmosphere.

The general nature of the sun's magnetic field may now be said to be known with some certainty, but the underlying causes of it remain obscure. The hypothesis that it is due to local whirls is examined, but the evidence on the whole seems to be against it.

The Atmospheric Scattering of Light.—The atmospheric transmission coefficients obtained at Mount Wilson from 1910

to 1916 have been analysed by F. E. Fowle (*Smithsonian Miscellaneous Collections*, **69**, No. 3, 1918) with some interesting results. In order to be free from the effects of selective absorption the discussion was limited to the spectral region between 0·35 and 0·50μ. The coefficient of absorption, a_λ, for wave-length λ, can be expressed as the product of two factors one, $a_{a\lambda}$ due to dry air and the other, $a^w_{w\lambda}$ due to an amount of water vapour above the station equivalent to w cms. of water. $a_{a\lambda}$ is practically constant, and writing $\log a_\lambda = \log a_{a\lambda} + w \log a_{w\lambda}$, and plotting a_λ against w, both $a_{a\lambda}$ and $a_{w\lambda}$ can be determined. Applying Rayleigh's law of scattering of a beam of light in a gaseous medium to $a_{a\lambda}$, which is found to vary inversely as the fourth power of the wave-length as required by that law, a value is obtained for N_o, the number of molecules of air per c.c. at 76 cm. pressure and 0°C. Fowle thus finds $N_o = (2{\cdot}72 \pm 0{\cdot}01)10^{19}$. The best value otherwise obtained is $(2{\cdot}705 \pm 0{\cdot}003)10^{19}$. Hence the term $a_{a\lambda}$ is due almost entirely to molecular scattering. This result confirms the accuracy of the Mount Wilson estimations of atmospheric losses affecting the radiation from the sun.

Search was also made for a scattering the same for all wave-lengths such as would be caused by large dust particles and which might be called a "dry haziness" effect. In 1910-11 this amounted only to 0·5 per cent. After the Mount Katmai eruption in 1912 it became 25 per cent. but decreased in 1913 to 3 per cent. and in 1914–15 to 1 per cent.

The transmission coefficients for atmospheric aqueous vapour ($a_{w\lambda}$) were also found to vary with the inverse fourth power of the wave-length. The scattering of radiation when passing through liquid water was shown to be the same as would be expected from the same number of molecules in a gaseous state. The unexpected result was obtained, however, that the same amount of water in the form of *atmospheric* water vapour gave 50 times as much absorption as required by theory. The explanation suggested for this result is that molecular aggregates are formed which increase the diameter of the scattering particles: such increase in diameter far more than compensates for the reduction in the number of separate particles as far as the amount of scattering is concerned. This molecular condition may be connected with ionisation phenomena, which suggests a connection with solar

radiation. Some evidence is brought forward by Fowle that the smaller the average solar radiation, or sun-spot number, the greater is the absorptive power of atmospheric water vapour.

The "moist haziness" associated with water vapour causes losses from the direct solar beam of about 2 per cent.

The Photography of Stellar Spectra in the Infra-Red.—Very little has been done so far in the investigation by photography of the spectra of stellar bodies in the region of long wave-lengths, and the chief need of stellar spectroscopy to-day is an addition to our knowledge in this direction. An investigation recently carried out by P. W. Merrill of the U.S. Bureau of Standards is therefore to be welcomed (*Scientific Papers of the Bureau of Standards, Washington,* No. 318).

He investigated the results obtainable from the application of dicyanin to stellar photography. Dicyanin is one of the dyes used for staining photographic plates to make them sensitive to the red and infra-red; but, to obtain the full advantages from its use, special methods of staining are required and the plates must be used very shortly after sensitising. By arrangement with Prof. Pickering, the 24-inch reflector of the Howard Observatory was used, in conjunction with an objective prism. Results of high accuracy were not sought for, the investigation only seeking to determine the possibilities of the method. Seventy-seven spectra of twenty-nine stars were obtained.

The spectra investigated were chiefly those of the earliest types (*O* and *B*) and of the latest types (*M*, *N* and *R*). It was found possible to photograph Fraunhofer's A band (wave-length $0 \cdot 760\mu$) and a considerable region of the spectrum beyond. The greatest wave-length observed was $0 \cdot 87\mu$, A strong band at 760μ, nearly coincident with the A band, was discovered in spectra of type *M*. This band is particularly strong in *Mb* and *Md* type spectra and may prove of use in the classification of late type stars. It is thought to be probably due to titanium oxide, thus extending the series found by Fowler in this type of spectrum. In spectra of class *N* bands were observed at 6920, 7080, 7230 which may be identical in origin with cyanogen bands.

The spectra of class *R* were found to differ considerably from those of class *N* as regards the spectral distribution of

energy. This suggests that the sequences *G*, *K*, *M*, and *G*, *R*, *N* may be parallel lines of evolution.

The results indicate the desirability of further investigation with longer exposures and large dispersion.

The Threshold of Vision.—A note on recent determinations of the minimum light perceptible to the human eye was given in SCIENCE PROGRESS, **12**, 552, 1918. The direct determination by Reeves was based upon observations of an artificial star 1 mm. in diameter from a distance of 3 metres. In *Astroph. Journ.* **47**, 141, 1918, he gives the results of further investigations as to the effect of the size of the stimulus and the time of exposure on the retinal threshold. The absolute threshold was determined for stimuli varying in size from a 2 mm. square to a 12 cm. square and viewed at various distances. In each case, before taking an observation, the eye was adapted to darkness. It was found that the threshold decreased considerably with increase in the size of the stimulus, but that the total energy entering the eye showed an increase, *e.g.*, for a 2 mm. square at 35 cm., the energy entering the eye at the threshold of vision was $25{\cdot}3 \times 10^{-10}$ ergs per sec., whilst for a 12 cm. square at the same distance it amounted to 564×10^{-10} ergs per sec.

Attempts were made to determine the length of time required for a point source to produce a perceptible sensation. For just perceptible intensity, the time was found to average about 2 secs. Independent experiments were made with calibrated photographic shutters placed before the test spot and the brightness necessary for the test spot to be perceived when exposed was determined. In this way, a rapid increase in sensibility up to 2 seconds was obtained.

The following are a few of the more important papers recently published :

Historical.—REPSOLD, J. A., Zur Geschichte der Astronomische Messwerkzuge. I. Heron's Dioptra, *Ast. Nach.* No. 4931. II. Alte Arabische Instrumente, *Ast. Nach.* No. 4935, 1918.

DREYER, J. L. E., On the Origin of Ptolemy's Catalogue of Stars, *M.N., R.A.S.*, **78**, 343, 1918.

FOTHERINGHAM, J. K., The Secular Acceleration of the Sun as determined from Hipparchus' equinox observations,

with a note on Ptolemy's false Equinox, *M.N., R.A.S.*, **78**, 406, 1918.

Gravitational Astronomy.—Wilkens, A., Über die Integration der Differentialgleichungen der Koordinatenstorungen der Planeten der Jupitergruppe, *Ast. Nach.* No. 4937, 1918.

Heinrich, A., Über die singulären Punkte gewissen Ungleichheiten im Asteroidischen Problem, *Ast. Nach.* No. 4930, 1918.

Sundman, K. F., Sur les conditions nécessaires et suffisantes pour la convergence du développement de la fonction perturbatrice dans le mouvement plan, *Öfversigt af Finsk. Vetensk. Soc. Förhand*, **58**, No. 24, 1916.

The Solar System.—The Sun.—Adams, W. S., Some Stellar Investigations at the Mount Wilson Observatory, *Journ. des Observateurs*, **2**, No. 6, 1918.

Véronnet, A., La Constitution physique du Soleil. Son évolution et la nôtre, *Rev. gén. des Sciences*, 29ᵉ An. 359, 1918.

Perrine, C. D., The Nature of the Sun's Corona, *Observatory*, **41**, 253, 1918.

Clayton, H. H., The Effect of Short-period Variations of Solar Radiation on the Earth's Atmosphere, *Smithsonian Miscell. Collections*, **68**, No. 3, 1918.

The Earth.—Schuster, F., Die yährliche und die monatliche Häufigkeit der Erdbeben, *Ast. Nach.* No. 4942, 1918.

Ditto. Latitude Variation.—Courvoisier, L., Zur Frage der tägliche Polhohenschwankung und des *z* Gleides, *Ast. Nach.* No. 4945, 1918.

Dyson, F. W., The Variation of Latitude, *M.N., R.A.S.*, **78**, 452, 1918.

Turner, H. H., The Movements of the Earth's Pole: Note on Sir F. W. Dyson's Analysis., *M.N., R.A.S.*, **78**, 462, 1918.

Dyson, from a new analysis of previous observations, finds a value for the mean free period of 432 days, a slight increase on Chandler's period. Turner finds discontinuities in the period, giving two periods of 421 and 443 days which fit in with his theory of alternating weather cycles.

A report of a discussion on "The Movements of the Earth's Pole" held under the auspices of the British Association Geophysical Committee is given in the *Observatory*, **41**, 246, 1918.

The Moon.—Russell, H. N., Fowler, M., Boston, M. C., Fuller, E., Photographic Determinations of the Position of the Moon, *Harvard Annals*, **80**, No. 11.

The Planets.—King, E. S., Eclipses of Jupiter's Satellites, *Harvard Annals*, **80**, No. 10.

Lau, H. E., Beobachtungen des Planeten Jupiter, 8 Reihe, *Ast. Nach.*, No. 4943, 1918.

Bolton, Scriven., The Surface Currents of Jupiter during the Apparition of 1917–18, *M.N.*, *R.A.S.*, **78**, 467, 1918.

History.—Jeffreys, H., On the Early History of the Solar System, *M.N.*, *R.A.S.*, **78**, 424, 1918.

The Generalised Theory of Relativity.—Woolard, E., Generalised Relativity and Gravitation, *Amer. Journ. Science*, **45**, 425, 1918. This is a good elementary summary of the subject.

Lense, J., Über Relativitätseinflusse in den Mondsystemen, *Ast. Nach.* No. 4934, 1918.

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Variable and Binary Stars.—Eighth Report of the Section for the Observation of Variable Stars, 1910–14. Director, C. L. Brook. *Mems.*, *B.A.A.*, vol. xxii. The volume contains the observations of 29 stars by 34 observers with various instruments from binoculars to a 12½-inch reflector.

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Stellar Distribution and Motions.—GRAHAM, T. S. H., The measurement of radial velocities of stars by means of the objective prism spectrograph, *Journ.*, *R.A.S.C.* **12**, 129, 1918.

LUNT, J., Radial Velocities of 60 Stars, *Astroph. Journ.* **47**, 201, 1918.

SEARES, F. H., The Brightness of the Stars, their Distribution, Colours and Motions, *Pub. Ast. Soc. Pac.* **30**, 99, 1918. This is a good summary of recent work.

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PHYSICS. By James Rice, M.A., University, Liverpool.

Volume xxx. of the *Proc. Phys. Soc.* contains two interesting papers by Prof. Nicholson. In Part I. (December 1917) he makes some tentative suggestions concerning the structure of the electron and of the nucleus of an atom. There are several difficulties which are at present encountered in discussing the form of electrons. One is the existence of a finite radius and therefore also a discontinuity at a prescribed surface which marks off a distinct region from the ether. This conception is somewhat at variance with the views of those who regard the electron as a structure built out of ether; but in the hands of Lorentz and others it has been of great service, and has led to conclusions concerning the variation of the mass of an electron with its speed which are in undoubted agreement with careful experiments. Such considerations involve the existence of a linear constant which is the same for every electron and is usually regarded as a "radius." Thus it appears that if e is the fundamental electronic charge (in electrostatic units), and a is the "radius" (in centimetres) the mass of the electron is a multiple of e^2/a, the multiple increasing with speed, but for small speeds (*i.e.* small compared to the speed of light) being equal to $2/3c^2$, where c cms. per sec. is the speed of light. This result rests on the assumption that all the charge of the electron is confined within a sphere of radius a and is uniformly distributed therein. (No

doubt any lack of uniformity in the distribution would alter the value of this multiple somewhat, but would not disturb its order of magnitude.) In order to make this expression for the mass agree with actually observed values, one has to give a value to a, the "radius," of the order 10^{-13} (in cms.). If we now turn to the consideration of the nucleus of an atom according to the model (such as that of Rutherford) at present found necessary to interpret such phenomena as radioactivity, atomic number, and scattering of charged particles by atoms, we find that in order to account for the mass of a nucleus (which is practically that of its atom) on the same basis as we account for the mass of the electron, we must assign a smaller "radius" to it than to the electron; yet the nuclei of the more complex atoms must, from certain considerations, be supposed to contain electrons and at the same time preserve their minute size. This is a serious difficulty unless we can suppose that electrons and positive charges can actually interpenetrate each other and occupy the same space. Furthermore, there is the difficulty about the coherence of the electron itself in view of the self-repulsive nature of its charge.

Nicholson points out that if we resort to the view which regards elementary charges as regions of strain in the ether we evade these difficulties if we can find a satisfactory basis for introducing a linear constant such as the "radius" mentioned above. But he remarks that it would seem more natural that such line constants should possess some significance throughout the whole ether, rather than merely come into existence in regions where the ether is strained into the form of matter. He suggests that the ether may be, in fact, cellular in structure, with these linear magnitudes involved in the specification of the cells and thereby in any strained structure composed of them. Such speculations apart, however, he works out this idea mathematically for a type of ether-strain which is for practical purposes concentrated at its "centre" and diminishes very rapidly outwards according to an exponential law. Thus his electron (based on that of Larmor) has no definite boundary. It is a region where there is a distribution of electric charge with a density ρ given by the equation

$$\rho = \rho_0 \epsilon^{-\lambda r}$$

ϵ being the Napierian base, r distance from the "centre" of the electron, ρ_0 being density at the centre, and λ being a constant whose dimension is a reciprocal length. Although this electron has no definite boundary (since ρ only vanishes if r is infinite) yet practically the whole of its charge is contained within a sphere whose radius is a small multiple of λ^{-1}, so that the electron might be said to possess a radius comparable with the length λ^{-1}. Expressions for electric field and energy are worked out. They degenerate to the usual expression, based on the inverse square law, for distances from the "centre" which are large compared to λ^{-1}. The field due to two such distributions of charge with their centres near one another is also dealt with, with the following striking conclusions. Whereas according to the ordinary conception of the finite electron, the repulsive force between two electrons would increase enormously on close approach, two such distributions as Nicholson considers would not behave in such manner. The repulsive force would tend to zero and not to infinity as the distance between the electron centres diminishes. For example, when the distance separating the centres is twice the linear constant λ^{-1}, the force is only ·05 of the force demanded by the usual inverse square law, and, as already stated, the limit is zero as the separation tends to zero. Such a coalescence of two electrons would of course be unstable, and the electronic charge e would have to be regarded as a constant of ethereal structure, just as λ; and no doubt Planck's constant would also be involved in a similar way, on account of the undoubted relation between this constant and e.

The tendency of the force between two such electric charges to vanish is of course true for charges of opposite sign. The large inertia of the positive charge involves of course very small values for the linear constant λ^{-1}, and therefore large values for λ—values which must be at least of the order 1,000 times as great as in the case of the negative electron. But the practical evanescence of the force (of attraction in this case) does not depend on the values of λ, and would remain. It is even possible that with a suitable law of density replacing the merely illustrative suggestion of exponential decrease outwards, there might be a reversal in the sign of the force. Hence a positive and negative electron would

not necessarily rush together and annihilate each other, but would form a doublet whose length would be comparable with that of a single electron. Oscillatory motion would be possible, and a radiation of wave-length comparable with the "radius" of the electron would be emitted.

This view that two ethereal structures can exist in this way without deformation in the presence of each other, and simultaneously occupy the whole ether, offers some difficulty, no doubt; but one no greater than that involved in the postulate that an electron composed of mutually repelling parts has a definite boundary. It has the advantage of suggesting an avenue of research in which one might resolve the difficulty of the minute but complex nucleus. For instance, a neutral doublet could be conceived as a distribution of electric density given by an expression such as

$$8\pi\rho = e(\lambda_1^3\epsilon^{-\lambda_1 r} - \lambda_2^3\epsilon^{-\lambda_2 r}).$$

The total charge is zero when integration is extended through all space. Considering λ_1 as less than λ_2, the density and electric force are confined to a region round the origin of r whose radius is of the order λ_1^{-1} almost precisely. The system behaves like a pair of charges $\pm e$ at the origin of radii λ_1^{-1}, λ_2^{-1}, which can be separated by the application in a suitable form of an amount of energy of the order $\lambda_1 e^2$. Such a type of doublet might form a component of a complex atomic nucleus; one would then preserve the extreme minuteness of the nucleus, and also the property of radioactivity or partial dissolution of the nucleus into positive and negative charges.

Prof. Nicholson's second paper is printed in Part II. (February 1918) of the Proceedings. In it he criticises some of the model atoms which have been employed by physicists to elucidate points in the behaviour of atoms and molecules. Nicholson himself in the *Phil. Mag.* (April and July 1914) has already pointed out that the Rutherford model is unstable (according to the principles of Newtonian dynamics) unless all the electrons are in one plane, rotating as a ring. True, this difficulty is somewhat evaded by the non-Newtonian mechanics worked out by Bohr (*Phil. Mag.* 1914, July, September, December). But a pyramidal form of atom has been employed by Stark in building up theoretical molecules, and in this paper Nicholson demonstrates the impossibility of their

existence in certain circumstances. The pyramidal atom consists of a nucleus, a ring of electrons in the form of a circle on whose axis lies the nucleus, and in addition a single stationary electron situated on the axis, this latter electron being the linkage by means of which binding with another atom is effected on Stark's view. Now the objections urged by Nicholson against Stark's model is a much more serious matter than those proposed in the case of other model atoms. No doubt a single ring of electrons in an atom on the Rutherford idea cannot be stable, but the disturbances which break up the atom on account of instability are those which are least likely to occur, so that, in spite of this possibility, the atom can have as long an existence in the form of an electrically neutral system, as is demanded by its known properties. But in the Stark model something more fundamental than stability is involved. Nicholson shows that no positively charged or neutral atom can exist in this form. Quite apart from any question of stability, it is not possible to satisfy the ordinary conditions for a steady rotation of the ring in such pyramidal atoms even in the absence of all disturbing forces. Besides, even though the atom may acquire an electron on its axis, if it is neutral or negatively charged before the operation, the ensuing structure is unstable as regards some of its more important vibrations. Its stability is not comparable even with that of a single ring, and it could not be endowed with any permanence. Consequently such atoms could never exist as positive or neutral systems, and could only have a very transitory life as negatively charged ones. As already stated, this conclusion is independent of questions of stability, and would hold no matter what system of dynamical reasoning is adopted, Newtonian or otherwise. This would appear to vitiate entirely all molecular constructions such as those of Stark.

PHYSICAL CHEMISTRY.

By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

False Equilibria.—The existence or non-existence of a false equilibrium in a chemical process is one of those problems which, after having attracted much attention and given rise to considerable controversy, eventually comes to be regarded

as quite definitely settled, until, after the lapse of a number of years, it is once more reopened in a new and acute form.

Twenty years ago P. Duhem (1), in his now classic researches on the thermodynamic treatment of chemical changes, had come to the conclusion that a false equilibrium could be realised in place of a true one, owing to the existence of viscous forces, which, by delaying the process, gave to the meta-stable state a degree of permanence which had much in common with the true equilibrium state. The distinction between this false equilibrium and the true lay in the fact that, on allowing the reverse process to occur, the position finally reached did not coincide with that attained in the direct process. The process was, in fact, thermodynamically irreversible.

This theoretical conclusion was apparently justified by a number of experimental results obtained by Pélabon (2), who investigated the reaction between hydrogen and selenium, and that between hydrogen and sulphur. It is essential to observe that in these reactions we are dealing with a solid or fused substance which interacts with a gas, *i.e.* the reaction is heterogeneous. Pélabon found that above 320° the true equilibrium was attained, but that at lower temperatures different end-points were reached according as to whether he started with the system hydrogen + selenium or with the system hydrogen selenide. Pélabon's results were adversely criticised, however, by Bodenstein (3), who reinvestigated the same reactions and came to the conclusion that in all cases a true equilibrium point was reached. Bodenstein regarded the limits of formation of hydrogen selenide or hydrogen sulphide found by Pélabon as simply arbitrarily selected points from a reaction which was still going on. Duhem (4), in reply to Bodenstein, pointed out that the actual limit of the reaction depended upon the mass of selenium or sulphur employed, the reaction being practically complete if sufficient fused or solid material were present, and apparently in Bodenstein's experiments much more sulphur was used than in those of Pélabon. Bodenstein (5), as a matter of fact, found that the presence of excess of selenium accelerated the reaction and permitted of a true equilibrium point being reached. The whole controversy has been very generally regarded as

being in Bodenstein's favour, and the theory of false equilibria came to be regarded as disproved.

The question has, however, been reopened recently by Bancroft (6), who has pointed out that in heterogeneous reactions we must expect in general to find *selective adsorption* or condensation of certain of the reactants or resultants upon the surface of any solid or liquid material present even in the case in which the solid or liquid material is itself taking part in the reaction, *i.e.* the surface of the sulphur or selenium. Owing to the specific nature of adsorption certain substances are adsorbed to a greater extent than others, and as a result of this the surface of the reacting solid or liquid may be so completely covered that the solid or liquid can take part no longer in the reaction, and hence the reaction is brought to a standstill prematurely, *i.e.* before a true equilibrium point has been attained. This view is a great deal more definite and clear than any based upon the rather hypothetical viscous or frictional forces referred to above. Obviously the "poisoning" effect of adsorption, as far as the solid or liquid is concerned, will be relatively greater the smaller the total amount of solid or liquid present. As Bodenstein's experiments have shown, the selenium acts as a catalyst for the reaction in which it itself participates, and it is reasonable to expect that sulphur acts in a similar manner. If the hydrogen sulphide or hydrogen selenide, produced as a result of the reaction, is very strongly adsorbed upon the sulphur or selenium, as Bancroft supposes, a phenomenon quite analogous to false equilibrium is to be anticipated, for the hydrogen sulphide or selenide will thereby "poison" the condensed reactant and prevent the reaction occurring further. As Bancroft says: "It is possible though not proved, that the discrepancy between Pélabon's and Bodenstein's results is due to Bodenstein having used relatively more sulphur and more selenium than Pélabon did, just as was claimed by Duhem. With varying amounts of catalytic agent to be 'poisoned' one may get all possible false equilibria between the limit of no reaction and the limit of reversible equilibrium. The theory of false equilibrium outlined is in harmony with all our thermodynamical relations." Analogous effects to those mentioned above have been observed in reactions which involve enzymes (heterogeneous reactions). The whole question of false equilibria

has taken on a new interest, such equilibria being now attributed quite logically to adsorption effects, effects which in the earlier days were not sufficiently recognised.

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6. BANCROFT, *Journ. Physical Chem.* **22**, 22 (1918).

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

UNDER the title of "The Crotonisation of Acetaldehyde" Sabatier and Gaudion (*Comptes Rendus*, 1918, **166**, 632) describe a peculiar series of reactions which result when ethyl alcohol vapour is slowly passed over uranium oxide at 360° C.; dehydrogenation and dehydration take place in one process, the alcohol being converted into crotonic aldehyde. Instead of starting from ethyl alcohol a better yield of crotonic aldehyde may be obtained by employing vapour obtained by heating paracetaldehyde. If the resulting product is roughly fractionated and the fractions 90–130° and 130–220° are separately passed together with hydrogen over reduced nickel heated to 170° normal butyl alcohol and normal hexanol are obtained.

The application of catalysts to the preparation of amines is illustrated by a recent paper of Mailhe and de Godon (*Comptes Rendus*, 1918, **166**, 467) in which these authors claim that by passing a mixture of the vapours of aniline and methyl alcohol in excess over aluminium oxide at 400–430° a mixture of mono- and dimethyl anilines is obtained; similarly toluidine and methyl alcohol passed over aluminium oxide at 350–400° give a mixture of mono- and dimethyl toluidines (*loc. cit.* p. 564). The same authors (*J. Pharm. Chim.* 1917 [vii], **16**, 225) also claim priority over Sabatier and Gaudier (*Comptes Rendus*, 1907, **165**, 224) in the discovery that primary amines may be converted into the corresponding nitrites by passing them over heated finely divided copper or nickel. Whatever the merits of the controversy may be, the reaction is an interesting example of reversible catalysis, the opposite reaction—namely, the hydrogenation of nitrites to amines by means of

nickel—having been described by Sabatier and Senderens in 1905. The preparation of tertiary amines from the halogen derivatives of hydrocarbons may, according to Matter (D.R.P. 301450 and 301832) be effected by means of sodamide. Thus if chlorobenzene is heated with sodamide at 110–120° triphenylamine is formed, while benzyl chloride under the same conditions yields tribenzylamine.

About a year ago Emmet Reid (*Journ. Amer. Chem. Soc.* 1917, **39**, 124 and 304) recommended the use of p-nitrobenzylbromide for the identification of acids and phenols. He found that when this reagent is boiled in alcoholic solution with the sodium salts of acids, esters are readily formed which can be identified by their melting-points ; in the same way sodium or potassium phenoxides gave solid phenolic esters. Brewster (*Journ. Amer. Chem. Soc.* 1918, **40**, 406) now states that good yields of this reagent may be obtained by gradually adding bromine, dissolved in carbon tetrachloride to a boiling solution of p-nitrotoluene, containing a trace of iodine, and exposing to sunlight.

Although the titration of glycine by means of formaldehyde is now a well-known and frequently employed method, the reaction between these two substances has not hitherto been explained. In a paper by Krause (*Berichte*, 1918, **51**, 136) it is shown that the reaction is somewhat complex, resulting in the formation of a condensation compound of the formula

$$HO.CH(CH_2.NH.CH_2COOH)_2.$$

When ordinary formalin is employed the methyl alcohol usually present in this substance takes part in the reaction as follows :

$$2NH_2CH_2COOH + 2CH_2O + CH_3OH = C_7H_{14}O_5N_2 + 2H_2O.$$

But if pure formaldehyde is employed, relatively more of this substance is used up and formic acid is produced at the same time, as shown by the following equation :

$$2NH_2CH_2COOH + 4CH_2O = C_7H_{14}O_5N_2 + HCOOH + H_2O.$$

The salts of glycine behave in a similar manner to the free acid.

A further contribution to the vexed question of the connecting link in carbohydrate formation during assimilation

in plants is made by Willstätter and Stoll (*Berichte*, 1917, **50**, 1777). The assumption originally put forward by Baeyer in 1870 was that formaldehyde was the intermediate compound between carbon dioxide and the carbohydrate; this view has been frequently attacked in recent years, and other compounds, such as oxalic or glycollic acids, have been suggested. According to the present authors formaldehyde is the only likely compound in the formation of which the volume of carbon dioxide absorbed is equal to the volume of oxygen liberated. The assimilatory quotient CO_2/O_2 for formaldehyde is 1, for glycollic acid it is 1·33, and for formic and oxalic acids it is 2 and 4 respectively. This quotient has now been accurately determined for a variety of different leaves, both ordinary foliage and the succulent leaves of the cactus; within a temperature range from 10–35° the figure has been found in every case to be unity whether the atmosphere was rich in carbon dioxide or deprived of oxygen altogether, thus forming a remarkable support for the accuracy of Baeyer's theory.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., F.G.S., University, Glasgow.

Geological Processes.—CHAMBERLIN, T. C., Diastrophism and the Formative Processes, IX., A Specific Mode of Self-promotion of Periodic Diastrophism, *Journ. Geol.* 1918, **26**, 193–7.

W. M. Davis reviews the whole question of coral reefs and submarine banks with reference to Darwin's theory of intermittent subsidence, and the glacial-control theory of Daly, which he regards as the only theories of reef formation deserving consideration at the present stage of the controversy (*Journ. Geol.* 1918, **26**, 198–223; 289–309 cont.). The general result of the discussion is that the long-enduring stability of reef foundations, and the abrasion of reefs and islands by the chilled and lowered glacial ocean, as demanded on Daly's view, are very improbable. Coral reefs are believed to be better explained by subsidence and aggradation than by stability and abrasion. Subsidence of considerable amount, aided by small changes of ocean level, have determined the conditions under which present-day sea-level reefs have been formed.

COTTON, C. A., Conditions of Deposition on the Continental Shelf and Slope, *Journ. Geol.* 1918, **26**, 135–60.

H. Dewey demonstrates the existence of a former upland plain of wide extension in Carnarvonshire, terminating at a

height of 430 feet against the Snowdon mountains (*Geol. Mag.* 1918, **5**, 145–57). Pre-glacial erosion has sculptured this plain into a region of hills and valleys which has its own drainage system independent of that of the mountain system adjoining it. A similar plain, believed to be not more recent than Pliocene, occurs in Cornwall and Devon at the same height. The evidence indicates that these plains were formed contemporaneously by marine erosion.

Prof. P. F. Kendall discusses in detail the origin of the splits in coal seams, especially the type in which a lense of dirt or sand with an arched roof and flat floor is intercalated in the coal. He ascribes these to the deposition of sediment along a stream channel in the original peat, covered later by a second layer of peat. In the subsequent contraction of the strata upon consolidation, the peat, being much more compressible, shrinks far more than the enclosed mass of sand, which, as shown by diagrams, must assume the form of a plano-convex lens with the convexity upwards. In the specific case of the Silkstone seam of Yorkshire the split in the seam, when mapped, occupies a long sinuous track like that of a meandering river—a fact which offers valuable confirmation of the ingenious theory put forward (*Trans. Inst. Min. Eng.* 1918, **54**, 460–79).

Stratigraphical and Regional Geology.—The Presidential Address of Dr. A. L. du Toit to the Geological Society of South Africa records the advances that have been made in the last fifteen years towards the elucidation of the detailed stratigraphy of the Karroo System (*Proc. Geol. Soc. S. Africa*, 1918, xvii–xxxvi). The Karroo occupies one half of the total area of the Union of South Africa; it has at its base the greatest Palæozoic glacial formation yet known; it is capped by a thick series of lavas and penetrated by a series of basic sills and dykes on a scale probably unsurpassed in any part of the world. In time it ranges from the Upper Carboniferous to the close of the Triassic, and was deposited under continental conditions upon the ancient Gondwana land mass. Its detailed investigation is therefore of great scientific interest, especially as it is also the source of South Africa's mineral fuel.

Dr. G. Hickling has compiled an excellent summary of the geology of Manchester as revealed by recent boring data (*Trans. Inst. Min. Eng.* 1918, **54**, 367–417). He shows that

there is no evidence of unconformity between the Bunter and Permian; and that the "Permian" sandstones, while they probably include strata of Upper Permian age, are in the main considerably older, and possibly the equivalents of the Keele Sandstones (Upper Coal Measures) of North Staffordshire. The courses of several large faults have also been satisfactorily determined.

In the Oamaru district, North Otago, New Zealand, fully described by Prof. J. Park (*Geol. Surv. N.Z.*, 1918, *Bull.* No. **20,** pp. 119), the oldest rocks are Early Palæozoic phyllites and mica-schists. On these basement rocks rests a great succession of Middle Tertiary strata. The lowermost beds are lignitic measures of quartz gravel and sand. Overlying them conformably is a great marine series with which are intercalated beds of volcanic mud and ash, and flows of basalt lavas. Some of the latter have the pillow form well developed as shown in an excellent photographic illustration.

Heron, A. M., The Biana-Lalsot Hills, Eastern Rajputana, *Rec. Geol. Surv. India*, 1917, **48,** part 4, 181–203.

Scrivenor, J. B., The Origin of the Clays and Boulder-clays, Federated Malay States, *Geol. Mag.* 1918, **5,** 157–68.

The Sooke and Duncan area (Vancouver Is.), described by C. H. Clapp (*Geol. Surv., Canada,* 1917, Memoir **96,** pp. 445) is a greatly dissected plateau of deformed Carboniferous and Lower Mesozoic rocks, invaded by granite batholiths, and then covered unconformably by a great series of Upper Cretaceous and Tertiary sediments and lavas. These rocks in their turn were folded, and invaded by small stocks of gabbro, which have differentiates of granite and anorthosite.

T. T. Quirke describes the Espanola district of Ontario, which forms part of the eastward continuation of the original Huronian area of Logan. For the most part the rocks are of Huronian age, with some older basement rocks consisting of highly metamorphosed sedimentary schists and slates, with intrusive granites and greenstones. These are equivalent to Coleman's Sudburian Series. The district is characterised by extensive thrust faulting (*Geol. Surv., Canada,* 1917, Memoir **102,** pp. 92).

Johnston, W. A., Pleistocene and Recent Deposits in the Vicinity of Ottawa, with a Description of the Soils, *Geol. Surv., Canada,* 1917, Memoir **101,** pp. 69).

SCHUCHERT, C., On the Carboniferous of the Grand Canyon of Arizona, *Amer. Journ. Sci.* 1918, **45**, 347–61; On the Cambrian of the Grand Canyon of Arizona, *ibid.* 362–9.

Geologia do Estado do Parana (Brazil), *Bol. Min. d. Agric. Indust. e. Comm.* Anno V., N. 1, 1916, Rio de Janeiro, pp. 67–143.

Igneous Rocks.—WASHINGTON, H. S., Chemical Analyses of Igneous Rocks published from 1884–1913 inclusive, with a Critical Discussion of the Character and Uses of Analyses, *U.S. Geol. Survey*, 1917, Prof. Paper **99**, pp. 1201.

The igneous mass of Cuttingsville (Vt.) is shown by J. W. Eggleston to be a composite stock, consisting of various syenites with essexite, arranged in a roughly concentric manner (*Amer. Journ. Sci.* 1918, **45**, 377–410). The order of eruption was from basic to acidic; and this is also the general order in the associated dyke rocks. This intrusion is correlated with the numerous small bodies of alkaline igneous rocks which occur scattered throughout the Quebec-New England region.

J. P. Iddings and E. W. Morley have published thirty new analyses of igneous rocks from the islands of Tahiti, Moorea, and the Society group (*Proc. Nat. Acad. Sci.* 1918, **4**, 110–17). Each island is a profoundly-eroded volcano consisting mainly of basaltic lavas rich in olivine and augite, which are occasionally accompanied by trachytes and phonolites. In two of the volcanoes deep erosion has exposed plutonic cores of gabbroic and theralitic rocks, and, in another case, syenites and nepheline-syenites.

Sedimentary Rocks.—GRABAU, A. W., Problems of the Interpretation of Sedimentary Rocks, *Bull. Geol. Soc. Amer.* 1917, **28**, 735–44.

BARRELL, J., Rhythms and the Measurement of Geologic Time, *ibid.* 745–904.

KINDLE, E. M., Diagnostic Characteristics of Marine Clastics, *ibid.* 905–16.

BLACKWELDER, E., Characteristics of Continental Clastics and Chemical Deposits, *ibid.* 917–24.

SHAW, E. W., Significance of Sorting in Sedimentary Rocks, *ibid.* 925–32.

VAUGHAN, T. W., Chemical and Organic Deposits of the Sea, *ibid.* 933–44.

GRABAU, A. W., Stratigraphical Relationships of the Tully Limestone and the Genesee Shale in Eastern North America, *ibid.* 945–58.

GRABAU, A. W., and O'CONNELL, MARJORIE, Were the Graptolite Shales, as a Rule, Deep or Shallow Water Deposits? *ibid.* 959–64.

BARROWS, A. L., Geologic Significance of Fossil Rock-boring Animals, *ibid.* 965–72.

The nine papers listed above are the products of a symposium held by the Geological Society of America on the interpretation of sedimentary rocks. This fascinating study is now attaining a great measure of popularity on both sides of the Atlantic, and aims at a reading of geological history informed mainly by the characteristics of the sedimentary rocks that build up the geological column. The longest and most important paper in the series is that by Prof. J. Barrell on sedimentary rhythms and the measurement of geological time. This contribution is worthy of a much more extended notice than can be given here. Briefly it aims at exhibiting the congruity of the evidence of the sediments, especially of their rhythmic alternations, regarding the age of the earth, with that afforded by the radioactive minerals. On the present data Prof. Barrell regards an estimate of 1400 millions of years as probable for the age of the first great invasion of the Laurentian granites. The beginning of the Cambrian is estimated at between 550 million and 700 million years. Palæozoic time is believed to have endured between 360 million and 540 million years; Mesozoic time between 135 millions and 180 millions; and Cainozoic time from 55 millions to 65 millions. Many problems in the accumulation of sediments are dealt with in a novel and stimulating manner. Finally the relations of organic evolution and of the source of solar energy to this vast expansion of geological time are examined.

Kindle describes interesting experiments upon the relative rates and sequence of the deposition of different sediments in salt and fresh water respectively. Clay, powdered chalk, and sand were fed into an experimental tank in the same order and amount to both media. In salt water the beds settled in proper order and with sharp lines of separation between the different materials. In fresh water, however, the order of superposition was different, because the clay was largely held

in suspension until all the sand had been deposited. Furthermore, the different classes of sediment were not sharply differentiated, but graded one into the other.

Prof. Grabau and Miss M. O'Connell, contrary to the generally-received opinion, have come to the conclusion that the graptolite shales of Southern Scotland and Scandinavia were deposited in very shallow water. This view is based on the remarkable transition in Moffatdale from fine, thin, black shales to thick, coarse, barren grits and conglomerates with marks indicative of continental deposition by rivers. This transition was mapped for each graptolite horizon; and it is thereby shown that Moffatdale lay in a small bay or lagoon, and could not have been in the open sea. The graptolite shales are regarded as mud deposits on the flood-plain and in the lagoons of a large delta, or series of deltas, where periodic high tides washed in the planktonic graptolites and stranded them on the flats. Occasionally they were swept into the regions of deposition of coarser sediments, and are hence now found in arenaceous sediments associated with worm-tracks and eurypterid fragments.

BLACKWELDER, E., The Study of the Sediments as an Aid to the Earth Historian, *Proc. Nat. Acad. Sci.* 1918, **4**, 163–7.

In this country there has recently arisen considerable discussion as to the origin of flint (see SCIENCE PROGRESS, July 1918, 39). In America the very similar substance chert has aroused a great deal of attention. W. A. Tarr (*Amer. Journ. Sci.* 1917, **44**, 409–52) has introduced a new theory of primary colloidal deposition of chert to explain the occurrence of this substance in the Burlington Limestone of Missouri. Other workers, however, are not inclined to accept this view. R. S. Dean shows by experiment that silica hydrosols are precipitated by calcium carbonate in the presence of carbon dioxide; while, in the absence of that gas, the carbonate acts as a stabiliser. To this reaction he attributes the formation of chert in the Mississippian and Cambro-Ordovician limestones of Missouri. The groundwater circulating in these limestones is known to contain a notable quantity of silica derived from adjacent shales. That the chert nodules are secondary and have been formed under pressure is shown by the bowing-up of the intercalated shale beds in which they are mainly found (*Amer. Journ. Sci.* 1918, **45**, 411–5). F. M. van Tuyl (*ibid.* 449–56)

believes that most occurrences of chert may be satisfactorily explained on the replacement hypothesis. The following features are indicative of replacement: occurrence of chert along fissures and the irregular shape of some chert nodules; the presence of patches of limestone in some chert masses and the association of chert with silicified fossils; preservation of structures and textures of other rocks in some cherts; failure of some chert to follow definite zones in limestone formations; and the occurrence of silicified oolites.

Barton, D. C., Notes on the Mississippian Chert of the St. Louis area, *Journ. Geol.* 1918, **26**, 361–74.

From a petrological study of the Leicestershire dolomitic limestone, L. M. Parsons arrives at the conclusion that the majority of the rocks are of contemporaneous origin, that is, formed from ordinary limestones by replacement in sea-water at or soon after deposition (*Geol. Mag.* 1918, **5**, 246–58). They are situated between the more normal Carboniferous Limestones of Derbyshire and the Charnwood region of ancient rocks. It is suggested that the dolomitic limestones have been formed in shallow portions of the Carboniferous sea adjacent to the old Charnwood land surface.

Economic Geology.—Emmons, W. H., *The Principles of Economic Geology*, 1918, pp. 506 (McGraw & Hill Book Co.).

The first part of the Geological Survey Memoir on refractory materials, dealing with the sources and geology of ganisters, silica-rocks, sands for open-hearth furnaces, and dolomite, is now published (Special Reports on the Mineral Resources of Great Britain, vol. vi. Refractory Materials. *Mem. Geol. Surv.* 1918, pp. 233). The accounts of fireclays and moulding-sands, and the details of the laboratory work on refractories generally, form the subjects of succeeding parts of the memoir. Refractory materials of many sorts are abundantly distributed in the British Isles, but their industrial use involves considerable selection and investigation.

According to a report by L. Hinxman and M. Macgregor on the distribution and geological position of the valuable fireclays and ganisters of the South of Scotland, the Millstone Grit is the chief source of refractory materials over this region, although they occur in all the remaining subdivisions of the Carboniferous. Large areas are still inadequately prospected for the highly refractory Millstone Grit fireclays. These rocks

differ greatly from ordinary underclays, and appear to have been deposited as fine siliceous muds in still and probably landlocked waters (*Trans. Ceramic Soc.* 1917–18, **17**, 35–56).

In an article on British supplies of potash felspar, Prof. P. G. H. Boswell summarises the requirements of the pottery and glass industries in respect to this mineral, and also discusses its use for the extraction of potash. A survey of British resources of felspar shows that no large supplies as good as those of Scandinavia are available; but further exploration, especially in Scotland, should be made (*Trans. Soc. Glass Tech.* 1918, **2**, 35–71).

Boswell, P. G. H., *A Memoir on British Resources of Sands and Rocks used in Glass-making, with Notes on Certain Crushed Rocks and Refractory Materials.* Second Ed., complete in one volume, 1918, pp. 183 (Longmans and Co.).

Dr. F. L. Stillwell regards the quartz reefs of the Bendigo goldfield (Vict.) as deposited from ascending aqueous solutions, but the solutions are believed to have been forcibly intruded in the same manner as igneous dykes (The Factors influencing Gold Deposition in the Bendigo Goldfield. *Commonwealth of Australia, Advisory Council of Science and Industry,* 1917, Bull. No. **4**, pp. 68; a summary of this bulletin is to be found in *Econ. Geol.* 1918, **13**, 100–11). A laminated variety of quartz is very common and frequently carries the most gold. This material is believed to be due to replacement of slate by quartz. A. M. Bateman criticises the above conception of origin (*Econ. Geol.* 1918, **13**, 222–3), and points out that if the ore deposits are to be accounted for by forcible intrusion with replacement, they might be expected to occur in almost any part of the folded rocks in which they are found, especially in the sheared limbs of the folds, and not so generally confined as they are to the crests.

Brown, J. C., Geology and Ore Deposits of the Bawdwin Mines (Burma), *Rec. Geol. Surv. India,* 1917, **48**, pt. 3, 121–78.

Lindeman, E., and Bolton, L. L., Iron Ore Occurrences in Canada, vol. i. *Dept. Mines, Canada,* 1917, pp. 71.

Imperial Institute, London. Zinc Ores, *Monographs on Mineral Resources with Special Reference to those of the British Empire,* 1917, pp. 64.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., East London College, University, London.

Morphology.—W. Brotherton and H. H. Bartlett have studied the variation of the size of the epidermal cells from the stems of Phaseolus (*American Journal of Botany*, April 1918) in light and darkness. They emphasise the importance of comparing those cells which occupy corresponding positions on the curves of variation. The work of G. Kraus and MacDougal has shown that in etiolated plants the increased length of the internodes is mainly a result of an increase in the length of the cell units, though in part an outcome of their larger number. The present authors find that under constant conditions variation in length of the internodes appears to be associated mainly with an increase in the number of the cells. The effect of light is regarded as directly or indirectly to retard cell division. There is apparently a physiological limit to the size which a primary cell can attain without undergoing division. In each internode there was an increase in the size of the cells from the base upwards till a maximum was reached followed by a diminution, at the top of the internode, which however was not so great as at the base.

Miss L. A. Tenopyr (*Bull. Torrey Bot. Club*, February 1918) has studied the size and shape of cells in leaves of different forms on the same plant (*Campanula rotundifolia* and *Lobelia erinus*) and in closely related species or varieties. In the common Harebell it was found that the broad radical leaves had epidermal cells with an average length of 0·04 mm. and width of 0·039. Those of the linear cauline leaves had an average dimension of 0·034 × 0·032 mm., and transitional leaves 0·038 × 0·034 mm. The results thus agree with those of Sierp, who found that in *Mirabilis jalapa*, *Nigella damascena* and *Pisum sativum* the higher the leaf was situated on the stem the smaller its component cells. Similar results were obtained for *Lobelia erinus*. A comparison of the epidermal cells from the lobed leaves of the common Chicory with those of an entire leaved variety showed that the latter were larger but of the same shape.

Correlation between morphological features of *Phaseolus* have been investigated by Harris and Avery (*Bull. Torrey Bot. Club*, March 1918). Usually three primordial leaves are associated with tricotyledonous seedlings and the latter more

frequently show fasciation of the axis and variability generally than those which are normal.

Anatomy and Cytology.—Dr. R. C. Davie summarises the results (*Ann. Bot.* April) of his examination of the pinna traces of Ferns in ninety genera and over 200 species. One hundred and twenty-six species show the marginal type of pinna trace and 96 species the extra-marginal type in which the pinna traces arise from the back of the leaf trace margin. In a few genera, viz. *Balantium, Leptochilus, Microlepia, Odontosira, Coniogramme, Notholæna,* and *Onychium,* both types occur.

M. Ishikawa Rigakushi (*ibid.*) describes the embryo sac and fertilisation in *Œnothera nutans* and *O. pycnocarpa.* The E.S. arises from either the chalazal or micropylar end of the tetrad and lacks the antipodals and one of the polar nuclei. There are thus four nuclei, consisting of the remaining polar nucleus, the egg nucleus, and the two synergids, as in *Ludwigia, Gaura, Godetia,* and *Circæa.* Double fertilisation occurs, resulting in a diploid endosperm.

Ecology.—W. H. Brown, E. D. Merrill and H. S. Yates contribute to the *Philippine Journal of Science* (vol. xii. No. 4) an account of the " Revegetation of Volcano Island, Luzon." This island is about 25 sq. kilometres in extent, surrounded by the waters of Lake Bourbon, which separate it from the mainland. Prior to the eruption of Taal Volcano in 1911 the vegetation appears to have been chiefly grass and small trees (more especially *Saccharum spontaneum* and *Ficus indica*). As a result of the eruption most of the vegetation was destroyed, except in the northern part of the island, where a number of bamboo clumps and some Bananas survived, relicts of the former villages.

The chief invading species in the recolonisation are grasses, but even after six years the vegetation is still only sparse, probably owing to erosion, the low water content, and high proportion of sulphates present in the soil. The following list embraces those species which are enumerated as very common and widely distributed: *Acacia farnesiana, Antidesma ghæsembilla* (eaten by birds), *Bulbostylis barbata* (eaten by animals), *Callicarpa blancoi* (eaten by birds), *Eugenia jambolana* (birds and man), *Ficus hauili* and *F. indica* (both eaten by birds), *Ipomœa pescapræ* (water), *Morinda bracteata* (? by

water, eaten by birds), *Pithecolobiam dulce* (eaten by birds), *Saccharum spontaneum* (wind), *Tabernæmontana subglobosa* (birds), *Trema orientalis* (by birds). Of the total of 292 vascular plants recorded since the eruption, 54 per cent. have probably been introduced by the agency of birds, about 20 per cent. by wind, and about 10 per cent. by water. The order of importance of these three agencies is thus the reverse of that for the revegetation of Krakatau, a difference that is probably to be attributed to the much longer distance of travel. So, too, with respect to the pioneer species, the role played by the lower cryptogams, which in the case of Krakatau was considerable, is here insignificant.

The lichen *Bilimbia artytoides* and a few mosses and hepatics attain a limited and local abundance in the damp ravines, but blue-green algæ have not been observed.

J. W. Shive and W. H. Martin (*American Journal of Botany*, April 1918) have demonstrated for Buckwheat grown in culture solutions that the optimal requirements of nutrient salts vary with the period of growth. The best growth of both tops and roots up to the period of flowering was found to be KH_2PO_4, 0·0144 m.; $Ca(NO_3)_2$, 0·0052 m.; $MgSO_4$, 0·02 m., whereas the best growth when grown to maturity was obtained with the same salts in the proportions 0·0108 m.; 0·013 m. and 0·1 m. respectively.

In the *Journal of Ecology* for March 1918, Dr. W. G. Smith deals with the ecology of *Nardus striata* and concludes that it is essentially characteristic of the zone marginal to retrogressive peat where the latter has been redistributed by various agencies. Irrigation, as also manuring, tends to reduce the amount of *Nardus* present in pasture.

In the same Journal Salisbury deals with the *Quercus sessiliflora-carpinus* woods of Hertfordshire and compares them with the woods of that county dominated by *Quercus robur*. The soils on which *Quercus sessiliflora* occurs are, in general, lighter, with a lower proportion of mineral salts than those where its congener predominates.

The ground-flora exhibits several well-defined societies dominated respectively by *Pteris aquilium, Rubus fruticosus, Anemone nemorosa, Nepeta glechoma, Mercurialis perennis* and *Ficaria verna.* The last two are particularly associated with areas of low acidity or high water content. In addition, the

flora of the paths and of the wood margins form well defined assemblages which supply the greater part of the species characterising the coppiced areas. These latter show a higher acidity than the uncut wood, and there is a marked diminution in the humus content of the soil of such areas.

In general the flora, which comprises some 269 phanerogams and Ferns, and about 130 cryptogams exclusive of Fungi, is essentially calcifuge in character.

W. H. Pearsall (*ibid.*) describes the Marsh and Fen Vegetation of Esthwaite Water and deals with the classification of Aquatic Plant communities.

The structure and development of the Plant Association is discussed by H. A. Gleason (*Bull. Torrey Bot. Club,* October 1917).

In a paper on the Floras of Eastern and Western Newfoundland (*Amer. Journ. Bot.*) M. L. Fernald lays emphasis on the fact that Newfoundland comprises a calcareous western region north of Bay St. George and the North Peninsula, whilst the central tundra, the south-eastern and south-western areas, are essentially siliceous and acid. The west is warm, sunny and fertile, yet has a flora of arctic facies, whilst that of the colder areas is more southern. The soil differences appear then to be more important than the climatic ones.

Taxonomy.—The *Journal of Botany* (for May) contains descriptions of new species of *Gouldia, Timonius,* and *Psychotria* by Dr. Wernham, of *Palinurus* and *Calimintha* by Mr. Wilmott, and of *Sedum* by Mr. Lloyd Praeger. In the June number Mr. Baker describes new species of *Sauranja* and the following month Mr. Spencer Moore gives diagnoses of additions to the following genera: *Gutenbergia, Erlangea, Vernonia* and *Jatropha.*

Prof. Bower (*Journ. Linn. Soc.* May) in the Hooper Lecture delivered last year, deals with the natural classification of plants, as exemplified in the Filicales.

An elaborate study of the species *Lupinus densiflorus* is contributed by C. P. Smith (*Bull. Torr. Bot. Club,* May) in which the author distinguishes twenty-six varieties, most of them new.

Prof. West has described and figured a new British species of *Gongrosira* (*Journ. Roy. Micr. Soc.* March 1918) under the name of *G. Scourfieldii.*

Economic Botany.—The *Indian Forest Record*, vol. vi. pt. 2, contains statistics relating to the growth and increment of various Indian trees, viz. *Shorea robusta*, *Dalbergia sissoo*, *Acacia catechu*, *Terminalia tomentosa*, *Tectona grandis*, *Quercus dilatata*, *Q. incana*, *Pinus longifolia*, *P. khasya* and *Cedrus deodara*.

M. de Launay (*La Nature*, May 18, 1918) describes the intensive culture of paper pulp in the Sierra Morena region, by a French company which exploits lead and coal mines. The greatest success has attended the planting of *Eucalyptus*, of which several kinds are employed. These trees are available for use in about twelve years. They are planted about 2,500 to the hectare and in three or four years' time these plantations already present the appearance of forests. Pines were also planted, but, compared with the *Eucalyptus*, were much slower in growth and were subject to fires, which do not affect the latter. The trees are employed in part for pit-props, for the supply of which the experiment was started; but, owing to its success, the original area has been quadrupled, and paper pulp, acetic acid, wood-spirit, and charcoal all form important products of the enterprise.

Various contributions have appeared recently on the subject of British Forestry [Forestry Sub-Committee Final Report, 1918, (ii) Salisbury, Timber Production in Britain, in Exploitation of Plants, 1917, (iii) Stirling-Maxwell, Scientific Forestry for the United Kingdom]. In the first of the papers cited the sub-committee emphasise the poor yield of the land at present under forest, and the consequent necessity for improving the conditions and enlarging the afforested area. It is estimated that two million acres could be planted without decreasing the home production of meat by more than 0·7 per cent., and the increased employment would amount to ten times that at present required. Such an area would provide a three years' supply. In the second paper stress is laid on the necessity for denser planting of deciduous trees, the subordination of game preservation to the requirements of forestry, and the extermination of rabbits. The value of ecological research to Forestry is also emphasised. In the third paper the author very justly pleads for more scientific methods, and in this connection points out the importance of trained forest officers.

The *Journal of the Royal Horticultural Society* for May

contains an account of investigations on the Narcissus disease by J. K. Ramsbottom. Originally attributed to a fungus, *Fusarium bulbigenum*, it was found that this parasite was very rare and that the main factor was an eel-worm, *Tylenchus devastatrix*. This conclusion was confirmed by inoculation experiments. The same species of eel-worm attacks Rye, Oats, Clover, Onions, and Hyacinths, and the experiments did not provide support for the view that each is the victim of a special biological strain. The author recommends soaking the bulbs for from 2–4 hours in water at 44° C. as a preventive measure. Infection of the attacked bulbs by parasitic fungi is not infrequent, but in all cases this appears to be secondary.

PLANT PHYSIOLOGY. By INGVAR JÖRGENSEN, Cand. Phil., D.I.C., University College, London. (Plant Physiology Committee.)

Carbon Assimilation.—The many hypotheses concerning the chemical aspect of carbon assimilation are considered in a recent monograph by H. Schroeder, "Die Hypothesen über die chemischen Vorgänge bei der Kohlensäure-Assimilation und ihre Grundlagen" (Jena 1917, pp. 168). As the author cautiously points out, the problems of carbon assimilation are not likely to be solved in the near future, and it is far more important to realise the limitations for experimental attack; no hypothesis can be proved: all that can be expected is greater or less probability. The need for physiological experiments is obvious; chemical, synthetic, and analytical work alone can scarcely prove convincing in regard to such a process as carbon assimilation, which occurs with such rapidity. The author admits that none of the hundreds of hypotheses he deals with can claim even probability, therefore he does not consider it justified to put forward any new hypothesis himself, as it probably would only share the fate of its predecessors. Such a cautious attitude is not adopted by Willstätter and Stoll, who in the last few years have presented several new hypotheses. In their last communications, "Über die Baeyersche Assimilations Hypothese" (*Ber. d. deut. chem. Ges.* **50**, 1771–91, 1917) and "Über das Verhalten des kolloiden Chlorophylls gegen Kohlensäure (*Ber. d. deut. chem. Ges.* **50**, 1791–1801, 1917), they renew the discussion of Baeyer's formaldehyde hypothesis. Their method of attack is so far noteworthy that an attempt

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is made to furnish a physiological proof for this well-known hypothesis. Unfortunately the basis on which this proof rests is not very satisfactory. It consists of an examination of the CO_2/O_2 ratio in gaseous exchange. If this is equal to unity under all conditions the authors appear to consider it proved that formaldehyde can be the only intermediate product in carbon assimilation. They do indeed find that under all conditions of experiment, even with prolonged intense assimilation, the ratio between the carbon dioxide taken in and the oxygen evolved is constant and equal to unity. They conclude, therefore, that no intermediate products of reduction other than formaldehyde are formed and accumulated, as otherwise the CO_2/O_2 ratio would undergo alteration. Apart from theoretical considerations, this investigation presents many novelties in regard to technique. For the first time since Boussingault a dynamic method is employed in which currents of gas pass over the leaves. That part of the investigation dealing with the assimilation of succulents is of particular interest, as it is shown that even here the CO_2/O_2 ratio approaches unity if assimilation continues over a long enough period.

The rapid growth of interest in the problems of carbon assimilation has resulted in a certain amount of discussion and criticism of previous work. The most substantial contributions in this respect come from W. H. Brown and G. W. Heise, "The Application of Photochemical Temperature Coefficients to the Velocity of Carbon Dioxide Assimilation" (*Philippine Journal of Science*, C. **12**, 1–25, 1917), "The Relation between Light Intensity and Carbon Dioxide Assimilation" (*ibid.* 85–97). These papers constitute a critical review of earlier work, but deal especially with the writings of F. F. Blackman and his pupils. It was clear, after the criticism to which Willstätter and Stoll subjected the work of Miss Irving on the assimilation of etiolated leaves, that Blackman's hypothesis regarding the interaction of factors conditioning assimilation (the principle of limiting factors) could no longer be regarded as valid. Brown and Heise's criticism renders Blackman's interpretation of his own results and those of his pupils still more improbable. In their first paper Brown and Heise consider the work of Miss Matthaei and of Blackman and Smith on the influence of temperature on carbon assimilation. It is shown that a critical examination of the work of these writers leads to

the conclusion that the temperature coefficient of carbon assimilation is not approximately 2, as Blackman avers, but rather approximates to unity, and so falls into line with the temperature coefficients of other photochemical reactions. Moreover, the critical examination of the data provided by earlier workers leads to the same conclusion. In their second paper Brown and Heise show that the data at present available regarding the relation between light intensity and carbon assimilation indicate that there is no direct proportionality between light intensity and assimilation, but for each increase in light intensity there is a progressively smaller augmentation of the rate of assimilation. This decrease of the rate of augmentation continues until a point is reached at which further increase in light intensity produces no measurable increase in assimilation.

The bubbling method, which so far, in spite of its extensive use, has, owing to its many inaccuracies, yielded few results of any value, has lately been much improved as a result of extensive researches undertaken in Kniep's laboratory. A paper by Hilda Plaetzer, " Untersuchungen über die Assimilation und Atmung von Wasserpflanzen " (*Verhandl. phys. med. Ges. z. Würzburg*, **45**, 31–102, 1917), in which these improvements of the method are embodied, is of special interest. The investigation deals largely with determinations of the " compensation point," *i.e.* the light intensity where assimilation and respiration just balance each other so that no gaseous exchange takes place. When water-plants with an intercellular system, *e.g. Elodea canadensis, Cabomba caroliniana, Myriophyllum spicatum*, were used as experimental objects, the bubbling method was employed. When water-plants without an intercellular system—*e.g. Spirogyra, Cladophora, Fontinalis*—were used, the assimilation and respiration were ascertained by determining oxygen and carbon dioxide content of the water in the experimental vessel. At a definite temperature the compensation point is different for the various species. It is not clear what factors condition this, but it is not merely the magnitude of the respiration. The position of the compensation point for any one species depends on the temperature. The light intensity decreases with the temperature, as seen from the following numbers :

Spirogyra at 20° C., 174 H.K. ; at 5° C., 26·7 ; Cladophora

at 20° C., 235; at 5° C., 62·9; Fontinalis at 20° C., 150; at 5° C., 40; Cinclidotus at 20° C., 400; at 5° C. 75.

No evidence is found for the assumption that respiration is increased by small increases of light intensity. The respiration of the plants examined decreases when the plants are placed in the dark, also during the night. Spirogyra forms an exception in so far that the first night after having been placed in the dark chamber its respiration increases. The author considers that this increase of respiration is due to nucleus and cell division which takes place during the night.

Although the observations recorded in this paper serve to indicate clearly the complexity of the various external and internal conditions, yet little light is shed on the mode of their interaction.

The question of the relation between carbon assimilation and other cell processes is also dealt with in a paper by G. Karsten, "Über die Tagesperiode der Kern- und Zellteilungen" (*Zeitsch. f. Bot.* **10**, 1–20, 1918). The opinion is expressed that light hinders the cell and nuclear division.

Osterhout and Haas, "Dynamical Aspects of Photosynthesis" (*Proc. Nat. Acad. Sci.* **4**, 85–91, 1918) and "A Simple Method of Measuring Photosynthesis" (*Science*, **47**, 420–22, 1918), propose a new method for measuring the assimilation of aquatic plants, which they assert is more accurate than those previously in use. The method is essentially a colorimetric one. To the water containing the plant tissue a trace of phenolphthalein is added, and the change in hydrogen ion concentration resulting from removal of carbon dioxide by assimilation is followed by comparison with the colour of standard buffer solutions. The experiments were conducted on a number of algæ and on *Potamogeton*, and from their results the authors conclude that the rate of assimilation increases for the first hour or two after exposure to light before becoming constant. The authors put forward two hypotheses which they think will account for this result, both of which involve the assumption that a catalyst is produced under the influence of light. The discussion of these authors' experiments and conclusions would be premature before more details are available, but it would appear that they under-estimate possible disturbing influences in their experiments.

A much-needed development of the quantitative measure-

ment of light is heralded by a paper by MacDougal and Spoehr, "The Measurement of Light in some of its more Important Physiological Aspects" (*Science*, **45**, 616–18, 1917), in which some preliminary measurements of light intensity in the blue-violet region of the spectrum is carried out by means of the photo-electric cell originally suggested by Elster and Gestel.

With regard to the products of assimilation, Kylin, "Zur Kenntnis der wasserlöslichen Kohlenhydrate der Laubblätten" (*Hoppe-Seyler's Zeitsch. physiol. Chem.* **101**, 77–88, 1918), has published the result of some analyses in respect to carbohydrates of foliage leaves of a number of species, including *Convallaria majalis*, *Gentiana brevidens*, *Tillia europæa*, and *Taraxacum*. The leaves were gathered at midday during May and June. He reports that, broadly speaking, the amount of glucose in the leaves varies inversely with the amount of starch, but *Convallaria majalis* contained little of both. Although sugar-leaves as a rule contain more sucrose than starch leaves, *Gentiana brevidens*, which is a sugar leaf, contains no sucrose, while the lime (a starch form) contains sucrose in considerable quantity. With regard to these results it may be pointed out that the methods of analysis are considerably less developed than those employed by Davis, Daish, and Sawyer, or even those of Gast, which were reported upon in these pages a year ago.

Finally may be mentioned a paper by Lakon, "Über die Bedingungen der Heterophyllie bei Petroselinum sativum (Hoffm.)" (*Flora*, **10**, 34–51, 1917), in which one of the most interesting aspects of carbon assimilation is dealt with—the relation of this process to the life of the plant as whole.

PALÆOBOTANY IN 1917. By Marie C. Stopes, D.Sc., Ph.D., University College, London.

With the exception of a large volume by Prof. Seward (vol. iii. *Fossil Plants*) there appears to be nothing of any size published in Palæobotany in 1917, although there are three or four papers of particular interest and importance.

Again this year there is a sad depletion of the ranks of the experts in the science of Palæobotany, and death has taken at least three whose names will long remain in the memory of students of the subject.

The obituary of Clement Reid, who died in 1916, appeared

in the *Journal of Botany*, June 1917. Reid's Palæobotanical work had dealt pre-eminently with the late Tertiary, and he, with the assistance of Mrs. Reid, had become the leading authority on the small and difficult fragmentary remains such as seeds in the Tertiary, and semifossil plants.

The two other notable losses occurred in France. Grand'-Eury, the famous veteran expert of the coal-fields, whose classical works on coal deposits and the plants associated with them are a monument in themselves; and Prof. C. E. Bertrand, following so soon Profs. Zeiller and Lignier, leave France bereaved of nearly all her brilliant and distinguished school of Palæobotanists. To the death of Prof. Bertrand was added the peculiarly poignant tragedy that he and his precious collections in Lille were in the hands of the Germans. He had been allowed to continue his work, but it needs little imagination to understand how much less able to resist disease he must have been with so sad a heart.

From France this year there comes little Palæobotanical work. One memoir which must be of great importance and is a continuation of the classic series on the Loire has, it seems, been published, but has not yet reached any of the libraries where it is available, so that I have not seen it. It is by Bureau, *Bassin de la basse Loire*, Fasc. 2, *Description des flores fossiles* (*Études gites min. France*, 4° ill.).

General Palæobotany.—The most important work of the year is the continuation of Prof. Seward's valuable text-book. In vol. iii. he deals with the Pteridospermeæ, Cycadofilices, Cordaitales, and Cycadophyta, thus leaving the whole of the Gymnosperms and Angiosperms for future treatment. This volume is as well illustrated and as full in its detail as the preceding parts of this exhaustive work, and brings up-to-date the consideration of all the families on which it touches. Consisting of 656 pages of text and 253 illustrations, it is a great achievement in war-time. This volume, like its predecessor, deals impartially both with impressions and structural material, and is illustrated by a very large proportion of new and excellent figures.

One or two smaller contributions to general Palæobotany appeared in semi-popular journals, several in Germany by R. Krausal. Mention might perhaps be made of one, on the leaf-form of *Ginkgo* (*Centralb. Min. Geol. Palæont.* p. 63) in

which he illustrated the very great variety of leaf-form in the living plant, comparing it with a fossil so-called species, and concluding that a revision of the fossil species is necessary.

Several important papers on FUEL, notably two parts continuing J. J. Stevenson's invaluable critical survey, " Interrelations of the Fossil Fuels," pt. ii. on the Cretaceous coals, and pt. iii. on the Jurassic and Triassic coals, have appeared this year (*Proc. Amer. Phil. Soc.* vols. lvi. and lvii.). In these surveys Stevenson dealt with the whole world, most usefully summarising and critically examining evidence drawn from the immense scattered literature on the subject.

Zalessky concealed in Russian, without any abstract, a paper entitled " Sur le saprop\u00e9lite marin de l'\u00e2ge silurien, form\u00e9 d'une algue cyanophycee," which, from its title and the illustrations, must be of exceptional interest. Hitherto English, French, German, or Latin r\u00e9sum\u00e9s, at least, have been obligatory on those producing scientific work published in such a language as Russian. A paper like this raises the question whether the facts are published at all in an international sense.

Conacher, in " A Study of Oil Shales and Torbanites " (*Trans. Geol. Soc. Glasgow*, vol. xvi. pt. ii.), published a most useful and well-balanced account of the oil shales, a difficult subject on which far too much is written by people who do not know what they are talking about. Conacher rightly insisted on certain fundamental similarities between coal and oil shales of some types. Critically considering previous views on the subject, he specified " Resin " as being the principal oil-yielding ingredient, and thinks that animal matter has played no appreciable or essential part in giving to oil shales or torbanites their valuable properties.

Of shorter papers, Thiessen's conclusion that the spore-exines have stratigraphic value in coal seams was given in abstract in *Science*, May 10; and Jeffrey (*Proc. Nat. Acad. Sci.* vol. iii.) came to various heterodox conclusions about coals.

Fischer (*Stahl u. Eisen*, vol. xxxvii.) gave an important survey of the present position of coal research.

Stratigraphic Palæobotany.—DEVONIAN plants were added to by Johnson (*Sci. Proc. Roy. Dublin Soc.* vol. xv.), who described some Pteridosperms from Ireland.

Kidston and Lang (*Trans. Roy. Soc. Edinburgh,* vol. li.) described with ten most beautiful plates, largely of microphotographs of structure, exceptionally interesting petrified remains of *Rhynia* from the Old Red Sandstone from Aberdeenshire. These plants were represented in the Chert by so large a number of excellently preserved remains that the genus is now the most completely known of all Devonian plants. The primitive nature of the genus will make it a great treasure to systematists and others concerned with the phylogeny of the lower plants. As described by Kidston and Lang, the genus was entirely composed of branched cylindrical stems, with neither roots nor leaves, the plant having grown in a gregarious fashion with some aerial branches ; on the latter were small lateral projections probably detachable for vegetative propagation. The plant bore large cylindrical sporangia with spores all of one kind. The affinities of such a plant are naturally the subject of much speculation. The authors concluded that it was more nearly allied to Dawson's *Psilophyton princeps* than to any other fossil, and they indulged in attractive comparisons and grouping—without, however, even mentioning the existence of the only two really important and recent memoirs dealing with Dawson's extremely unreliable data. It is indeed inexcusable to deal with *Psilophyton* and neither to mention David White's valuable memoir published in 1905 nor that of Halle on the lower Devonian plants from Norway published last year and drawn attention to in this Journal.

The Carboniferous was represented by a very important work by Kidston and Jongmans, the text of their monograph on the Calamites of Western Europe. The plates in this classical memoir were published in 1915 and in the present text detailed descriptions of Calamites proper were given. The authors considered the group from the external appearance of impressions and stem-casts and so on, not dealing with the petrified anatomy of internal parts.

Kidston also contributed two memoirs on the British Carboniferous :

(*a*) " Fossil Plants from the Scottish Coal Measures " (*Trans. Roy. Soc. Edin.* vol. li. p. 709) described seven new or interesting Carboniferous plants, of which five were new. Each is recorded with a short, precise description and excel-

lent illustrations. The specimens were all "impressions," and the following species were described: *Sphenopteris incurva*, sp. n., foliage with curved terminal pinnule; *Sphenophyllum cuneifolium* Sternb. f. *amplum* with exceptionally large leaves; *Sigillaria elegans*, leaf scars; *S. incerta*, n. sp., leaf scars whose arrangement is intermediate between the Favularia and Clathraria sections of the genus; *S. Strivelensis*, n. sp., leaf scars; *Stigmaria minuta* Goep., rhizome with root scars; *Lagenospermum parvulum*, n. sp., small seeds entirely surrounded by cupules.

(*b*) R. Kidston, in an important memoir on "The Forest of Wyre and Titterstone Clee Hill Coal Fields" (*Trans. Roy. Soc. Edin.* vol. li. p. 999), gave detailed lists of the fossil plants of the various horizons from different localities; a synopsis of all the plants known from the Westphalian and other series; careful critical descriptions of various of the more interesting specimens, among which are *Annularia spicata* Gutb., *Cingularia typica* Weiss, *Carpolithes membranaceus* Goep., and the new species *Cingularia Cantrilli*, n. sp., *Sigillaria Pringlei*, n. sp., followed by *Sphenopteris Dixoni*, n. sp. An interesting record of the well-known plant *Neuropteris heterophylla* is given from the Keele group, which appears to be the first record of the plant in the Radstockian series. Dr. Kidston's conclusions do not confirm those published by Dr. Arber on the Staffordshire Coal Measures. The plates in this paper have maintained the pre-war excellence which characterises Dr. Kidston's work.

R. Etheridge, Jun., "Arrangement of the Leaves in the Australian species of *Noeggerathiopsis*," Postscript by A. C. Seward (*Geol. Mag.* p. 289), illustrated two very beautiful leaf-clusters of *Noeggerathiopsis*, in one of which 7, in the other 9, radiate apparently from a common centre, indicating a whorled or very close spiral arrangement. The specimens are of "Upper Coal Measure (Permo-Carboniferous) age." The short paper included many references to the literature on the subject.

Linking Palæozoic and Mesozoic, we have H. Yabe, on the "Geological and Geographical Distribution of Gigantopteris" (*Sci. Rep. Tokyo Imper. Univ.* ser. 2, vol. iv. No. 2), giving a detailed study of the age and distribution of this rare genus, with a map showing the localities at which it has been found in America and Eastern Asia. In the paper are descriptions

of three Asiatic species by Koiwai, one identified with Schenk's species *G. nicotinæfolia*, one with Yabe's species *G. dentata*, and one new, though not yet named.

The two most important contributions to the MESOZOIC both come from the Antipodes:

A. B. Walkom, in publications 257 and 259 of the Queensland Geological Survey, continued and concluded his Flora of the Ipswich and Walloon Series. In these parts he described a large number of species, the genera being those universal for the Mesozoic and many of the species the same as those widely prevalent in Europe. The genus *Thinnfeldia* was treated at greater length than the others, and four species were described; a most important addition to the knowledge of this puzzling but widespread and common genus is made by the description of fertile specimens. Details not only of the sori are visible, but the structure of the sporangium is given, as in a number of them the cell-walls can be observed. It appears that there is no annulus. The sporangia are in groups of 3–5 in the sorus. This information is of great value; though some authors may be divided over the question whether the plant is a true *Thinnfeldia*, as most people think, or a *Dicroidium*, as Gothan decided. The author pointed out that the separation of the two artificial genera is based on no stable generic character.

In a later part of the work a new species was described as *Otozamites Queenslandi*; but the illustrations do not bear out the generic allocation. Detailed comparisons of these plants with those of the rest of the world are reserved for a further publication. In the meantime, it seems obvious that the Ipswich and Walloon beds are of two distinct ages.

E. A. N. Arber in Bulletin 6 of the New Zealand Survey dealt with the earlier Mesozoic floras of New Zealand in a very thorough and careful manner, including in the memoir not only descriptions of the plant species, but maps, a synopsis of previous records, and comparisons of the floras with others. The fourteen plates are very well reproduced. The greater part of the plants described are of Rhætic and Jurassic ages, and the well-known, world-wide genera preponderate.

A particularly interesting record is from the Neocomian, of several dicotyledonous leaves clearly showing their venation, which are described as *Artocarpidium Arberi*, sp. nov., by Dr.

Laurent, whose note on the subject is included in the memoir.

These signs that the interesting floras of the Southern Hemisphere are at last being described in a modern manner, and rendered available for comparison with the other contemporary floras, will be welcomed by Palæobotanists all the world over.

Wilson, in the *Geological Survey Divisional Report for* 1916, *Ottawa*, gave lists of a number of Mesozoic and other plants added to the Museum. The collections were mostly identified by Knowlton. Berry had a short paper on the Mesozoic flora of the Atlantic coastal plain (*Bull. Torrey Bot. Club*, vol. xliv.). Berry also published four or five small papers on the TERTIARY ; in *Torreya*, vol. xvii., he showed that the Pleistocene plants in Maine afforded evidence that the climate was then not much different from what it is at present.

Family Histories and Anatomy.—Last year under this heading there was something to say about almost all of the leading groups of plants, but this year there are only three or four papers to be mentioned, so that the headings will not be maintained.

Algæ are represented by M. F. Romanes' " Note on an Algal Limestone from Angola " (*Trans. Roy. Soc. Edin.* vol. li. p. 581), in which are described sections of an Albanian limestone from Angola. The rock showed, when weathered, warty knobs about 1 cm. in diameter, which make up the greater part of the rock, and are cemented together by a fine calcareous paste. The alga forming these knobs had a concentric form of growth round some such nucleus as an echinoid plate, or part of a spine. Two genera were identified, the more abundant being *Girvanella* Nich., the other a species of *Lithothamnion*, Phil. Two new specific names, *G. Minima* and *L. Angolense*, were proposed, but without exact diagnoses.

The Pteridosperms are represented by a detailed descriptive memoir on "The Heterangiums of the British Coal Measures " by Scott (*Linn. Soc. Journ. Bot.* vol. xliv.). In this *H. Shorense*, sp. nov., *H. minimum*, sp. nov., were described in detail from micro-sections, and Williamson's original species were also described and critically compared. Dr. Scott concluded that the various species can be grouped into two subgenera, one including the first of the new species, together with Williamson's *H. tiliæoides* and *H. Lomaxii*, while the

other new species will fall under Eu-heterangium. This interesting genus has long wanted revision, and this memoir affords the chief contribution of the year to what may be described as Anatomical Palæobotany.

The Bennettitales are represented by a small paper by Stopes (*Ann. Bot.* vol. xxxi.) in which some little rootlets with remarkably pretty root-hairs are described as having been so associated with *Bennettites* as to leave little doubt that they belong to it.

The Gymnosperms are represented by two anatomical studies of fossil wood, both in the *Annals of Botany*, vol. xxxi. One by the late Miss Holden on Palæozoic stems from India, in which she described two new species, *Dadoxylon indicum* and *D. Bengalense.* The first specimen was unusual in showing the pitting of medullary ray cells, and it was otherwise very well preserved. Yasui described a Tertiary *Sequoia* from Japan.

This year I feel even more uncertain than usual that I have been able to ascertain the whole year's harvest, as the delays owing to the war have been quite exceptional, and, as I hear from one of the most prolific of writers on the subject, several authors are not sending out their work at all.

ZOOLOGY. By PROF. CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., University of Manitoba, Winnipeg, Canada.

OWING to postal difficulties, certain journals for the period under notice have not been available, but it is hoped to fill up these gaps in the next review.

Protozoa.—A great deal of work has of necessity been done on the protozoan parasites infecting the troops, and in "Human Intestinal Protozoa in the Near East," Pt. 5, by Wenyon and O'Connor (Wellcome Bureau of Scientific Research, 1918), we have a report of investigations carried out in and around Alexandria. The occurrence and spread of intestinal infections of protozoa in British troops and natives has been studied, attention being paid to the carrier question. Three new protozoa from the human intestine are described and also suggestions made for the diagnosis and treatment of amœboid dysentery.

Invertebrata.—Several interesting experimental studies have

been carried out in this group of the animal kingdom. In "The Regeneration of Triangular Pieces of *Planaria maculata*, a Study in Polarity" (*Journ. Exp. Zool.* vol. xxv. Feb. 1918), Olmsted describes a series of experiments made on the regenerative power of this flat-worm. The animals were cut in various ways, and under certain conditions it was found possible to bring about the regeneration of a new head at right angles to the former median axis. Except under these fairly limited conditions there is undoubtedly a marked tendency for the original polarity to be retained. Copeland, in his experiments on "The Olfactory Reactions and Organs of the Marine Snails *Alectrion obsoleta* (Say.) and *Busycon canaliculatum* (Linn.)" (*ibid.*), found that the olfactory organ in *Alectrion* was undoubtedly situated within the mantle cavity. To complete the inquiry, however, a larger species was investigated, and it was found that if the osphradium was destroyed the sense of smell was also eliminated and did not return until, at any rate, a partial regeneration of that organ had occurred. The siphon which is moved from side to side during progression introduces the olfactory stimulus into the mantle cavity and so enables the animal to discern on which side the cause of the smell lies, and so, if it be food, the animal can move towards it. It was found possible, by taking advantage of this, to lead the snail about the aquarium. "The reactions to light and gravity in *Drosophila* and its mutants" have been investigated by McEwen (*ibid.*). The author finds that the young female is slightly more positively phototrophic than the young male, but after nine days this difference has practically disappeared, and also interestingly enough that certain mutants, whose wings are absent or defective, show no phototropism in the same way as normal flies whose wings have been removed. Lastly, this indifference to light appears as a sex-limited character in the nutant known as "tan."

The somewhat striking results of Martini, who reported that in *Hydatina*, a Rotifer, the numbers of cells in the various organs were constant, and so the total number in the whole animal was also constant, have been reinvestigated by Shull in "Cell Inconstancy in *Hydatina senta*" (*Journ. Morph.* vol. xxx. March 1918). It was found that there is a slight but not very marked variation in the number of cells in each organ, and also in the number of nuclei in the syncytial yolk gland.

The original finding of Martini, therefore, although not absolutely true, is still approximately so.

The living and fixed eggs of *Strongylocentrotus* were utilised by Painter in "Contributions to the Study of Cell Mechanics, ii. Monaster Eggs, iii. Narcotised Eggs" (*Journ. Exp. Zool.* vol. xxiv. Jan. 1918) to examine the changes in the protoplasm during cell-division. These activities have been noted by other authors who did not investigate their cause. As certain narcotics allow the division of the egg to take place without the formation of asters, these are excluded as causative agents, and it appears as if the nucleus alone is responsible since its activities always precede those of the cell protoplasm.

Other papers include: "Cushion Cells of the Pharynx of *Prorhynchus applanatus* Kennel" by Kepner and Scott (*Journ. Morph.* vol. xxx. March 1918); "The Eye of *Polycystis goettei* (Bresslau)," by Kepner and Lawrence (*ibid.*); "The Oögenesis and Early Embryology of *Ascaris canis* Werner" by Walton (*ibid.*); "Differential Susceptibility and Differential Inhibition in the Development of Polychete Annelids," by Child (*ibid.*); "Studies of Amitosis; its Physiological Relations in the Adipose Cells of Insects, and its Probable Significance," by Nakahara.

Vertebrata.—A fairly extensive study of the functional activities of an Ascidian has been made by Hecht and recorded by him in "The Physiology of *Ascidia atra* Lesueur, i. General Physiology, ii. Sensory Physiology" (*Journ. Exp. Zool.* vol. xxv. 1918). The movements are brought about by several sets of smooth muscles using the test as an exoskeleton. Food in the form of plankton is caught up by mucous and passed across the branchial wall into the œsophagus. The animal is sensitive to tactile stimuli, but only to light of very high intensity, and the ocelli do not appear to be photoreceptors. It is also sensitive to heat and chemical stimuli.

Other papers include: "On the Branchial Epithelium of Ammocœtes," by Wallin (*Anat. Rec.* vol. xiv. March 1918).

"The History of the Eye Muscles" has been worked out by Neal (*Journ. Morph.* vol. xxx. March 1918), who finds that the first myotome gives rise to the muscles innervated by the oculemotor nerve, the superior oblique is derived from the second myotome, while the external rectus has a double origin.

Its ventro-lateral portion is a derivative of the second myotome, and its dorsal-median part comes from the third myotome, a conclusion previously put forward by Bohrn in 1904.

Other papers include: "Metabolic Activity of the Nervous System, ii. The Partition of Non-Protein Nitrogen in the Brain of the Grey Snapper (*Neomœnis griseus*), and also the Brain Weight in Relation to the Body Length of this Fish," by Hatai (*Journ. Comp. Neur.* vol. xxix. February 1918); "A Seasonal Study of the Five-spined Stickleback," by Hess (*Anat. Rec.* vol. xiv. February 1918); "A Bibliography of Fishes," vol. ii. Authors' Titles L–Z, by Dean, enlarged and edited by Eastman (*Amer. Mus. Nat. Hist.* 1917); "The Results of Thyroid Removal in the Larvæ of *Rana pipiens*," by Allen (*Journ. Exp. Zool.* vol. xxiv. January 1918); "Some Experiments on Regeneration after Ex-Articulation in *Diemyctylus viridescens*," by Morrill (*ibid.* February); "The Effect of the Extirpation of the Thyroid upon the Thymus and the Pituitary Glands of *Rana pipiens*," by Rogers (*ibid.* January); "The Effects of Inanition upon the Development of the Germ Glands and the Germ Cells of Frog Larvæ," and "The Acceleration of Metamorphosis in Frog Larvæ by Thyroid Feeding and the Effects upon the Alimentary Tract and Sex Glands," both by Swingle (*ibid.*); "Effects of the Extirpation of the Thyroid Gland in *Rana pipiens*," by Terry (*ibid.*); "Studies on the Growth of Blood-vessels in the Tail of the Frog Larva—by Observation and Experiment on the Living Animal," by Clark (*Am. Journ. Anat.* vol. xxiii. January 1918); and "Pharyngeal Derivatives of *Amblystoma*," by Baldwin (*Journ. Morph.* vol. xxx. March 1918).

"The Formation and Structure of the Zona Pellucida in the Ovarian Eggs of Turtles," by Thing (*Am. Journ. Anat.* vol. xxiii. March 1918) includes an account of a structure whereby nutritive material can be conveyed from the maternal blood to the forming yolk. It is composed of a number of very fine filaments from the surrounding epithelial cells which penetrate the homogeneous substance of the zona and end in knob-like swellings on the yolk. The somewhat complex formation of the zona itself is also fully dealt with.

Other papers include: "Concerning the Renal Portal System in *Chrysomys marginata*," by Robinson (*Anat. Rec.* vol. xiv. January 1918).

In "The Corpus Luteum in the Ovary of the Domestic Fowl" (*Am. Journ. Anat.* vol. xxiii. January 1918) Pearl and Boring give an interesting account of this gland in a bird and state that it is derived from theca interna. It is normally connected with ovulation, and homologous with the similar structure in mammals. The same authors also discuss the external characters, behaviour, anatomy, and cytology of "Hermaphrodite Birds" (*Journ. Exp. Zool.* vol. xxv. February 1918). The eight specimens studied were all basally females, and it was noticed that the interstitial cells had no causal relation to the secondary sex characters, whereas the degree of external femaleness was precisely correlated with the amount of lutear cells or pigment; the sex or degree of activity of the gonad has no direct causal relation to the development of comb, spurs, and wattles.

Other papers include: "Vestigeal Gill Filaments in Chick Embryos, with a Note on Similar Structures in Reptiles," by Boyden (*Am. Journ. Anat.* vol. xxiii. January 1918); "The Effect of the Heart-beat upon the Development of the Vascular System in the Chick," by Chapman (*ibid.*).

Sugita continues his "Comparative Studies on the Growth of the Cerebral Cortex, 3. On the Size and Shape of the Cerebrum in the Norway Rat (*Mus norvegicus*), and a comparison of these with the corresponding characters in the Albino Rat. And 4. On the Thickness of the Cerebral Cortex in the Albino Rat" (*Journ. Comp. Neur.* vol. xxix. February 1918). At the same age the brain of the Norway rat is heavier than that of the albino, and it is also slightly longer, wider, and higher. Its volume is about 21 per cent. above that of the albino, but its specific gravity is less. The cerebral cortex is slightly thicker in the Norway, particularly in the region of the occiput, a condition probably correlated with the somewhat better development of the eye and the visual apparatus. The lower percentage of water in the albino is perhaps related to smaller amount of grey matter. An instructive analysis of the time relations between menstrual age, copulation age, ovulation age, and fertilisation age of human embryos is given by Mall in a note "On the Age of Human Embryos" (*Am. Journ. Anat.* vol. xxiii. March 1918). Apparently the average time between copulation and fertilisation is one day.

Other papers include: "Advantages of Sagittal Sections

of Pig Embryos for a Medical Embryology Course," by Allen (*Anat. Rec.* vol. xiv. March 1918); "Observations on the Shape of the Erythroplastid in the Wing of the Bat," by Arey (*ibid.* January); "Histology of the Sensory Root of the Trigeminal Nerve of the Rat *Mus norvegicus*," by Hoag (*ibid.* February); "Persistence of the Posterior Cardinal Veins in an Adult Cat," by Metcalf (*ibid.* January); "Determination of the Size of the Heart by Means of the X-rays," by Bardeen (*Am. Journ. Anat.* vol. xxiii. March 1918); "The Isolation, Shape, Size, and Number of the Lobules of the Pig's Liver," by Johnson (*ibid.*); "The Brachial Plexus of Nerves in Man, the Variations in its Formation and Branches," by Kerr (*ibid.*); "The Absence of Hernal Nodes in the Domestic Pig," and "Some Observations on Megacytes in Lymphatic Tissues," both by Meyer (*ibid.* May); and "The Position of the Insertion of the Pectoralis Major and Deltoid Muscles on the Humerus of Man," and "The Fontanella Metopica and its Remnants in an Adult Skull," both by Schultz (*ibid.* January and March).

General.—Hoskins, in a note on "Microscope Lamps for Students" (*Anat. Rec.* vol. xiv. January 1918) suggests, as a satisfactory lamp, a sixteen-candle-power frosted electric bulb dipped two or three times in an alcoholic solution of Bleu de Lyon. One such lamp will provide sufficient light for four students, and is quite inexpensive.

Other papers include: "Some Considerations Regarding Microscopical Technique," by McClung (*ibid.* May); "Is the Influence upon Development, Metamorphosis, and Growth of Thymus, when taken as Food, due to a Specific Action of that Gland?" by Uhlenhuth (*Journ. Exp. Zool.* vol. xxv. February); and "Chondriosomes in the Testicle-cells of *Fundulus*," by Duesberg (*Am. Journ. Anat.* vol. xxiii. January 1918).

ANTHROPOLOGY. By A. G. Thacker, A.R.C.Sc.

The nationalities of Europe have been much under discussion during the last four years; indeed, apart from the progress of the actual military operations, probably no other subject has been so prominently before the public. Yet a surprisingly small proportion of what has been written has been valuable or even accurate. A serious knowledge of European ethnography is still confined to a very narrow circle, and the ordinary jour-

nalist habitually makes the most grotesque errors. This is the more to be regretted because the subject, though much more complex than the average Englishman supposed before the war (he must now be utterly bewildered by the torrent of unfamiliar names), is not so difficult that an outline of the facts could not be made comprehensible in quite a brief explication. The chief complexities relate, of course, to Eastern and East-Central Europe. A most important and valuable contribution to this part of the subject has been published in the *Journal of the Royal Statistical Society*, vol. lxxxi. pt. 2, March 1918, in the shape of a paper by Geoffrey Drage entitled "Pre-war Statistics of Poland and Lithuania." The article (which is nearly 100 pages in length) is partly of sociological and partly of ethnographical interest; but it is, of course, only the latter aspect with which we are here concerned. It appears that the total Polish population of the world is about 26,000,000, of whom 23,000,000 still live in Europe, nearly all (except half a million who have migrated to Western Germany) dwelling of course in the sundered territories of historic Poland. It is important to note, however, that the Poles constitute less than 40 per cent. of the population of historic Poland. The Poles in their day were an imperial race, and they imposed their rule upon millions of Russians. Thus certain provinces of historic Poland became part of Russia proper, not merely of "Russian Poland," and Russian they ought to remain. Minsk and Volhynia have only 10 per cent. of Poles, Moghileff and Kieff have but 3 per cent. On the other hand, in the so-called Kingdom of Poland 75 per cent. of the population are Polish. The Poles are in a very large majority in Western Galicia. In Prussian Poland the Poles are a majority in Posen and Silesia, but not in the province of West Prussia. The paper also gives interesting and important data relating to the Lithuanians, Letts, White Russians, Ukrainians, and Jews. Mr. Drage says that the total Hebrew population of the world is slightly under 12,000,000, almost exactly half this number being Polish Jews. The Jews constitute about 15 per cent. of the population of the so-called Kingdom of Poland, and 10 per cent. of that of Galicia. Several maps are appended to the paper showing the distribution of the Poles.

Among the more interesting of recent contributions to *Man* are two fairly long articles on Witchcraft by Margaret A.

Murray. The first (in the April number) is entitled "Child-sacrifice among European Witches." The writer says that "in studying the cult of the witches, plain and irrefragable proof is found that the personage called by Christian writers 'the Devil,' was considered by the witches themselves to be God incarnate as man. To this deity they made sacrifices of various kinds, the most important of such sacrifices being that of a child. The child was either a witch's child or was unbaptized; in other words, it did not belong to the Christian Church." The writer gives some interesting evidence, on which she comments: "It is impossible to believe in any great frequency of this sacrifice, but there is considerable foundation in fact for the statement that children were killed, and it accounts as nothing else can for the cold-blooded murders of children of which the witches were sometimes accused." The second article (in June) is entitled "Divination by Witches' Familiars." Extraordinary evidence of supposed divination by these "imps," "familiars," or "puckerels" is given.

In the May number of *Man* there is a long article by Captain E. G. Fenton, R.A.M.C., on certain grooves to be seen in Malta which he believes to be cart-ruts dating from the early Middle Ages or even from Roman times. In the June number there is a reply from Prof. Boyd Dawkins in which he contends that these supposed cart-ruts are really only "joints" widened by the weathering of the calcareous rocks.

Another article, "A Linguistic Fragment from Western Kordofan," by Brenda Z. Seligman, in *Man* (for April) also calls for mention. The fragment in question is a short vocabulary obtained from an old Pygmy, the words belonging to a dialect which is apparently unknown.

To the June number of the same magazine Dr. Bronislaw Malinowski contributes a long article on "Fishing in the Trobriand Islands."

Few authors have worked more energetically than has Mr. Reid Moir to convince the world of the reality of pre-Palæolithic implements. He has again set forth his views in a paper entitled "The Ancient Flint Implements of Suffolk," which will be found in the *Proceedings of the Suffolk Institute of Archæology and Natural History*, vol. xvi. pt. 2.

Those interested in the religions of savages may be referred to a new essay by Dr. E. S. Hartland entitled "Religion among

the Indians of Guiana," which appears in the *Rationalist Press Association Annual* for 1918.

PALÆONTOLOGY. By W. P. PYCRAFT, F.Z.S., A.L.S., F.R.A.I., British Museum (Natural History), South Kensington, London.

HAVING regard to the times, surprise can scarcely be felt at the fact that there is little to record in regard to fossil animals for the first six months of 1918.

Dr. Branislaw Petronievics (*Ann. and Mag. Nat. Hist.* pp. 67–69, 1918) gives a brief account of the results of a re-examination of the lower jaw of that primitive and remarkable mammal *Stereognathus ooliticus*, preserved in the Museum of Practical Geology. Marsh, years ago, contended that this fragment represented, not the lower, but the upper jaw. Dr. Petronievics received permission to convey the specimen to the British Museum of Natural History, for the purpose of removing more of the matrix, and, as a result, he has been enabled to show that Owen was right in his determination of the nature of this jaw, but inaccurate in regard to his description of the teeth. The oblique position of the cusps of these, he believes, is due to the motion of the lower jaw from behind forwards, a movement which he mistakenly supposes to be opposite to that which obtains among rodents. In an earlier paper which appeared in this Magazine (pp. 284–9, 1917) too late for mention in my last summary, the author describes anew the skull of that remarkable mammal *Tritylodon longævus*, originally described by Owen. Many new features of importance are established as a consequence of this re-examination. He disposes of the view which has been held that this skull is that of a theromorphous reptile. Its mammalian characters are, he insists, beyond dispute; but it is to be regarded as the most primitive of known mammals; and furthermore affording direct proof that the mammals have their origin in Reptiles, most probably in Theriodont Reptiles.

Mr. Lawrence M. Lambe (*Canada Dep. of Mines, Geol. Survey*, No. 53, Geol. Series, pp. 2–84, 1917) discusses the Cretaceous Theropodous Dinosaur *Gorgosaurus*. The skeleton of this remarkable creature is described with great care, and the memoir concludes with a discussion on the supposed appearance and habits of the animal during life. That it was

a bipedal carnivore seems certain. The author suggests that the remarkably small and feeble forelimbs were of assistance in minor changes of position, when prone; or when rising to a sitting posture; and also for receiving the weight of the fore part of the body when bringing its full length to the ground. In these conjectures he may be right. But we hesitate to adopt his suggestion that it was, in part at any rate, a carrion eater.

Prof. Herman Douthitt (*Contributions from the Walker Museum*, vol. ii. No. 14–41, 1917) describes at length the structure of the curious amphibian known under the name of *Diplocaulus*. Much already has been written on this theme, but the author has had the advantage of studying far more perfect material than has yet fallen to the lot of any other investigator. As a result, he has been able to set at rest many doubtful points in regard to the morphology of the skull, and has also added materially to our knowledge of the rest of the skeleton.

Prof. Samuel W. Williston (*Contributions from the Walker Museum*, vol. ii. No. 4, 1918) reviews the present state of our knowledge of the evolution of the reptilian vertebra, and in a second contribution to the same number discusses the osteology of some American Permian vertebrates.

In the first-mentioned paper he confirms the views expounded years ago by Cope on this theme, adding thereto a number of new facts, and some most excellent text figures. In his second paper he adds materially to our knowledge of the osteology of the primitive amphibians *Eryops* and *Chenoprosopus*, thanks to the discovery of new material. He also brings to light a number of new facts in regard to the very remarkable theromorph reptiles *Naosaurus*, *Edaphosaurus*, and *Sphenacodon*. As he remarks, "no valid characteristics distinguishing *Edaphosaurus* from *Naosaurus* have yet been found in the skulls, or appendicular skeleton"; but, he now points out, there is a well-marked difference in the remarkably elongated neural spines of the vertebræ, which make these creatures among the most extraordinary that have ever lived. In *Naosaurus* these spines have their free ends broadly dilated and thickened, while in *Edaphosaurus* they are slender and pointed. Finally he describes a new genus and species of the *Diplocaulidæ*, under the name *Platyops parvus*, based on two specimens

found in the Craddock bone-bed near Seymour, Texas. One of the most striking features of these two skulls is the absence of the parasphenoid rostrum between the pterygoid openings. The author failed to find a pineal foramen, though this is described and figured in the memoir on *Diplocaulus* by Prof. Douthitt already alluded to in this review.

Mr. L. Hussakof (*Bull. Amer. Mus. Nat. Hist.* vol. xxvii. pp. 761–7, November 1917) describes two new species of fossil fishes, collected by the American Museum Congo Expedition. The first of these is a species of *Lepidotus* new to science (*L. congolensis*); based on fragments of the jaws and scales. It was obtained from the Mission of St. Gabriel, situated on the right bank of the Congo, a few miles above Stanleyville; a station not previously known as a locality for fossils. The second species was obtained from Landana, some sixty-five miles north of the mouth of the Congo. It is undoubtedly a ray, and is provisionally referred to the genus *Rhinoptera*. Unfortunately no more than a single tooth was obtained, and it would seem that it may possibly represent an immature specimen of *Myliobatis*, of which one species has already been recorded. The formation from which this was obtained is correlated, approximately, with the Montien stage of Northern France and Belgium.

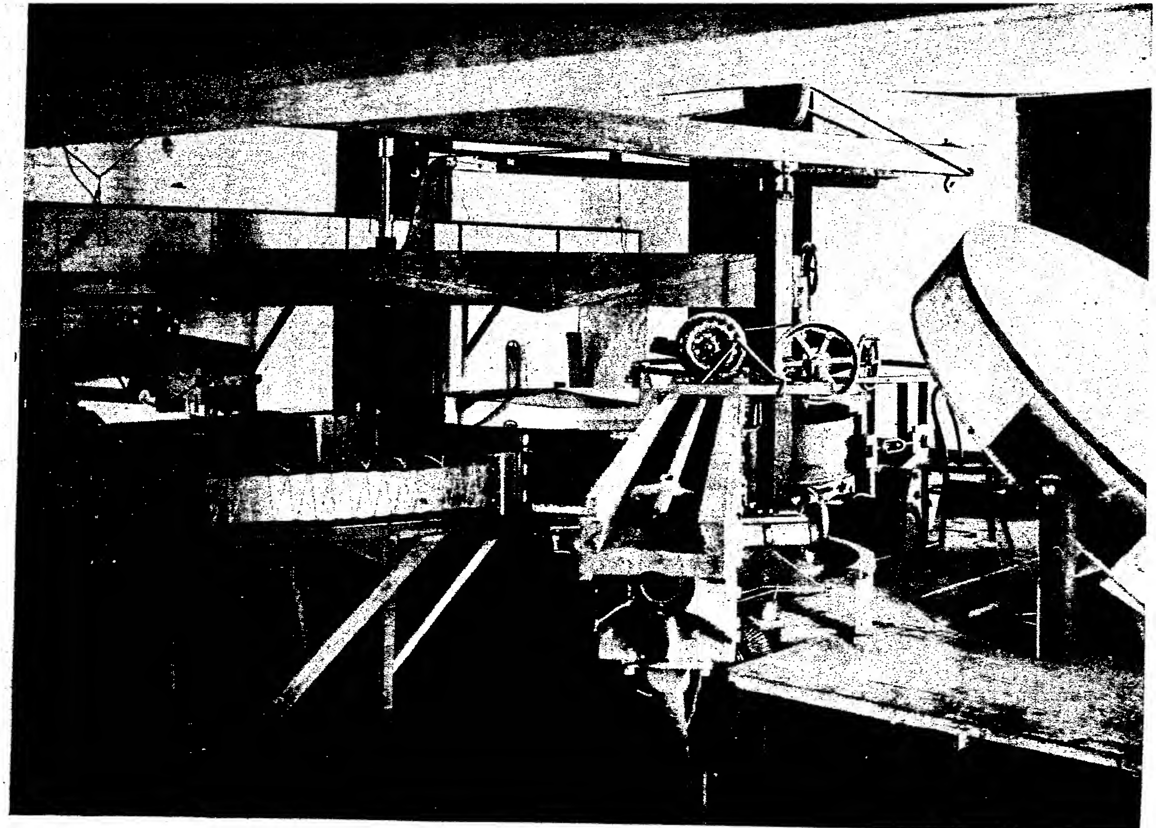

PLATE I.—The 100-inch mirror on the polishing machine.

ARTICLES

THE 100-INCH REFLECTING TELESCOPE OF THE MOUNT WILSON SOLAR OBSERVATORY

By H. SPENCER JONES, M.A., B.Sc.,
Chief Assistant, Royal Observatory, Greenwich

The completion of a great achievement in telescope construction is now being witnessed, the final adjustment to and preparatory trials of a reflecting telescope far exceeding in size any telescope that has previously been constructed being now in progress. A brief account of the design and construction of the telescope therefore appears desirable.

Appropriately enough, this telescope belongs to an American observatory, the Mount Wilson Solar Observatory, Pasadena, California. Though we may envy our American allies the fine instrument which they possess, we may console ourselves with the fact that the skies in this country would not be sufficiently good for such an instrument to be used with advantage. In order that the full aperture of so large a mirror may be advantageously utilised, the most perfect definition is needed. For some purposes, however, large light-gathering power rather than perfect seeing is required.

The favourable atmospheric conditions at Mount Wilson may be judged from the following table giving the number of clear hours and nights spent in observation during the year 1915–16, extracted from the last annual report of the Director.

Date.	Hours of darkness.	Hours clear.	Observations. All night.	Observations. Part night.	Observations. None.
1915, September	295	207	19	5	6
October	336	273	24	5	2
November	330	159	8	13	9
December	346	162	10	11	10
1916, January	346	47	1	8	22
February	308	155	11	6	12
March	324	199	15	8	8
April	286	220	22	6	2
May	266	223	23	6	2
June	230	219	24	2	4
July	255	241	29	2	0
August	269	236	24	6	1

It may in fact be stated that the amount of clear sky is more than can fully be utilised.

The construction of the 100-inch telescope was made possible by the generous offer of an American millionaire, Mr. John D. Hooker of Los Angeles, who presented to the Carnegie Institute of Washington in the year 1906 the sum of 45,000 dollars for the construction of the largest mirror that could be obtained for mounting in a reflecting telescope for the Solar Observatory, with the hope that the diameter of the mirror would reach 100 inches. The 5-foot telescope for the observatory was then nearing completion and the experience gained in the construction of this instrument gave promise that the practical difficulties might be successfully overcome. There were carefully considered by Prof. Hale, Director of the observatory. They were as follows:

(1) The manufacture of a suitable glass disc. For large mirrors, experience indicates that the thickness should be somewhere about one-seventh or one-eighth of the diameter in order that sufficient rigidity may be obtained and that it may be possible so to mount the mirror that the figure will not be distorted. Thinner discs are also more liable to suffer temporary disturbance of figure through change of temperature. A glass disc 100 inches in diameter and 13 inches thick weighs over 4½ tons, and the practical difficulties of casting so large a disc in one piece are enormous. The glass must be sufficiently homogeneous to be tolerably free from internal strains, which might cause it to fracture either during working or subsequently, particularly if sudden changes of temperature occurred. It must also be free from bubbles which, if present, might be exposed by the grinding.

There are very few glass works which would think of attempting the task. Some of the French makers seemed to have a special aptitude for the purpose and the St. Gobain Company, who had long experience and full understanding of the requirements, expressed their willingness to undertake it.

(2) The production of a perfect paraboloidal figure. These large mirrors are not, as might be expected, spherical in figure. The surface which possesses the property that a beam of parallel light falling upon it is reflected so that every ray passes accurately through a point is a paraboloid. A sphere does not possess this property, the defect being known as spherical

aberration. To parabolise a large mirror accurately is not an easy matter : the process is one of trial and error, frequent and careful optical tests being required to check the progress of the work. Mr. Ritchey of the Mount Wilson Observatory had succeeded with the 60-inch mirror, and with the experience so gained it seemed probable that the difficulties of working the still larger disc would be overcome.

(3) The design and construction of a mounting capable of carrying the mirror with the necessary accuracy. The total weight of the telescope would be very large, yet it must be well balanced and capable of being driven in a regular manner by clockwork.

(4) Changes of focal length due to temperature variations. Prof. Perrine recently stated that when figuring a 90-cm. flat for use in testing a $1\frac{1}{2}$ m. paraboloid at Cordoba Observatory, it was found that when the temperature rose as much as $\frac{1}{2}$°C. in from two to six hours the flat became sensibly convex. Unless particular precautions are taken, changes of focus due to change in temperature in large reflectors occur which may interfere greatly with the quality of the photographs. At Mount Wilson, there is the advantage that during the observing season the night temperature is nearly constant after 9 p.m., and it was thought that during the daytime it would be possible to maintain the mirrors at the average night temperature by means of a refrigerating plant.

As it appeared therefore that all the difficulties could be overcome the 4·5 ton disc was ordered from the French Plate Glass Company of St. Gobain, France, and the erection of buildings for grinding, figuring, polishing, and testing the mirror were commenced. Funds for the mounting, dome, and building were still required, but it was thought that they would be forthcoming.

Careful designing of the buildings for working the mirror was necessary. These consisted of a large room 34 feet square by 20 feet high opening into a long testing hall, 100 feet long and 10 feet wide, to enable the mirror to be tested at its centre of curvature or with parallel light. A constant and uniform temperature is necessary for the testing, so that the room had to be heated by an automatic heater. The greatest precautions were taken to avoid damage from scratches during polishing : double windows were provided, sealed to prevent the admission

of dust; the air entering the room was filtered; the walls and ceiling were coated with shellac; canvas was suspended above the grinding machine, and the floor was kept wet during polishing. It was also necessary that a 60-inch plane mirror for use in testing the paraboloidal surface of the 100-inch mirror should be constructed.

The problem of casting the disc was carefully considered by the St. Gobain Company. One of the chief difficulties was how to anneal thoroughly so large a slab of glass. If the disc were allowed to cool too suddenly or unevenly, internal strains would be set up which, even though they might not cause the slab to fracture, would almost certainly produce irregular changes of figure of the completed mirror with change of temperature. It was necessary after casting the slab for it to be raised to such a temperature that internal strains would be relieved and then to be allowed to cool extremely slowly and uniformly.

After repeated trials first on smaller discs and then on large ones, a disc was obtained in 1908, which the glass company considered satisfactory and which was therefore forwarded to Pasadena. On arrival there, it was found that there were numerous flaws within the disc and it was immediately rejected. The company expressed their willingness to bear the loss and to construct another disc. In consultation with Prof. Ritchey a new glass furnace, as well as a new annealing oven and accessories, were constructed; the new furnace and melting-pot were capable of holding 20 tons of material, and improved methods of annealing were introduced. Meanwhile, a perfect 60-inch disc for the plane mirror for testing had been made. The further attempts to produce a 100-inch disc were destined to failure. In 1910 a successful disc was cast, but it broke during annealing. The same fate happened to a further disc in 1911. It was therefore decided in the autumn of 1910 to commence work upon the disc already received and temporarily laid aside. The glass company agreed to this being done, payment for the disc being conditional upon its proving suitable for use as an astronomical mirror. Examination of the disc showed that whilst there were several sheets of air-bubbles running through it, they did not approach the surface sufficiently closely to interfere with a perfect paraboloidal figure. It appeared probable that the internal strains would not be sufficiently harmful and the process of

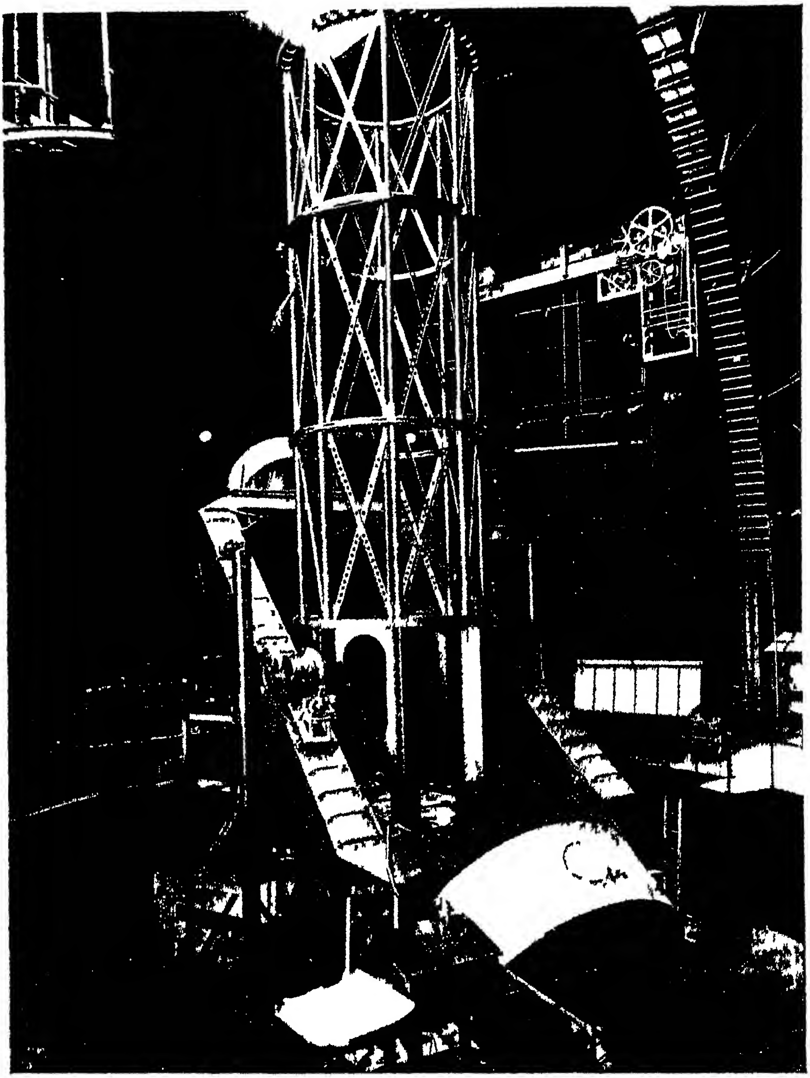

Plate II.—General view of telescope tube and mounting.

fine grinding of the spherical surface was commenced. The surface was then polished in order that tests might be made as to the character of the figure at different temperatures. Meanwhile, further attempts were made at St. Gobain to get a perfect disc: a flawless disc of 100 inches aperture and 8 inches thickness was produced, but this was not considered sufficiently thick. The progress of the work on the first disc fortunately soon indicated that it would be suitable for its purpose.

The working of the mirror was in the very capable hands of Prof. G. W. Ritchey, who had had considerable experience in figuring and testing large mirrors at the Yerkes Observatory, and who had also successfully worked the Mount Wilson 60-inch mirror. Details of the various processes in the construction of a large mirror and of the precautions needed at each stage have been lucidly given by Prof. Ritchey in his paper "On the modern reflecting telescope and the making and testing of optical mirrors" (*Smithsonian Contributions*, vol. xxxiv.). After rough grinding and edging the mirror was ground to the spherical form with a radius of curvature slightly greater than twice the desired focal length of the finished paraboloid. The mirror was then polished, frequent tests being made during the process to ensure a perfect figure being obtained free from zonal or irregular errors. The parabolising was then done by shortening the radii of curvature of the inner zones, using special polishing tools. The parabolising is a most difficult operation and calls for the utmost skill in working and testing. The delicacy of the operation can be realised from the fact that in the 100-inch mirror the depth of the finished paraboloid differs from that of the spherical surface to which it was brought previous to parabolising by almost exactly 0·001 inch at the centre, where the difference is a maximum. The work of changing from the spherical surface to a paraboloid therefore involves removing less than one-thousandth of an inch of glass from any part of the surface, yet it occupied over a year, the work being suspended for a time during the winter when the artificial heating needed for the optical shop made accurate tests impossible. Tests were made each morning after a day's figuring, special arrangements being provided for lifting and holding the mirror upright for and during testing.

Two methods of testing were employed: (1) the mirror was tested at the centre of curvature to determine the total

amount of parabolisation. This test consists in measuring the curvature of successive zones of the surface from the edge to the centre by a specially delicate optical method known as the "knife-edge" test and comparing the values obtained with the computed values. (2) The mirror was tested at its focus with the aid of the 60-inch plane mirror made specially for the purpose. With a perfect paraboloid a source of light placed in its focus will emit light which the mirror will reflect as a parallel beam of light. This will be reflected back along its path by a perfectly plane mirror, at right angles to the axis, and brought to a focus again. The deviation from a true focus gives an indication of the defects in figure of the mirror. This method of testing is invaluable for detecting and correcting slight zonal errors of surface. A combination of the two tests enables a degree of certainty to be obtained which cannot be obtained by either alone. Special care was necessary throughout the testing to avoid irregular changes of figure due to temperature variation.

When the parabolising had been completed, a photographic test was made by the well-known Hartmann method, as a check on the visual methods, and to provide a permanent record of the surface. Outside the central 8-inch zone (which is covered by the projected area of the Newtonian and convex mirrors) the largest deviation of the observed from the theoretical focal lengh for any zone was 0·14 mm., or only one part in 92,000.

As an instance of the immense amount of work involved in completing this mirror, it may be mentioned that for testing the 60-inch flat, which it was necessary to construct for testing the 100-inch mirror, three spherical mirrors of 14 inches aperture and 16 feet radius, of 36 inches aperture and 50 feet radius, and of 27 inches aperture and 135 inches radius respectively, had to be constructed, besides numerous grinding and polishing tools.

The mirror as completed has an aperture of nearly 101 inches, a focal length of slightly over 42 feet, a thickness of 12·75 inches at the edge and 11·50 inches at the centre, and a weight of over 4 tons. Plate I. shows the mirror on the polishing machine.

The silvering was done without difficulty, the mirror being used to form its own bath. Thirty-two ounces of silver nitrate

and 150 gallons of distilled water were used. The mirror was finished during 1916.

Meanwhile progress had been made with the construction of the mounting and the dome. The provision for these was made possible in 1912 by a gift from Mr. Carnegie to the Carnegie Institute of Washington. Mr. Hooker, the donor of the mirror, had died in 1916, and so did not live to see the completion of the project which he had originated. It was decided that the mirror should be used in three ways, giving different equivalent focal lengths, viz. (1) in the principal focus, to avoid loss of light, possible distortion of the field, and exaggerated change of focal length due to changes in the figure or position of the auxiliary mirror : in this method the light is deflected to a focus at the side of the tube near its upper end by a small optically-worked plane mirror of elliptical shape, placed just inside the focus of the paraboloid and at an angle of 45° to the axis ; (2) in the Cassegrain form, the light being then reflected back from the upper end of the tube by a small convex mirror, perpendicular to the axis, and brought to a focus near the lower end : equivalent focal lengths up to 300 feet can thus be obtained; (3) in the coudé form, in which the cone of rays from a convex mirror at the upper end of the tube strikes a diagonal plane mirror placed at the intersection of the polar and declination axes and reflects the light in a constant direction, viz. towards the south pole of the heavens. This method has the great advantage that the focus remains in a constant position as the telescope is moved so that it is possible to use it for high-dispersion photography of stellar spectra with a long-focus spectrograph without the necessity of the spectrograph being attached to the telescope itself.

The dome and mounting were designed by Messrs. D. H. Burnham & Co. of Chicago, to particulars provided by Prof. Hale, and their construction was carried out by the Fore River Shipbuilding Works. The total mounting weighs 100 tons; the telescope tube itself is a skeleton tube, 40 feet long, built up in sections. It is well shown in Plate No. II., which represents the telescope as it was in October 1917, being then complete except for the upper mirror. The type of mounting is a modification of that designed by Prof. Ritchey for the 5-foot reflecting telescope of the Yerkes Observatory. The tube is supported by two massive trunnions fixed to a rectangular frame-

work, 32 feet by 16 feet, which constitutes the polar axis. The heavy lower end of the tube is very short, so that it can swing through this frame for motion in declination. This design gives a good support to the telescope tube and avoids the use of heavy counterpoises, as in the German type of mounting. The weight of the polar axis is taken by mercury flotation, the two mercury tanks being seen in Plate II. at the ends of the polar axis. This gives a very efficient anti-friction apparatus.

The foundations for the dome and telescope pier were commenced in 1913. The pier proper measures 20 feet by 45 feet at its base and is 33 feet high. The top is extended on all sides with a circular floor 52 feet in diameter, which is supported by heavy reinforced concrete brackets. From this floor a circular wall rises to the level of the main floor of the steel building. A special extension pier was added with a top sloping at an angle equal to the latitude of the observatory, to carry the spectrograph, etc., when the telescope is used in the coudé form. The dome is a handsome structure with a diameter of about 100 feet. The height of its top is 105 feet. To prevent as much as possible variations of temperature inside, the dome has a double roof of sheet iron with an intervening air space of about 2 feet. Louvres are arranged at the top of the dome to allow the escape of heated air. The shutter is 20 feet wide and very massive. It is divided vertically and consists of two portions moving on horizontal tracks at the top and bottom. Along the faces of the two sections are cushions to render them air tight. The dome is shown in Plate III., with the shutter open. This plate gives an excellent idea of the beautiful situation of the observatory. This is not without its disadvantages, and when the heavy parts of the mounting had been constructed it was necessary first to widen the mountain road up from Pasadena before they could be brought to the observatory. Particularly delicate was the operation of bringing the finished mirror from the Pasadena workshop to the mountain-top. It was packed in a special manner, and supported by springs to prevent any possibility of its fracturing through jolting.

The mounting of the mirror is an important matter, as with an improper mounting distortion of figure would result. The mirror is supported at its back surface by a number of weighted

PLATE III.—Exterior of the dome of the 100-inch telescope.

levers with flat plates at one end which support the mirror evenly over its surface and counterbalance its weight at all elevations. The edge support consists of a counterpoising ring, bearing along the neutral plane of the mirror, the ring being pressed against the mirror by a number of short weighted levers. This type of support was designed by Prof. Ritchey for the large reflector at the Yerkes Observatory and has proved extremely efficient. An elaborate water-cooling arrangement surrounds the mirror to maintain it at a constant temperature.

The rotation of the dome, the opening and closing of the shutters, and the setting of the telescope in right ascension and declination are all electrically controlled, so that the manipulation of the instrument in spite of its large size is very simple.

At the time of writing, regular observations have not been started with the instrument. Before these can be commenced there are many small adjustments to be made, and slight modifications of various parts will doubtless be found necessary. After these are completed the details of the performance of the telescope will be awaited with interest. It may be asked, Of what advantage is so large an instrument? Its main importance lies, of course, not only in its large light-gathering power, which will enable much fainter objects to be studied than would be possible even with the 60-inch telescope, but also in its large resolving power, which will enable very close objects to be separated. In photographic work, the impression on the plate is cumulative, and by giving very long exposures photographs of very faint objects may be secured. But there are limits to the possible length of exposure, for the background of the sky is never absolutely dark, and too long an exposure leads to fogging of the plate. No increase of exposure time will, however, increase the resolving power of the telescope, although it may enable fainter detail to be seen. The advantage of the great resolving power would be lost if the observing conditions were not extremely favourable, and in that respect the Mount Wilson Observatory is very fortunately situated, so that there is every reason to believe that it will be possible to utilise to the full the resolving power of the instrument. The 100-inch telescope will be admirably adapted to the study of the spectra of faint stars, a direction in which an extension of our knowledge is badly needed; to the photography of faint nebulæ and other objects of interest; to follow the variations in

brightness of faint novæ and other faint variable stars; to the study of stellar clusters, and to many similar purposes.

Astronomers have by now obtained an extensive and comparatively detailed knowledge of the region of the heavens in the immediate neighbourhood of the sun. The distribution of the stars, their motions in and across the line of sight, their spectral types, intrinsic brightnesses, and in some cases their masses are known with tolerable accuracy. A considerable amount of statistical information as to the region at a somewhat greater distance from the sun has also been accumulated. With the new telescope, the astronomer will be able to probe yet further into space, and the information which will be gathered will no doubt have a profound influence on theories of the structure of the Universe. This probing process with the 60-inch at Mount Wilson has recently led to the formulation of certain conclusions which are at considerable variance with what had been generally accepted as to the structure of the stellar universe, based on the study of our more immediate neighbours. We congratulate Prof. Hale and his staff on the possession of so fine an instrument of research, the product of years of thought and work, and on having such good observing conditions that they can use it with success.

We are indebted to Prof. Hale for his kindness in forwarding the photographs from which the plates which accompany this article are reproduced.

SOME ASPECTS OF ANIMAL COLOURATION FROM THE POINT OF VIEW OF COLOUR VISION

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PART II.[1]

The Consideration of Blue

With the exception of green, blue is the colour least often found on these insects' wings (*vide* diagram No. 1). Blue and violet are here dealt with together, because the vast majority of the blue tend towards violet rather than green, and because violet was found to have a distribution exactly similar to that of blue. Referring to diagram No. 2, it can be seen that blue differs from all the other colours in that it is much less often found in small than in large or moderate areas. In the five groups considered separately the distribution is: large areas, fifteen times; moderate areas, seventeen times; small areas, twenty-three times; giving a proportion more in favour of the small areas than is the case in diagrams Nos. 1 and 2: this difference is due to the inclusion in diagrams Nos. 1 and 2 of the Lycænidæ, where large areas of blue greatly predominate. The following table (No. 3) gives the arrangements of pattern which are associated with the fifteen instances of large blue areas. It is seen that in every case a very conspicuous type of pattern is found, namely, a central blue area outlined by a band of black or deep-toned brown. In the Lycænidæ this type of pattern is also very common.

[1] For Part I. see July number, 1918, Vol. XIII., No. 49 (Diagrams 1 to 7).

TABLE NO. 3

1.	SATYRINÆ.	Cœlites adamsoni	blue wings with marginal band of brown.
2.	NYMPHALINÆ.	Prothoë regalis	broad blue bar across forewing.
3–8.	"	Kallima (6 species)	central blue, with brown marginal band.
9.	PAPILIONIDÆ.	Achillides tamilana	large blue patch on dark green hindwing.
10.	EUPLOEINÆ.	Penoa deione	whole of wing blue.
11.	"	Nemana camaralzeman	forewing blue or violet outlined with a band of black or dark brown.
12.	"	N. modesta	
13.	"	Penoa limborgii	
14.	"	Salpinx leucogonys	
15.	"	Stictoplœa harrisii	

Habits of the insects as given in *Lepidoptera Indica*: (1) a shade-loving insect; (2) settles on trunks of trees with wings closed, no note of flying habits; (3-8) rocky nullahs, flies over tops of trees, settles with wings closed in trees or on trunks; (9) flies in a regular beat round and round in the hottest part of the day; (10-15) no note of habits, except in 14, which frequents sunny sandy beds of streams and open places in jungle.

Blue in moderate areas is found as a bar or patch lying on a very dark-toned background of either black, brown, or green (see table No. 4), an arrangement of pattern which probably results in a conspicuous appearance. It may be mentioned that out of the thirty-two occasions on which blue is used in large and moderate quantity, eighteen are found in the Papilioninæ and the Euploeinæ.

TABLE NO. 4

1.	EUPLOEINÆ.	Danisepa ramsayi	a blue bar across the forewing.
2.	SATYRINÆ.	Panantirrhœa marshalli	a violet bar.
3.	NYMPHALINÆ.	Labranga duda	a blue bar across both wings.
4.	"	L. durga	a blue bar across both wings.
5.	"	Cynitia andersoni	a blue bar outlining the wings.
6.	"	Kaniska canacea	a blue bar across both wings.
7.	"	K. haronica	a blue bar across both wings.
8.	PAPILIONINÆ.	Iliades polymnestor	an irregular blue bar across both wings.
9.	"	I. parinda	an irregular blue bar across both wings.
10.	"	Sabaria polyctor	a blue band across the hindwing.
11.	"	S. ganesa	a blue band across the hindwing.
12.	"	S. significans	a blue patch on the hindwing.
13.	"	S. triumphator	a blue patch on the hindwing.
14.	"	Achillides paris	a blue patch on the hindwing.
15.	"	Harimala crino	a blue bar across the hindwing.
16.	"	H. buddha	a blue bar across both wings.
17.	"	H. palinurus	a blue bar across both wings.

Habits of the insects as given in *Lepidoptera Indica*: Nos. (1-5), (11-13), and (15) no notes are given; Nos. (6) and (7) beds of streams and trunks of trees at rest; (8) wooded country, no other note; (9) in forest and open ground, quite fearless; (10) margins of rivers and streams; (14) river beds; (16) flies very high and fast; (17) bathes in the water of streams like a swallow.

There is thus little doubt that blue is commonly used in warning colouration, and from the point of view of colour vision it is clear why this should be the case Violet and red were almost certainly the first two colours to become visually perceptible; for this reason blue, and more especially violet-blue, would be used for conspicuous purposes, especially as the majority of animals may still have only trichromic vision.

If warning colouration is for short-distance visibility (see p. 78), then blue and especially violet-blue should be the colour for use at high illumination, because under this condition the maximum visual perception is at orange-yellow far away from blue-violet, and also because blue has small power of penetration and thus becomes at short distance dulled by the opacity of the atmosphere. It has been seen that for other reasons red should be the colour chiefly used at low (not nocturnal) illuminations for warning purposes: if these premises be good, then forest insects should present often red, rarely blue; and insects living in the full sunlight often blue and seldom red. The same should hold with birds whose low-illuminated breasts should be coloured red, and high-illuminated backs more often blue. It has already been shown that in the insects under consideration, red is chiefly found in forest species (red is very much more commonly found in the nocturnal Lepidoptera than is blue). It remains to analyse the habits of the blue insects given in Tables 3 and 4. This information is given in footnotes to these tables, where it can be seen that the vast majority fly in open sunny situations and that there is a striking contrast between their habits and those of the shade-loving red insects (see p. 78). The fact that large blue areas are found in the Kallima butterflies (see table No. 3) requires explanation, because these insects do not belong to a protected group: in this case the blue appears to be used to produce what may be called the conjuring effect: the conjuror relies to a large extent for the delusion of his audience upon making them look for the wrong thing. This is exactly what a bright blue Kallima does when chased by an enemy: it suddenly settles with closed wings and thus instantly

changes from a bright blue creature into apparently a dead leaf; the enemy failing of course to realise that such a change has taken place, continues to look for a bright blue insect (Marshall, *Trans. Ent. Soc.* 1902, pp. 355, 363). This method of protection is very widely made use of and probably accounts for many of the brilliant colours which are concealed when animals are at rest but displayed during motion. There is also evidence that two colours of great contrast are sometimes used in this manner to heighten the effect: for instance, from one aspect red is shown, then with a sudden change of position blue is displayed, and finally settling, the animal displays only protective colours. This must be contrasted with warning colouration, which instead of being concealed at the last moment is displayed either always or not until discovery by the enemy is certain.

It is, however, possible that this conjuring colouration, although it cannot be a warning against unpalatability, may nevertheless be warning or distinctive colouration against difficulty of capture, and as such may form a model for mimicry.

Blue in small areas remains to be dealt with: it occurs in the Nymphalinæ nine times and in the Papilionidæ fourteen times, in the Parnassiinæ as small eye-spots of blue on a white ground, in *Orpheides demoleus* and in several species of *Papilio* as small blue spots forming a marginal pattern to the hind-wing and associated with yellow. The occurrences of small blue areas in the Nymphalinæ are given in table No. 5:

TABLE NO. 5

1. Stibochiona nicea	black insect with blue marginal eye-spots.
2. Euthalia lubentina	small blue patch at margin of hindwing.
3. Euvanessa antiopa	small blue patch at the junction of the wings associated with yellow markings.
4. Junonia hierta 5. Eugonia xanthomelas 6. Aglais kaschmirensis 7. A. rizana 8. A. ladakensis	marginal row of blue spots associated with an orange and black centre.
9. Acidatia hyperbius	a marginal patch of blue, with black spots associated with orange markings.

In many of these cases the arrangement of blue, especially when forming a marginal pattern, gives rise to an inconspicuous appearance. It is remarkable that in many cases

the blue is placed close to the complemental colours orange and yellow. Complemental colours, in so far as they produce white or grey when blended at a distance, will render an object inconspicuous (see small white, p. 73), nevertheless at short range red, blue, and violet will be little dimmed by being close to the complemental green, orange, and yellow. It follows that as conspicuousness at long distance would seem to be seldom required, so red should be used to produce conspicuousness in animals having green backgrounds, and blue against brown backgrounds. On the other hand if there were no limit to the distance at which visibility is required, as for instance in flowers, then exactly the opposite would hold: red flowers should be found in the open where illumination is good, and blue amongst the foliage where the lighting is poor: this is found to be the case in British wild flowers (see table No. 6), and favours the view that the colour of flowers is related to the visual perception of insects and is not a warning colouration to herbivorous animals.

TABLE NO. 6

	Open.		Scrub.		Wood.	
R	43	5	3	2	0	0
RW	178	20·5	35	20	20	14
RB	94	10·5	24	14	18	12·5
B	39	4·5	14	8	14	10
W	177	20·5	46	26·5	40	28·5
Y	251	29	40	23	21	14·5
Gr.	82	9·5	10	6	28	20
Br.	5	0·5	1	0·5	1	0·5
Total	869	100	173	100	142	100

Compiled from *British Plants*, 11 volumes, J. T. Boswell Syme. R, red; RW, pale red and pink; RB, purple; B, blue; W, white; Y, yellow; Gr., green; Br., brown. First column gives the occurrences, the second the percentages.

This completes the examination in those cases in which the sexes do not present any difference in colour. Before passing to consider sexual dimorphism of colour, it will be convenient to tabulate the various ways in which colour has been seen to result in either a conspicuous or inconspicuous appearance (see table No. 7).

TABLE NO. 7

	Inconspicuous.	Conspicuous.
Brown	small and moderate areas: central large areas with a marginal patterns.	Large areas without a marginal pattern or outlined with a black band.
Black	small area in combination with small areas of white, yellow, orange, and brown.	large and moderate areas; and small areas forming a marginal band.
White	small areas, especially when combined with black or dark brown.	large and moderate areas, especially when outlined with a black band.
Yellow and orange .	small (and probably moderate) areas, large central areas with a marginal pattern.	large areas, especially when outlined with a black band.
Blue and violet .	small areas, especially when associated with small areas of yellow and orange.	large, moderate, and small areas.
Red .	probably small areas when associated with green.	small, moderate, and large areas.

By applying the above table to sexual differences, it will be possible to decide whether males are more conspicuous than females, or whether some other factor governs the sexual difference. In the Euploeinæ and Papilionidæ few sexual differences in colour occur, but in the Satyrinæ, Pierinæ and Nymphalinæ 144 species present this dimorphism (see diagrams Nos. 8, 9, 10 and 11).

The diagrams show the occurrences of colour in those species which present sexual dimorphism. Diagram No. 8 is the combination of diagrams Nos. 9, 10 and 11. The lettering is as in former diagrams.

If now table No. 7 be applied to this material, the following distributions result (see table No. 8), which show that with three exceptions sexual dimorphism in colour can be entirely defined as a difference in conspicuousness and that it does not appear to be necessary to seek any other factor. The exceptions alluded to occur in the distribution of small and moderate areas of orange and moderate areas of white (see stars in table

No. 8), and are accounted for by the fact that orange in the male is very commonly replaced by either white or yellow in

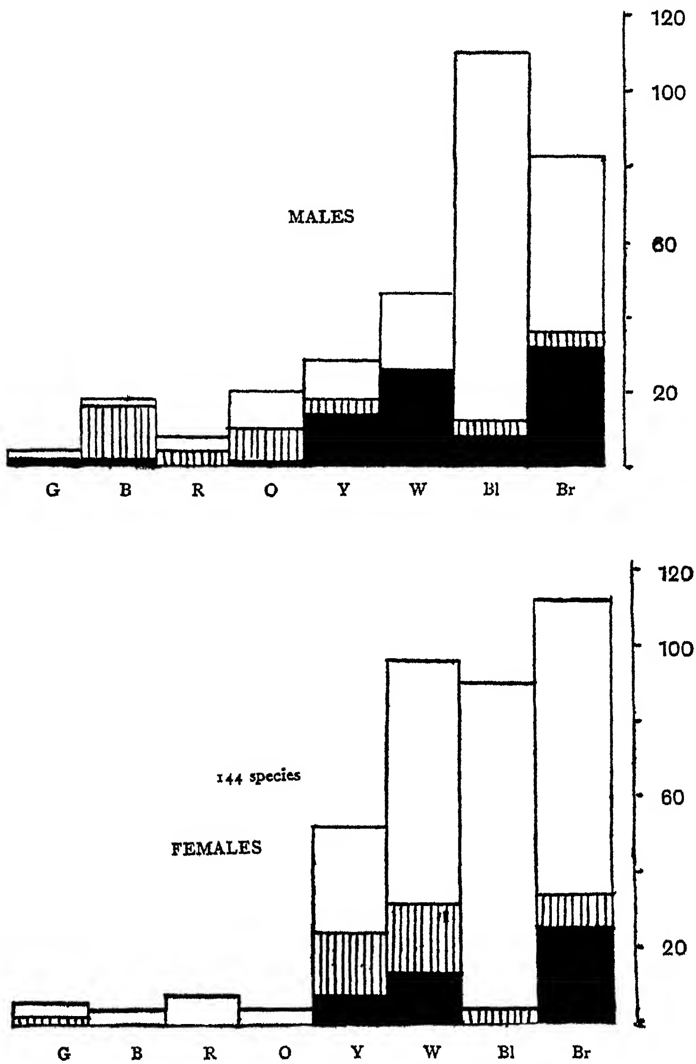


Diagram No. 8.

the female (see table No. 9). This replacement of orange and yellow by white will render the female more inconspicuous for reasons which have already been given; the replacement of

TABLE NO. 8

		Females.				Males.			
		P.	N.	S.	T.	P.	N.	S.	T.
Inconspicuous	Small areas of brown	34	41	4	79	19	27	0	46
	Moderate areas of brown	3	1	6	10	0	3	1	4
	Small areas of white	36	22	17	75	9	10	1	20
	Small areas of yellow	8	10	13	31	4	5	1	10
	Moderate areas of yellow	8	5	3	16	1	3	0	4
	Small areas of orange	1	3	0	4*	2	7	2	11*
	Moderate areas of orange	0	0	0	0*	2	3	3	8*
Conspicuous.	Large areas of black	0	0	0	0	0	8	1	9
	Moderate areas of black	0	5	0	5	0	5	0	5
	Large areas of white	14	0	1	15	26	0	0	26
	Moderate areas of white	9	9	0	18*	0	0	0	0*
	Large areas of blue	0	0	0	0	2	1	0	3
	Moderate areas of blue	0	0	0	0	4	10	1	15
	Moderate areas of red	0	0	0	0	4	0	0	4

P., Pierinæ; N., Nymphalinæ; S., Satyrinæ; T., Total.

TABLE NO. 9

Male.	Female.	Satyrinæ.	Nymphalinæ.	Total.
Orange	white	1	4	5
Orange	yellow	6	6	12
Yellow	white	1	4	5

TABLE NO. 10

	Nectariniidæ			Loriidæ.		
	Dorsal.	Lateral.	Ventral.	Dorsal.	Lateral.	Ventral.
Violet	50	34	28	3	—	—
Blue	43	36	34	35	26	17
Red	21	26	49	43	52	51

orange by yellow will have the same effect by removing the colour away from the conspicuous red end of the spectrum, so that these exceptions go to prove the rule.

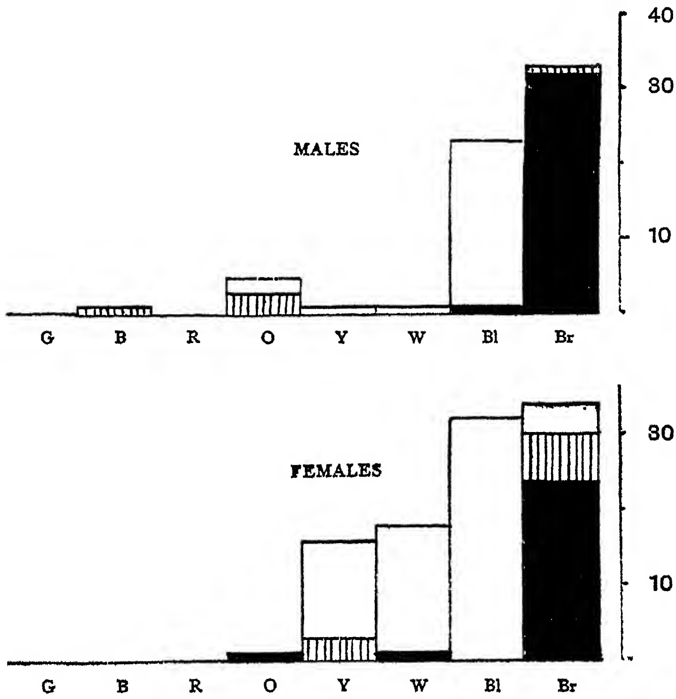
This completes the consideration of the Indian Lepidoptera. A short examination of birds will now be made, and will consist chiefly in testing certain predictions which have already been made.

When dealing with solid objects, it is necessary to take into account chiaroscuro, because colours will have quite different value according as to whether they are in high illumination on the back of the bird, or in low, on its ventral surface. It has already been predicted that for this reason one would expect

to find violet and blue laid down on the back more often than on the under parts, and *vice versa* for red. For purposes of classification the bird's body is divided into three areas according to the lighting, as shown in fig. 1.

Two families of birds were chosen for analysis, the Nectariniidæ (monograph Shelley, London, 1876 to 1880) and the Loriidæ (monograph Mivart, London, 1896).

Table No. 10 gives the occurrences of violet, blue, and red

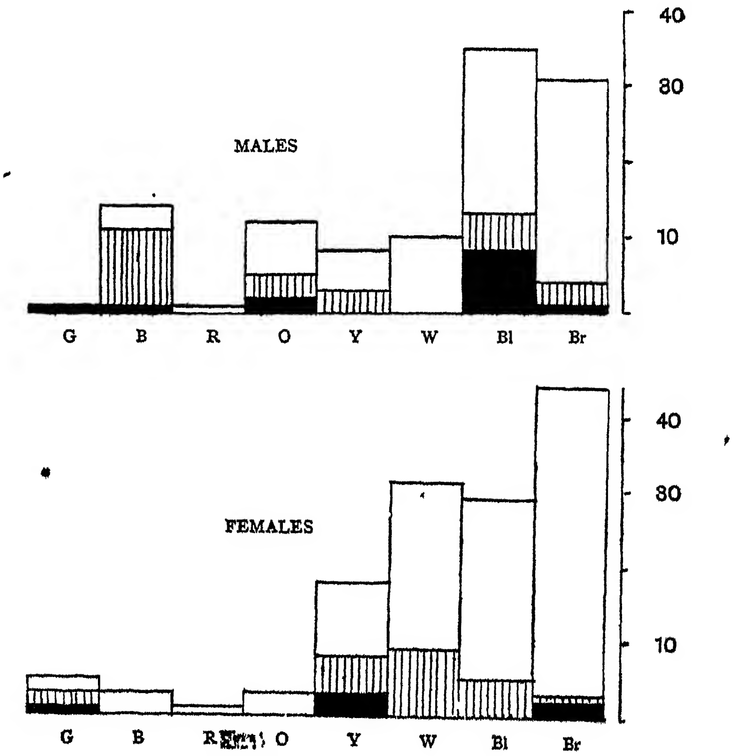


Satyrinæ (36)

Diagram No. 9.

in these areas. In the Nectariniidæ these colours occur only in the male; and the table shows that in them violet and blue occur more often on the dorsal than the lateral surfaces, and more often on the lateral than on the ventral; and that red has the opposite distribution. In the Loriidæ the sexes are alike, but the colour distribution is similar to that of the Nectariniidæ, with the exception that there is no difference between the lateral and ventral surfaces as regards the number of times

in which red occurs. It will be noted also that violet seldom occurs in these birds: this is undoubtedly due to the fact that the author, in description, has confused the colours violet and purple, using the term purple in very many cases to describe that which is obviously violet. This failure is common and is doubtless due to a want of appreciation of the colour violet owing to its rarity, and the great predominance of purple, in

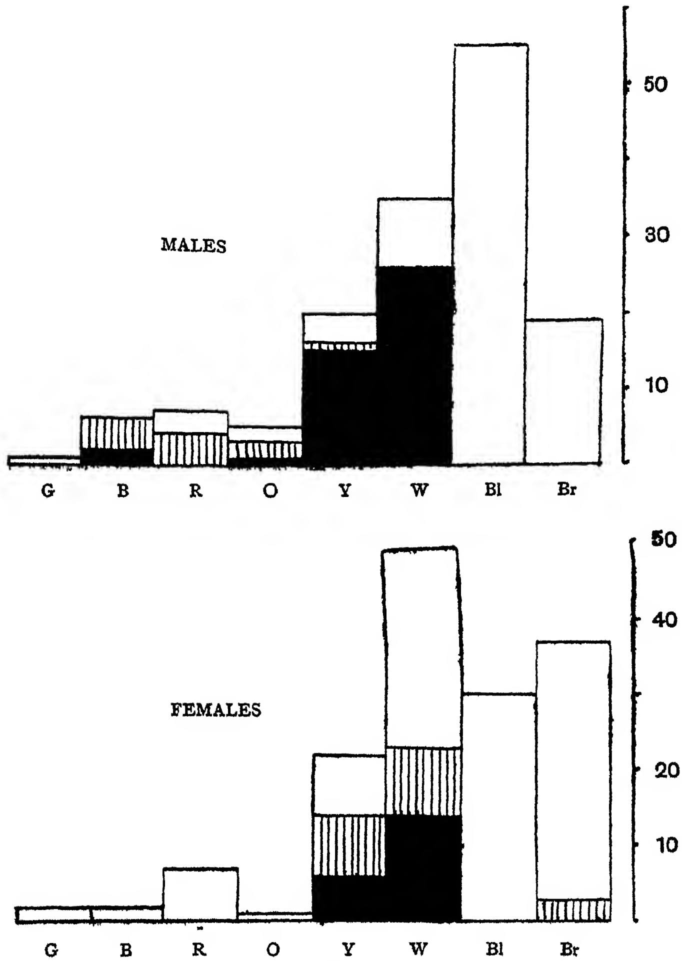


Nymphalinæ (48)

Diagram No. 10.

the arts. Thus the consideration of these birds fulfils the prediction. The Nectariniidæ also support the contention that yellow is not a conspicuous colour except by reason of its luminosity, for in a great many species the males have bright yellow at the throat and breast, with dorsal and lateral areas of red, blue, and violet; in females the red, blue, and violet areas are replaced by green, but the yellow at the throat and breast

is retained as in *Eudrepanis duyvenbodei* and *Anthreptis malaccensis*. On a few occasions yellow is used in a conspicuous manner in the male, and in these cases it is not retained in the female: as, for instance, in the male of *Ginnyris adelberti*,



Pierinæ (60)

Diagram No. 11.

where it forms a fan-shaped patch on the throat, surrounded by a large area of black. As further evidence that yellow is a relatively inconspicuous colour, it may be mentioned that in the Loriidæ red in the male or in the adults is occasionally

replaced by yellow in the female or young, as in *Eos fuscata* and *Charmosyna stellæ*.

The Nectariniidæ and Loriidæ were chosen, because in the first family there is great sexual dimorphism in colour, whereas in the Loriidæ the sexes are almost entirely alike. A comparison of the colour distribution in these two families confirms that which has already been shown to hold as regards sexual dimorphism in the Indian Lepidoptera, namely, that the differences between males and females are entirely a matter of differences in conspicuousness. In the Nectariniidæ, the differences between males and females appear to be exactly similar to the differences between the colouration of the Loriidæ and of an inconspicuous group of birds like the Warblers.

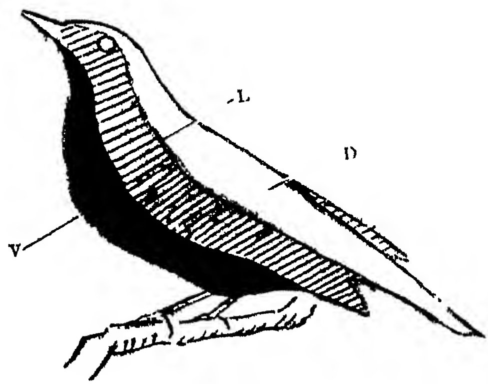


Fig. 1.
D., dorsal area; L., lateral area; V., ventral area.

The main features which have been brought out by this consideration of animal colouration are: that the enemies of insects have or have recently had only trichromic colour vision, as instanced by the use of brown against green backgrounds, and the use of yellow in association with inconspicuous arrangements of pattern; that in animals red is chiefly used to produce conspicuousness at low illuminations, blue at high illuminations, whereas in flowers the reverse occurs; that patterns, composed of complemental colours laid side by side, are inconspicuous at long range, that is beyond their blending distance, but very conspicuous at short range or within their blending distance; that sexual differences of colour in birds and insects when considered from the point of view of colour vision can be entirely accounted for on the basis of a difference in conspicuousness.

POPULAR SCIENCE

THE PRESERVATION OF GAME-BIRDS AND ITS RELATION TO AGRICULTURE

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The relationship existing between game-bird preservation and agriculture is a subject that presents numerous difficulties and many intricate controversial problems. Most, if not all, of these have little to do with the game-birds themselves, but are rather questions between landlord and tenant. Of such we disclaim any practical knowledge, but with the ways and the habits of those wild birds scheduled as game-birds, and their relationship to the land and the crops grown thereon, we claim a long and intimate acquaintanceship, and it would seem worth while, in view of recent developments, to once again carefully set forth what we believe to be the true economic position of these birds.

If they are injurious to the farmer, then the sooner the Game Act is repealed the better, for, after all, agriculture is much more important to the nation than the preservation of game-birds. On the other hand, if they are beneficial, *i.e.* the cultivation, breeding, and preservation of the birds themselves, as apart from any indirect benefits that accrue to certain members of the community from the fact that they offer sport to another section, the facts cannot be too widely known. Moreover, if, apart from their direct value, they offer a source of home-grown food, their value and importance is further enhanced.

That the various Game Laws are statutes of perfection we do not for a moment claim; on the other hand, the merest tyro interested in agriculture could off-hand suggest many much-desired improvements, but these and all such subsidiary subjects are foreign to the object we at present have in view,

viz. without prejudice, and upon bare facts, to fix more precisely the actual economic position such birds occupy.

Before placing the evidence before the reader, it will be well to carefully consider the charges that have been brought against the preservation of game-birds, and the replies that have been forthcoming. In so doing it is very important that we consider only such charges and replies as have been made by some responsible body of individuals, whom, we presume, have carefully considered the evidence from all sides, before framing their final opinion. We have therefore chosen the opinions expressed by two important bodies of men, both of which are directly concerned with the prosperity of agriculture and all appertaining to the land.

In the *Report of the Land Inquiry Committee* [1] we read, p. 255, " The preservation of game is among the reasons why the land is not at present producing its full yield." On p. 277, " Considered from the point of view of the national interests the damage done by game is too serious to be overlooked. . . . Moreover, our evidence shows that farmers are often hindered from putting their land to its best use by the presence of game. . . . Further, as already shown, there is evidence that not merely is land uncultivated, but large areas are altogether out of cultivation owing to the preservation of game. This land, instead of providing food for the people, provides sport and delicacies for the few, and is the source of much damage and annoyance to neighbouring farmers."

Now, if these charges are true, and not the outcome of political prejudice and partiality, then the matter urgently calls for immediate and drastic action; but before we can accept such sweeping charges it will be necessary to examine them in greater detail.

According to Lord Lovat,[2] " Grouse-shooting is, of all forms of sport, the most profitable to the general population. . . . It produces a maximum of profit to the wage-earner with the minimum of waste, an otherwise unproductive subject is converted into a source of profit."

Lord Lovat is a landlord, and no doubt enjoys grouse-shooting, so it may be said that his view is a prejudiced one.

[1] London: 1913, vol. i. Rural.

[2] *The Grouse in Health and Disease.* London, 1912.

Well, then, let us listen to the opinion of the land agents of the country, men who, under almost every varying condition, must have been brought into close and intimate touch with the question. "Where a moderate head of winged game is reared, it is an advantage to the land. Practically all farmers agree with this statement. Except where there are woodlands, partridges are the most important game-birds in this country, and the better the land is cultivated the more birds it will carry. The damage they do to crops is so small that it is more than counter-balanced by the good that they do to the land. Pheasants undoubtedly do more harm to crops, but they are often blamed for the misdeeds of sparrows, starlings, rooks, and pigeons. Like partridges, they are useful on the land. Those that are hand-reared are, of course, artificially fed. For the rest of their food, they feed, like domestic fowls, chiefly on insects and weed-seeds, whose destruction is a boon to the farmer."[1]

Here is an expression of opinion from a large body of men "who spend their lives in daily contact with all the practical problems of the agricultural industry," and who have the management of "thousands of acres in every county in England and Wales, as well as in many parts of Scotland," and they represent every shade of political opinion.

Again, from the same source we learn (p. 228): "The average landowner is not in a position to sacrifice income by withdrawing land from cultivation, if it is worth more to farm than it is for game. If land does not pay for cultivating, it is put to its best, if not its only, use by being devoted to game. Without game preservation, it would be practically derelict, and, as a means of affording employment and wages alone, apart from the production of game as food, it is better to use the land than to let it lie altogether idle."

With one voice, the charges above-mentioned are refuted, and the source from which that refutation comes will, we think, carry conviction.

We might dwell upon the value to agricultural communities of the profit that the preservation of game-birds provides, *i.e.* the indirect benefits in the form of "wages for temporary assistants, such as drivers, ghillies, pony-men, and dog-men, traffic in household supplies, sporting requisites, hiring, carting,

[1] *Facts about Land.* London, 1916, p. 223.

etc., and outlay on building, equipping, and maintaining shooting-lodges. These indirect profits cannot be very exactly stated, but a fair estimate of their amount may be obtained when it is stated that it is the combined experience of shooting tenants that for every £1 spent in rent, from 15s. to £1 is spent on other expenses connected with the undertaking."[1] But all these are, to the unbiased mind, so obvious that we will pass on to a consideration of the actual habits of the birds themselves.

The three commonest game-birds in this country are the pheasant, the red grouse, and the partridge, in addition to which we have the woodcock, capercaillie, quail, ptarmigan, and snipe. It is with reference to the first three species that the charges of damage are chiefly levelled, and particularly against the pheasant.

Careful examinations of the crops and stomach contents of a large series of adult and young birds, obtained from various parts of the country during the past six years, examination of and experimentation with the fæces, experiments on captive birds, and numerous field investigations, form the basis upon which the above-mentioned charges will be shown to be largely fictitious. It is not possible here to give detailed schedules of the food contents of the stomachs and crops of the individual specimens examined, but the summaries and figures at once indicate the results obtained.

The Pheasant

The pheasant is essentially an inhabitant of woods. Yarrell[2] writes: "Woods that are thick at the bottom, with long grass kept up by brambles and bushes, thick plantations, or marshy islands and moist grounds overgrown with rushes, reeds, or osiers are the favourite resorts of pheasants, in default of which they take to thick hedgerows, but can seldom be induced to remain long on any ground bare of shelter, however undisturbed. Wood and water are indispensable."

Considerable diversity of opinion exists as to whether the pheasant digs with its beak or scratches for its food, like the common fowl. Our experience is that it behaves very much like the fowl, first scratching away any surface material, and

[1] Lord Lovat in Introduction in *The Grouse in Health and Disease.*

[2] *Hist. Brit. Birds*, 1884, vol. iii. p. 98.

not digging like the rook. The average length of the pheasant's beak is 20–23 mm., whereas that of the rook is 52–53 mm. The measurements taken are from the tip of the beak to the commencement of the skin, irrespective of whether feathered or not.

It is now over five years since we set forth the results obtained from an examination of the stomach contents of one hundred and eighty-three birds,[1] and we need not here repeat them; but it is necessary to state that each subsequent year has confirmed these, so that in May 1917 we were able to summarise and definitely state the percentages of food that constitute this bird's food.[2] This is now set forth in detail for each month of the year (Table I), and diagrammatically summarised in figs. 1 and 2.

TABLE I.—SHOWING THE PERCENTAGES OF THE VARIOUS FOODS CONSUMED BY THE PHEASANT FROM JANUARY TO DECEMBER

Month.	Leaves, fruits, and seeds of weeds.	Grain.	Roots and stems.	Insects.			Earthworms.	Slugs.	Miscellaneous.	Total animal food.	Total vegetable food.
				Injurious.	Beneficial	Neutral.					
January	36·5	3·5	6·0	20·0	—	1·0	8·5	2·5	22·0	30·0	70·0
February	37·0	2·0	7·0	21·0	2·0	2·0	7·5	3·0	18·5	35·5	64·5
March	32·5	—	3·5	22·0	—	2·5	8·5	4·0	27·0	37·0	63·0
April	33·0	1·0	2·5	23·5	1·5	3·0	9·5	2·5	23·5	40 0	60·0
May	34·5	1·5	1·5	23·5	1·0	2·5	9·0	3·5	23·0	39·5	60·5
June	38·0	1·5	1·0	28 0	2·0	1·0	9·5	3·0	16·0	43·5	56·5
July	40·5	2·5	—	26·0	1·5	1·0	8·0	4·5	16·0	41·0	59 0
August	50·5	3·5	—	26·0	1·0	2·0	8·5	4·0	4·5	41·5	58·5
September	58·5	4·0	—	28·5	1·0	—	6·0	2·0	—	37·5	62·5
October	52·0	3·0	1·5	22·0	—	1·0	10·5	2·5	7·5	36 0	64·0
November	49 0	3·5	2·5	20·0	1·0	1·5	11·5	1·5	10 0	35·5	64·5
December	38·5	3·0	3·5	20·0	1·0	·5	8·0	1·0	24·5	30 5	69·5
Totals and averages	41·7	2 4	2·4	23·4	1·0	1·5	8·7	2·8	16·1	37·4	62·6

The particular items are worthy of some consideration. Firstly, we notice that the total vegetable food consumed in a year is 62·6 per cent. and the animal food 37·4 per cent. The total average percentage of grain is 2·4. In August it is 3·5 per cent., 4 in September, 3 in October, 3·5 in November,

[1] *Journ. Land Agents' Soc.* 1913, vol. xii. pp. 583–6.
[2] *Ibid.* 1917, vol. xvi. pp. 249–55.

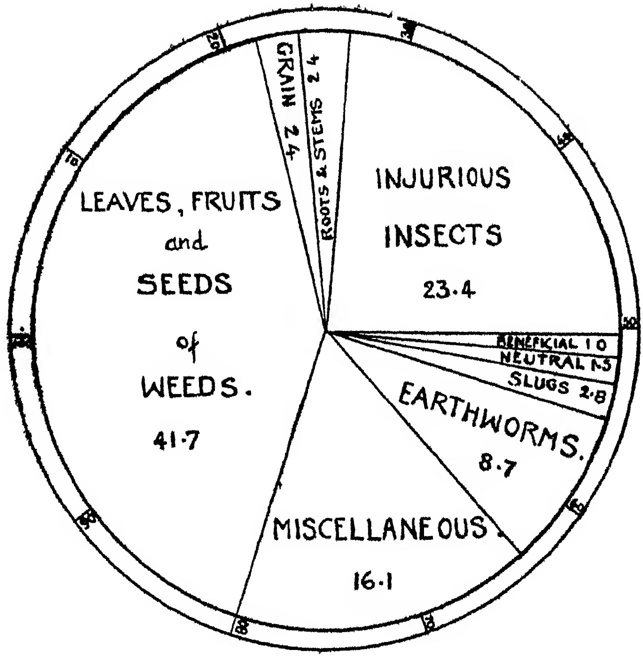

FIG. 1.—Diagram showing percentages of food of the pheasant.

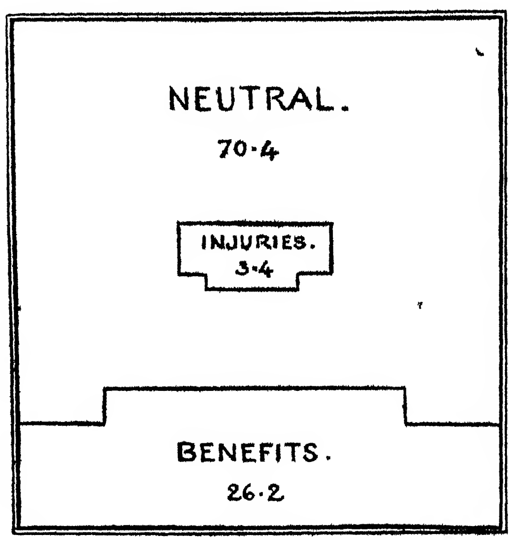

FIG. 2.—Diagram summarising injuries, benefits, etc., of the pheasant.

3 in December, and 3·5 in January. In no month is the percentage high, and seeing that 3·5, 3, and 3·5 per cent. were obtained in November, December, and January respectively, and the highest percentage is only 4 per cent. in September, the actual amount of grain stolen from the corn crop must be exceedingly small. Of the remaining vegetable matter the leaves, fruits, and seeds of weeds which constitute 41·7 per cent. need no comment, and the bulk of the roots and stems belong almost wholly to those of two weeds, viz. the Bulbous Crowfoot (*Ranunculus bulbosus*) and tubers of the Lesser Celandine (*R. ficaria*). There remains therefore the miscellaneous vegetable matter, the average total of which constitutes 16·1 per cent. of the total food. "Was this farm produce?" or "Did microscopical examination show any trace of mangolds, turnips, beet, or other crops?" During the whole of the six years we have been engaged upon this investigation we have failed to find any trace of such. The bulk of this miscellaneous vegetable matter was grass, and the remainder fragments of leaves, but of what plants it is impossible to say.

Now, supposing that we had no contra account to show on our balance-sheet, the above record scarcely warrants the assertion that the damage done by this species "is too serious to be overlooked," or that it is "the source of much damage."

No farmer will begrudge these birds their 44·1 per cent. of weeds, but we find that they also consume 23·4 per cent. of injurious insects (of which, in many parts of the country, wireworms and leather-jackets form a large proportion), and the beneficial and neutral species that they destroy are scarcely worthy of mention. Of earthworms, 8·7 per cent. are eaten, and 2·8 per cent. of slugs.

In face of this record, it is impossible for any one who is concerned only in arriving at the truth to condemn this bird. From the standpoint of the agriculturist it is one of the most beneficial species we have.

We do not for a moment contend that pheasants never do any damage; there must be plenty of isolated cases in which considerable damage has been done, but these are the exception, and must not be allowed to prejudice the mind against pheasants generally, as if such habits were natural to all birds of this species. We must try and look at the matter in a fair manner. The above average percentages are not obtained from isolated

cases, but from specimens collected from all parts of the British Isles during a period of six years.

There is a considerable amount of evidence to show that pheasants are frequently blamed for the misdeeds of other birds, particularly the wood-pigeon and the rook. Some little time ago our attention was drawn by a farmer to the damage that pheasants were doing to a clover crop by eating the young leaves. Close observation was kept upon the particular field for the better part of a day, but not a single pheasant was seen to visit it, although there were plenty in an adjoining wood, but wood-pigeons were seen in the field at all times of the day. One of these was shot, and upon being opened was found to be literally stuffed full of young clover-leaves. Cases of this kind are known to every keeper and all interested in game-birds. The very fact that they are preserved seems sufficient excuse to some people for blaming them for the misdeeds of all other feathered culprits.

It is a well-known fact, and has frequently been commented upon, that where many pheasants are reared there is usually an absence of wireworm. In this connection we may cite the statement of Mr. F. C. Gooch[1] of cases where this bird was found with 1,200 and 726 wireworms in their crops.

We do not propose to trouble the reader with the experience of other observers; it is sufficient to know that they agree very largely with our own, and, failing any other six years' record showing materially different percentages, we must refuse to believe that the pheasant is other than a most valuable bird, and that its relationship to agriculture is directly beneficial also. Where, however, an excessive number of birds are reared on a small acreage, in short, for shooting tenants who are mainly concerned in obtaining their money's worth, then damage is bound to ensue, for the conditions are altogether unnatural.

The Red Grouse

All who have paid any attention to the food of the Red Grouse are unanimously of opinion that heather (*Calluna vulgaris*) constitutes the bulk of its food, in addition to which the cranberry, blaeberry, cloudberry, creeping willow, sorrel, bog-myrtle rush, tormentil, etc., are eaten.

[1] *Morning Post*, January 17, 1917.

A summary of the food of this bird covering a period of five years shows that 77·5 per cent. of the total food consumed per year is vegetable matter, and 22·5 per cent. animal matter. Of the former, 73 per cent. consists of the leaves, fruits, and seeds of weeds, 1 per cent. of roots and stems, and 1·5 per cent. of grain, the remaining 2 per cent. being of a miscellaneous nature. Of the animal matter, 14 per cent. consists of injurious insects, 1 per cent. of beneficial insects, and 2·5 per cent. of neutral insects, 3 per cent. of earthworms, and 2 per cent. of slugs.

One might enlarge upon the very exhaustive report of the Grouse Disease Committee upon the nature of the food of this bird, but it is surely patent to every unbiased mind that, whatever faults may be laid to the charge of this species, none can be advanced against it so far as the nature of its food is concerned.

The Partridge

Any one conversant with the habits of the Partridge may be surprised to find that it has been accused of injuring crops. We have watched them during the day foraging in coveys along the hedge-sides, feeding upon various insects, insect larvæ, weeds and weed-seeds. On insects this species feeds to a much greater extent than the red grouse, and it is significant that districts where the largest corn crops are produced, and the land is best cultivated, frequently also produce the largest number of these birds.

Examination of the intestinal and stomach contents over a period of five years show that of the total bulk of food consumed 59·5 per cent. is vegetable matter and 40·5 per cent. animal matter. Of the former, 53·5 per cent. consists of leaves, fruits, and seeds of weeds, 3·5 per cent. of grain, and 2·5 per cent. of miscellaneous vegetable matter. Of the animal matter, 23 per cent. consists of injurious insects, 3 per cent. of beneficial species, 4 per cent. of neutral species, 6·5 per cent. of earthworms, and 4 per cent. of slugs complete the summary.

It will be noted, on comparing the totals for the red grouse and the partridge, that this latter bird consumes nearly twice as much animal food as the former, and that the grain content is also slightly higher; but, for the bulk of their food, both species depend upon the leaves, fruits, and seeds of weeds.

Species.	Leaves, fruits, and seeds of weeds.	Grain.	Roots and stems.	Insects.			Earthworms.	Slugs.	Miscellaneous.	Total animal food.	Total vegetable food.
				Injurious.	Beneficial.	Neutral.					
Red grouse . .	73·0	1·5	1·0	14·0	1·0	2·5	3·0	2·0	2·0	22·5	77·5
Partridge . .	53·5	3·5	—	23·0	3·0	4·0	6·5	4·0	2·5	40·5	59·5

Having now considered in some detail the nature of the food of the pheasant, the red grouse, and the partridge, it will be useful to summarise that of all three species, in order that we may more precisely estimate the total injuries and benefits. Such a summary gives us a total annual consumption of 66·5 per cent. of vegetable matter and 33·5 per cent. of animal matter. Of the former, only 2·5 per cent. consists of grain, 56 per cent. of leaves, fruits, and seeds of weeds, 1·1 per cent. of roots, and 6·9 per cent. of miscellaneous vegetable matter. Of the animal matter, 20·1 per cent. consists of injurious insects, 1·7 per cent. of beneficial species, and 2·7 per cent. of neutral species, 6·1 per cent. of earthworms, and 2·9 of slugs. In short, 72·8 per cent. of the food is of a neutral nature, 4·2 per cent. injurious, and 23 per cent. beneficial.

Even if we omit any mention of the benefits, the injuries inflicted by the feeding habits of these three game-birds are scarcely worthy of mention as compared with their value as food; but when we take into account the 23 per cent. of injurious insects and slugs, they cannot be regarded as other than a direct benefit to agriculture, and a valuable source of home-grown food. Moreover, it is interesting to note that efforts have been made in other countries to preserve all game-birds, and in not a few foreign species have been introduced in consequence of their value to the agriculturist and as a source of home-grown food.

With the politics of game-bird preservation we have already stated we are not here concerned, and we refuse in a study of this kind to listen to the partisans of either one political faction or the other; but when so valuable an asset to agriculture and the country in general is threatened, it is surely not beyond the wit of man to so modify or amend the existing Game Laws

as to preserve the private rights of tenant farmers, and so remove a grievance which has undoubtedly engendered much ill-will against these three particular species of birds.

At the present time all home-grown food is of double value, and every factor that aids in destroying injurious insects is also indirectly conducive to increasing that supply. The three species of game-birds here considered fulfil both of these purposes.

In connection with one species, the pheasant, our Government have deemed it wise to proclaim and foster its destruction; but in view of its exceedingly beneficial services to agriculture, such action is to be deeply regretted and most strongly condemned. If the destruction of wild birds is at last going to receive attention, there are species such as the house-sparrow, wood-pigeon, rook, starling, etc., which are annually costing the country millions of money in the food products destroyed. Just as scientific investigation has shown our game-birds to be beneficial, so it has shown these to be injurious. That the former should be destroyed and the latter allowed to continue their depredations upon the nation's food is surely a striking instance of the lack of appreciation of scientific investigation by those in authority, and how little they are really concerned with the true interests and welfare of agriculture.

CORRESPONDENCE

To the Editor of "Science Progress"

A CORRECTION

From J. Reid Moir

In reference to your reviewer's cogent remarks in the July issue of Science Progress (p. 63) regarding the elephant-bone described in my paper "On some Human and Animal Bones, Flint Implements, etc., discovered in two Ancient Occupation Levels in a small Valley near Ipswich," I know of no reason for thinking that this "apparently anachronistic tibia is of the nature of a derived fossil." The specimen was found in my presence impacted in the lower floor, associated with the Upper Monsterian flint implements and other relics described by me. The bone presents a precisely similar appearance to many of the specimens from this occupation-level, and, like these, it exhibits no signs of rolling or abrasion by any agent of transport. I have no doubt but that the bone is of the same age as all the other relics from the lower floor. In fact, this appears to me to be what is generally known as a "certainty."

J. Reid Moir.

Ipswich, *August* 8, 1918.

NOTES

William II. (Charles Mercier, M.D., F.R.C.P.)

PROF. LUGARO is well known and highly valued in this country for his work in alienism, and whatever he says about madness is worthy of attention.[1] In the first year or two of the war, people in this and other Allied countries were much inclined to attribute it to the personal ambition of the Kaiser and the Emperor of Austria, and to argue that the first of these was certainly, and the second was probably, mad. I have myself examined in *Land and Water* the first assertion, and arrived at the same conclusion as Prof. Lugaro, that bombastic, truculent, and ridiculous as William II. has shown himself to be, there is no reason to suppose that he is mad. He is Emperor of the German people, and he displays in the highest degree the qualities that the German people admire and cultivate—that is to say, brutality, treachery, greed, unbounded conceit, and braggadocio. "He is fickle, restless, superficial, vain, and very ambitious; but he is not a man to impress his personal stamp on history. Mediocre in intelligence, he has never shown any genius for leadership; infirm of purpose though exuberant in activity, he has always finished by sailing with the current, or at most has allowed himself some impulsive act, some inopportune exploit, for which he has had immediately to make amends." "The Emperor is at bottom a docile, though occasionally maladroit, instrument of the Government." With this judgment I wholly agree. As for Francis Joseph—a dull, stupid, immoral animal, with the intellect of an ox and the heart of a bigot, it is absurd to suppose that his personal influence counted for anything in the determination of Austria to go to war. No, the war is the natural and inevitable expression of the spirit of the German nation. "It is not an accidental and unexpected catastrophe, but it is the result of a thousand historical forces; it is not the expression of individual caprice, but emanates from the will of a nation. The war was prepared for with the long, patient, dogged labour of several generations; it was foreseen and was predicted." The blame rests upon the whole German nation. Germans "do not understand that a people may possess a hundred universities, a thousand laboratories, innumerable perfect workshops, and a flourishing commerce, and, notwithstanding all this, may be barbarians." If the war was due to madness, it was the madness of the whole German nation—a madness that began in megalomania, and is now turning, as megalomania often does, to paranoia—the delusion of persecution. Germany "curses the wrong-headed resistance of ungrateful peoples who do not wish to know happiness under German discipline. Belgium persists in not letting herself be finished off. France will yield neither money, territories, colonies, nor liberty. The English cousins, traitors, will not quietly allow themselves to be bereft of all the means of life and everything that makes life worth living. Italy fancies that she has a personality of her own. Russia, the barbarian, will not let one even strangle little Serbia. It is a downright conspiracy of wilful wickedness."

[1] *An Emperor's Madness, or National Aberration*, by Ernesto Lugaro. Routledge, 1916.

No doubt Prof. Lugaro is now preaching in the main to the converted. Few people now take the war to be the work of any individual person, or even of any small group of individual persons, except as to the precise time and manner of its outbreak, and most people have had a general notion of the peculiar beastliness of the German mind impressed upon them by the events of the war; but I do not know of any book that sets these things out so clearly, so forcibly, so convincingly, as this little volume of Prof. Lugaro's. If I did not know, from long experience of alienists, logicians, and philosophers, how utterly impervious men are to the plainest and most convincing demonstration when their prejudices or their interest militate against conviction, I should commend the book to Lord Lansdowne, Lord Loreburn, and the noisy little band of pacifists, anti-conscriptionists, and traitors who serve Germany's interests so well in this country. There must be some among them who are honest. There may be some among them who can be influenced by reason. If there are any of them who combine the two qualifications, their pacifism and Germanophilism will not survive the reading of this admirable little volume, which I most heartily commend to them as well as to every one else.

The Sheffield Association of Metallurgists (W. H. Hatfield, D.Met.)

I have pleasure in accepting your invitation to send a few remarks upon our Association. Several different scientific and technical associations have been recently formed, and each has been, no doubt, created to meet some generally felt want of its members. Our own is no exception to that rule, and your readers may be interested to learn something of our aims and objects. An adequate application of Science to Industry is one of the most essential needs of our time, and one of our principal objects is to facilitate that application.

Sheffield has been fortunate in the past in that she has laid well for the future. We possess a University of indigenous growth. The University developed some few years ago from our University College, the latter in its day having resulted from the joining up of our Technical School and the Firth College, founded some forty years ago by leading townsmen of sound insight. During this period of evolution a number of highly trained men have been fed into the local industries, and it will be clear that, together with the influx of scientific men from outside, we have now a substantial number of men in the district engaged upon the scientific and technical sides of industry. One of our objects is to give these men opportunities of more intimate intercourse with each other.

The war has inculcated a more enlightened attitude towards the exchange of ideas, and we have sought to turn this to account by holding weekly meetings at which subjects of immediate interest to the industry are discussed. As the Press is excluded and our meetings are distinctly informal, a freedom of discussion is obtained such as can hardly be expected at the formal meetings of our well-known scientific institutions. The result is, that from a scientific and technical point of view our members are able to help each other, and it will be appreciated that such an all-round interchange of ideas cannot but have suitable influence upon progress.

The social side of our Association is to be cultivated by the formation of a Club, and it has been already tentatively suggested that we consider the housing of it in the same building which is shortly to be erected for our Chamber of Commerce. From this you will see that the industries, as such, are putting forward a friendly hand and appreciate the work which we are doing. In connection with the Club,

it is our intention not only to create a good technical library, but also to form a museum for taking care of objects of interest to the members.

A few remarks are due to the economic side of our activities. One of the objects of our Association is to take care of the professional status of scientific men engaged in the industries. The Full Members of our Association are all qualified men, *i.e.* they have either a Degree or Associateship of a University or University College, together with several years' experience in a recognised laboratory, or, if they have not this qualification, then they have attained a position in their industry which is a sufficient indication of their capacity. It will thus be seen that we have joined up a large number of men who are quite capable, should the occasion arise, of taking care of the status and dignity of the profession. These are difficult times, and as to what direction the Association's energies will need to be employed in such matters remains entirely to the future. As a body we feel that our status is considerable, and at any rate in this district the application of science to industry is increasingly appreciated. An indication of our point of view will be obtained from the fact that we, as a body, consider industry to be composed of three forces—Capital, Enterprise, and Labour. We consider the Applied Science Men an important factor in Enterprise.

It may be of interest to state that, besides an Associate Membership, which embraces the younger trained men, we have a large number of Associates who are men not entitled for Full Membership, yet who hold responsible positions, and are fitted, in the opinion of the Council, to take part in the discussions and enjoy the amenities of the Association. The reason for the formation of this class of Membership lies in the fact that it is to the friendly intercourse between the management in the works and the more scientific members of the staff that the smooth and satisfactory application of science to industry depends.

I think that in some ways our Association is novel, but we have no doubt as to its usefulness, and we sincerely trust that other manufacturing cities and districts will follow our example and form similar associations. If such local associations can be formed, it may not be impossible to affiliate them, and the result would be a national organisation, in which case we feel that the welfare of the industries with which the members are associated will be well served to the advantage of all concerned.

In conclusion I may say that our relation with existing scientific institutions and societies is most cordial.

Notes and News (D. O. W.)

Honours.—Among those whose names appeared in the Honours List, published on the occasion of the King's Birthday, were the following:

G.C.V.O.: Sir Alfred Keogh.

K.C.B.: Surgeon-General H. D. Rolleston.

K.C.M.G.: Sir Ronald Ross, K.C.B., F.R.S.; Sir William Leishman, F.R.S.; Prof. John Cadman.

K.C.S.I.: Sir Thomas Holland, F.R.S. (President Indian Munitions Board).

Knights: H. Baldwin (Dental Surgeon to the King and head of the Kennington Facial Hospital); M. Robson (past Vice-President of the Royal College of Surgeons); P. W. Squire (Chemist to the Royal Family for half a century).

K.B.E.: Horace Darwin; Lieut.-Col. A. G. Hadcock, F.R.S.; A. C. Houston (Director of Water Examinations, Metropolitan Water Board).

C.M.G.: Prof. H. L. Ferguson, Professor of Ophthalmology, University of Otago.

C.B.E.: Prof. E. C. C. Baly, F.R.S.; J. Barcroft, F.R.S. (Superintendent of Physiological Investigations, Chemical Warfare Department, Ministry of Munitions); Conrad Beck; H. N. Dickson (Professor of Geography, University College, Reading, and Head of the Geographical Section of the Naval Intelligence Division); Prof. H. B. Dixon, F.R.S.; Major A. S. Eve, F.R.S. (Resident Director of Research, Admiralty Experimental Station, Parkeston); C. H. Wordingham (President of the Institution of Electrical Engineers, and Director of Electrical Engineering at the Admiralty).

O.B.E.: Prof. P. G. H. Boswell (Geological Adviser to Ministry of Munitions); F. G. Edmed (Principal Assistant Chemist, Inspection Department, Ministry of Munitions); A. P. M. Fleming (Engineer, British Westinghouse Company); J. S. Flett, F.R.S. (Assistant to Director of Geological Survey of Great Britain); Miss Hilda P. Hudson (Aero-dynamics Technical Research, Aircraft Production Department); Prof. H. M. MacDonald (Labour Department, Ministry of Munitions); Prof. J. C. Philip (Hon. Secretary Chemical Society); S. Smiles (Assistant Professor of Chemistry, University College, London): Miss May Thorne, M.D.; Miss Martha A. Whiteley (Chemical Warfare Department, Ministry of Munitions).

M.B.E.: F. P. Burt (Lecturer in Chemistry, Manchester University); F. Harrison Glew (Radium Expert, Ministry of Munitions); J. Irvine Orme Masson (Research Chemist, Woolwich); Miss Frances Micklethwait (Experimental Chemical Supply Officer, Trench Warfare Department); Lieut. J. R. Partington (M.I.D.); Lieut. E. K. Rideal (M.I.D).; J. D. McBeth Ross (Chemical Warfare Department, Ministry of Munitions); J. Waite (in charge of Steel Research, Ministry of Munitions); C. A. White (Principal, Aston Technical School, Birmingham).

Sir R. T. Glazebrook has been awarded the Albert Medal of the Royal Society of Arts for 1918, "for his services in the application of science to the industries of peace and war, by his work as director of the National Physical Laboratory since 1899, and as Chairman of the Advisory Committee for Aeronautics."

Sir Joseph Larmor has been awarded the Poncelet Mathematical Prize of the Académie des Sciences.

Oliver Heaviside has been elected Honorary Fellow of the American Institute of Electrical Engineers.

Sir J. J. Thomson, Prof W. H. Bragg, and Dr. W. Coolidge have been elected honorary members of the Röntgen Society.

Sir James Dewar has been awarded the Medal of the Society of Chemical Industry in recognition of the services he has rendered to chemical industry by his work in pure and applied science.

The Pereira Medal of the Pharmaceutical Society has been awarded to Miss H. C. M. Winch.

We have noted with great regret the announcements of the decease of the following during the past quarter: The Duke of Northumberland (President of the Royal Institution); Mr. C. D. Ahrens (optical instrument maker); Prof. Vladimir Amalitsky (Professor of Geology in the University of Warsaw, and Director of the Polytechnic Institute in that city); Prof. C. Blarez (Professor of Chemistry at the University of Bordeaux); Dr. F. Braun (Professor of Physics at Strassburg and Nobel prize-winner in 1909—the prize being divided with Signor Marconi); Dr. R. O. Cunningham (Emeritus Professor of Natural History and Geology at Queen's College, Belfast); Dr. W. J. M. Ettles (one time President of

both the Optical Society and the Hunterian Society); Dr. R. G. Hebb (editor of the *Journal of the Royal Microscopical Society*); Dr. F. Hodson (head master of Bablake School, Coventry, and editor of *Broad Lines in Science Teaching*); Col. Bertram Hopkinson, F.R.S., C.M.G. (Professor of Applied Mechanics at the University of Cambridge), in a flying accident; M. Jules Lachelier (French philosopher); Dr. E. Newall Arber (demonstrator in Palæobotany at the University of Cambridge), Prof. H. G. Plimmer, F.R.S. (Professor of Comparative Pathology at the Imperial College of Science); Dr. G. M. Searle (Professor of Mathematics at the Catholic University, Washington); Prof. K. Toyama (Professor of Zoology in the University of Tokyo).

Mr. Charles Hawksley bequeathed a sum of £3,000 to the Institution of Civil Engineers for the provision of scholarships and prizes.

Sir Basil Zaharoff has provided £25,000 for the establishment of a Chair of Aviation at the University of London.

It was reported in *Science* (Feb. 22, 1918) that the anti-vivisectionist contributors to the Red Cross in the U.S.A. had commenced a lawsuit to prevent the Society from devoting $100,000 of their funds to medical research involving experiments on animals. In order that the investigations, so urgently needed for the relief of sufferers from trench fever, gas gangrene, gas poisoning, and similar war diseases should not be hindered, Dr. Cleveland H. Dodge of New York has offered to provide the funds required for the whole of the work, and has subscribed $250,000 for the purpose. His generous action will permit of a large extension of the research originally planned.

The following extract from the report of the proceedings in the libel action between Sir Charles Hobhouse and Mr. Godfrey Isaacs typifies the Government attitude towards scientific matters in pre-war days, although it is perhaps unusual to find it stated in so bare-faced a fashion and by such authority.

> "Asked why he had been selected to be the chairman of the Wireless Research Committee, having no scientific knowledge of Wireless, Sir Charles said that this was the principle on which appointments to committees were made. (Laughter.)
>
> "He was Chancellor of the Duchy of Lancaster, and was used for all sorts of odd jobs. The Chancellor of the Duchy was supposed to have the least to do of all Ministers, although he had plenty on his hands. Before the witness was chairman of the Research Committee he was chairman of the Fisheries Committee."

There does not seem to be any very reassuring evidence that the same method of appointment will not be used in future also.

At a special general meeting, held on July 31st, the Royal Society discussed the desirability of expelling enemy alien members on a motion by Sir George Beilby and Dr. M. O. Forster, that "as there is no indication that the scientific men of Germany are unsympathetic towards the 'abominable malpractices of their Government,' steps should be taken for removing all enemy aliens from the membership of the Society." It was decided that the delegates of the Royal Society should raise the question at the forthcoming conference with the representatives of the academies of Allied countries.

It is stated in the daily press, on Brazilian authority, that the guns used for the bombardment of Paris are made of Zirconium-steel; the basis of the report being apparently that large quantities of baddeleyite were purchased by Germany (? before the war commenced). Zirconia has a high melting-point and is used as a refractory for furnace linings, etc., and in the Report on the *Metalliferous Ores used in the Iron and Steel Industry*, published by the Department of

Scientific and Industrial Research, it is stated that Weiss, in 1911, took out a patent for the use of zirconium, in place of titanium, as a deoxidising agent in metals and alloys which also greatly increased the tenacity and strength of the metal. There is no indication in the Report that the process has been used in this country, and in pre-war days the output of zirconium minerals was spasmodic—*e.g.* Brazil produced 25 tons in 1911 and 126 tons in 1909, all for the German market.

The correspondent of the *Morning Post* reports that M. Roald Amundsen started on his polar expedition on June 25 in his new ship, the *Maud*. The expedition is expected to cost 1,000,000 kroner (£56,250), and its chief object is the scientific exploration of the ocean and atmosphere of the polar regions. The attainment of the North Pole itself is regarded as being of minor importance. M. Amundsen will undertake the magnetic observations for which the outfit has been provided by the Carnegie Institution of America. The rest of the scientific work is in charge of M. Sverdrup. Balloons and kites will be used to investigate the higher strata of the atmosphere, and the Aurora Borealis will be photographed by a special method devised by Prof. Störmer. The stores carried are calculated to be more than sufficient for five years, and included in the scientific equipment is a wireless outfit and a number (?) of aeroplanes.

The annual report of the Rockefeller Foundation for 1917 contains a remarkable record of the humanitarian work which is being carried on by the Institution in all parts of the world. A large proportion of the normal income has been diverted to work in connection with the war, since "the well-being of mankind throughout the world" so obviously depends on the success of the "forces of freedom." By an appropriation of $5,000,000 from the Capital Fund and a further donation of an equal amount from the Founder, it has been possible to carry on, in addition, the usual work of the Foundation. This includes an educational campaign against tuberculosis in France; anti-malarial work in Arkansas and the Mississippi Valley; a campaign designed to exterminate yellow fever from the whole world; the establishment of medical colleges in China, and the provision of a hospital steamer for work among the isolated islands in the Philippine group. The Foundation has a principal of about $120,000,000 at its disposal, the income derived therefrom being just over $7,000,000.

The annual report of the Decimal Association shows that, during the past year, there has been a considerable growth of opinion in favour of the decimal system. A large number of public bodies of all kinds have passed resolutions favouring the change in our system of units, and the Decimal Coinage Bill, which is based on the results of conferences between the Association, the Institute of Bankers, and the Association of Chambers of Commerce, has been introduced into the House of Lords by Lord Southwark. The debate on the second reading of the Bill was adjourned on the understanding that the Government would institute an inquiry into the whole question of decimal coinage. Reports have been received from the British Chambers of Commerce in Italy, Brazil, and the Argentine, emphasising the benefit which would accrue to our foreign trade if the metric system of weights and measures were made compulsory throughout the Empire. A similar conclusion has been reached by the *American Journal of Electricity*, which reports that the non-use of the metric system in the U.S.A. among engineers and in the electrical trade is beginning to be felt as a serious handicap in American efforts to secure a commercial and engineering foothold in South America. The system has been adopted in the American army for artillery work and map construction, and it is stated that, since February 1917, the shops in Pekin have also adopted

metric measures. The following appeal is made to all who are interested in the movement:

"The present is a critical time in the life of the Decimal Association. Success appears within our grasp, but to carry on the rapidly increasing propaganda work, greater expenditure is required, and all those who are in sympathy with our aims are earnestly requested to become members of the Decimal Association, or, if already subscribers, to send a donation in order that this great opportunity be not lost through lack of funds."

The offices of the Association are at 212, Finsbury Pavement House, E.C.2.

The Twelfth Annual Report of the Executive Committee of the British Science Guild (July 1918) contains a memorandum dealing with the same subject. The sub-committee appointed to consider the question oppose any sudden change in our system of units, and propose rather that the decimal system should be introduced gradually by making it more convenient than that in present use. They suggest that it should be used in Government announcements and contracts beginning with easily practicable cases, which should be gradually extended until ultimately all tenders, invoices, etc., for government purposes are compulsorily expressed in the new system. It is further suggested that contract should be made unenforceable in the Law Courts except in terms of the Metric System, and that the expense of officially certified conversion into that system of the terms used in the contract should be put upon the litigant seeking relief. An attempt has been made by Kynoch & Co. to introduce the metric system into their works. It had to be given up partly because no one would invoice to them in metric units, and partly because the Railway Companies refused to accept goods for which the consignment notes were expressed therein. As to this last it may be noted that the Government has already power to insist on railway companies accepting such goods.

Another very important document included in the report of the Guild is that signed by Sir W. A. Tilden dealing with the Dye Industry. It is there pointed out that the share and debenture capital of the German companies has recently been much increased, and that still further increases are contemplated. The total amount is stated to be considerably in excess of fifty millions of pounds, while to compete with this the amount employed by firms in this country is between four and five millions only. Moreover, the financial aspect does not give a full measure of the disparity in the two positions, for Germany has at her disposal a large body of expert chemists trained and experienced in the processes of manufacture. In England the number of qualified chemists is insufficient for the purpose of the dye trade alone, and out of this inadequate supply "many scores, perhaps hundreds, have been taken for the army." (Since the report was written there has been a further call-up of chemical students of low medical grade.) Finally the German has realised the all-important fact that chemists must be admitted to a position alongside the business men on the directorate of technical companies. Until this is realised here also it will be impossible to cope with the organisation of German firms.

Further steps have been taken by the Government to aid the Dye Industry—*e.g. Nature*, May 23, June 30, July 25. In the first place a sum of £2,000,000 is to be distributed through the Board of Trade to those firms not helped at the time when British Dyes, Ltd., was formed. Of this sum £1,250,000 is to form a loan repayable in twenty years' time (or less if the profits exceed 9 per cent.); £600,000 is to be used for the extension of plant and buildings, and £150,000 is to

be devoted to research. A first instalment of £1,000,000 will be available in the financial year ending March 31, 1919, and the rest in the two years following. In addition to this monetary assistance the importation of foreign dyestuffs will be controlled for a period of ten years after the war by a system of licences. Finally, it appears that British Dyes, Ltd., are to amalgamate with Messrs. Levinstein, Ltd.

In his Presidential Address at the annual meeting of the Science Guild on June 19, Lord Sydenham referred chiefly to the problems of post-war reconstruction and scientific education. He quoted (apparently on the authority of Sir Robert Hadfield) some amazing statistics bearing on this latter question. The total incomes of State-aided modern Universities and University Colleges in England and Wales is about £700,000, of which 34 per cent. is derived from Parliamentary grants. In Germany the corresponding figures are £2,000,000 and 80 per cent., while in America the income is £20,000,000, of which £7,000,000 is at the disposal of the Colleges of Agriculture and Mechanical Arts. In addition to this, for each 10,000 of the population there were 16 full-time students in Scotland, 13 in Germany, 10 in the U.S.A., and 5 only in England and Wales. Lord Sydenham concludes that the Fisher Education Bill will at least furnish an opportunity for the much-needed improvement in the education of the masses, and that if only the recommendations of Sir J. J. Thomson's Committee were grafted on to it, there would follow a great increase in the number of science students.

It appears that New Zealand is the only one of the larger self-governing Dominions which has not yet set up any machinery for the State Organisation of Scientific and Industrial Research. This seems to be due largely to the fact that the Government has not yet realised the important bearing this matter will have on the future welfare of the industries of the country. At the request of the Minister of Internal Affairs, a report has been drawn up by G. Hogben, Esq., C.M.G., and Dr. J. Allan Thomson, summarising the schemes adopted in the other parts of the Empire, and also the suggestions which have been put forward as to the procedure which should be adopted in New Zealand. Their report shows that the New Zealand Institute, the Board of Science and Art, the General Council of Education, and the Wellington Philosophical Society are all in practical agreement as to the steps which should be taken. They urge the Government to set up a National Advisory Council on Research, or a Board of Science and Industry, which should encourage and co-ordinate scientific and industrial research in the Dominion, and advise the Council of Education as to the lines along which a general improvement in scientific education could be brought about. On the industrial side there appears to be, in particular, need for more trained hydro-electric engineers, and also for methods whereby the agricultural produce of the country may be brought into closer touch with the markets of the world.

One material result of this activity in the Dominion has been the publication of the *New Zealand Journal of Science and Technology* issued under the authority of the Minister of Internal Affairs. It is intended primarily as a means of publishing in a collected form such departmental papers and reports as may, in the opinion of the Board of Science and Art, deserve permanent record. (Long papers, which cannot conveniently be published in the Journal, will, in future, be issued as Bulletins, numbered consecutively from 1 upwards.) In addition, however, the Journal will contain a certain number of more popular articles, so that it may appeal to a wide circle of readers and "focus the more favourable attitude to science created by the war." As far as it is possible to judge from the first two numbers of the Journal (Jan. and March 1918) it will contain a collection of papers which should afford valuable assistance to the

industries of the country, while, from the outsider's point of view, the articles on the biology and geology of various localities will be of most importance. It is to be hoped that the enterprise will meet with its deserved success and help to stimulate greater interest in science in the Dominion.

Last quarter Dr. Charles Mercier reviewed in its broad outlines the report of Sir J. J. Thomson's Committee on the position of Science in our Educational System, and there will probably be very little disagreement with the praise he bestowed upon it. Nevertheless, when we come to consider the finer details, it must be admitted that some are open to serious criticism. Thus, the last section of the report deals with "the supply of trained scientific workers for industrial and other purposes." It begins by deploring the leakage—some 60 per cent. of the total entrants—which occurs in secondary schools before the general course is finished, and states that:

> "The want of appreciation by parents of the benefits of secondary education prevents a full utilisation of the resources in the way of scholarships which are even now available. . . . Much as we may regret it, there is no doubt that appeals for secondary education for its own sake appear far-fetched to the majority of parents, and leave them untouched." [1]

To correct this view it is suggested that "steps should be taken to put before parents in as clear and as simple a way as possible the careers open to those who complete a course of secondary education . . . and the assistance diligent students might expect from schlarships." It is not impossible that there may be some difficulty in doing this. Diligent students may indeed obtain assistance from scholarships; they will certainly have to undertake a strenuous course, and undergo the strain of many examinations; the middle-class parent will certainly have to bear a considerable financial burden, but what is not certain is the ultimate reward. One short paragraph in the report throws a little light on this darkness:

> "If industry wants men of scientific ability who have taken a College course extending over four or five years, it must be prepared to pay for them. To offer salaries of £100 to £150 a year with very indefinite prospects of future advancement is useless The salaries and prospects of advancement must be such as to induce able young men to continue their education up to the age of 22 or 23 and to persuade poor parents to bear the additional burden involved."

Further information will be found in a letter written by Dr. E. N. da C. Andrade to *Nature* (June 29, 1916) on "Science, Scholarships, and the State," which presents an aspect of the matter most inadequately treated in this report. Finally, in another sphere, there is the statement made by Mr. T. Gautrey, at a meeting of

[1] This notion is not limited to the middle classes. In a speech on the Fisher Bill the Right Hon. Sir F. G. Banbury expressed himself thus: "His experience in the City was that the man who took Firsts at Oxford generally came out last, and that the man who could hardly write his name *generally* came out first. The explanation was that education could not put into a man that instinct of self-preservation and common sense which was the foundation of all success in business. How could education assist a farm labourer to spread manure on a field? The best labourer he had known was wholly illiterate. If the waste of the war was to be replaced it would be necessary for the young to start as early as possible in doing a day's work instead of wasting time on useless book learning." (Extracted from Prof. Pope's Presidential Address to the Chemical Society, March 1918.) A finer double-barrelled testimonial for the utter uselessness of a public school classical education would be hard to find.

the L.C.C., in connection with the threatened strike of women teachers, that "some 400 women teachers were just entering the Council's service after seven years' training, and . . . at £108 a year they would be worse paid than any other class of adult permanent employees of the Council, *except the lavatory attendants in the parks and open spaces, who were getting sixpence a week less*"!

The need for science graduates is not disputed; the demand is another matter, and it is unfair to attract students by numerous scholarships to pass through a University or College course and then to leave them stranded, or with an entirely inadequate reward, at the end. Before increasing scholarships and increasing the competition for a bare livelihood in what, in pre-war days, was an overcrowded profession, it is first necessary to educate the industrial world; to open a reasonable number of posts in the technical departments of the Government to men who have specialised in science and to improve out of all recognition the position of technical teachers. Once the public realises that first-class opportunities are provided by a scientific career there will be no lack of candidates.

One other point demands criticism. In connection with the need for raising the status of teachers in secondary schools it is stated that, "In no profession is it more important that the members should have reasonable opportunities of foreign travel and wherewithal to purchase books," and, again, in another place, "It is essential in Technical as in other schools that there should be a considerable raising of the status and remuneration of whole-time teachers, and that this improvement should include a national scheme of adequate pensions."[1] When, however, the stipends of University teachers are reviewed a different conclusion is reached. It is admitted that "there is serious discontent with the salaries *and prospects* of the Junior Staff in the scientific departments." (The exact significance of "Junior Staff" is not defined, but from the context quoted it may be presumed to apply to nearly all but Heads of Departments.) Also that "there are many competent men doing responsible work at salaries far below what they could have earned in other walks of life." The concession proposed is to allow them at least two days a week for private research. Granted that these men do not need "the wherewithal to purchase books," or "opportunities of foreign travel," or to pay subscriptions to scientific societies, or to take any part in the social life of the scientific world, nevertheless the Committee might at least have pointed out that in many cases no small part of the "considerable time at their disposal for original research" will necessarily be occupied in hack work required to make up their salaries to a living wage. If there were a great demand for their services elsewhere the procedure recommended might be defensible; but in the years immediately preceding the war such was not the case. Hence the discontent with the prospects which the Committee admits but makes no effort to remedy. The whole tenor of the report is the need for more students to pass to the Universities and Technical Colleges. Apparently these are to gain their knowledge from teachers whose sole interest, in very self-defence, must be the achievement of research which will enable them to escape from their environment, and to whom any time given to the improvement of their teaching, to the study of the difficulties of their students, even to the preparation of their lecture experiments, is time

[1] Mr. Fisher "hopes to introduce a Bill relating to the superannuation of secondary school teachers during the present session of Parliament." [The chief clauses have been published. It is proposed to give all teachers in State-aided Institutions pensions on the Civil Service scale. Teachers in Universities are specifically barred!]

deliberately wasted and expended to their own detriment. The final statement in the paragraph, while suggesting a remedy, only serves to betray the unfairness of the conclusion the committee has reached:

"There will always be men whose best work can be done in laboratory teaching and organisation, and it is desirable that the Universities *should be* in a position to retain them by providing for senior men a *small* number of permanent posts of substantial value; *at present this is seldom possible.*" The italics are ours.

* * * * * *

The Governing Body of Birkbeck College have appointed Dr. George Senter, formerly Head of the Chemistry Department of the College, to the office of Principal, recently vacated by Dr. George Armitage-Smith, who had filled the position for more than twenty years. Birkbeck College has been recognised by the Royal Commission as the future centre of Evening University work in London.

The preliminary report of the Water-Power Committee of the Conjoint Board of Scientific Societies, which has just been issued, states that, while the potential water-power of the Empire amounts in the aggregate to at least 50,000,000-70,000,000 horse-power, very little has been done, except in Canada and New Zealand, to develop its usefulness. (To indicate the magnitude of this figure it may be pointed out that the total power from all sources now being used in the various countries of the world is estimated at 120,000,000 h.p.) In the United Kingdom only 8·3 per cent. of the available water-power is being utilised—a figure which compares badly with 43·4 per cent. in Germany and 25 per cent. in the United States. In this respect, indeed, Great Britain is the most backward of all the countries considered with the exception of Russia (5 per cent.). In Canada some 1,750,000 h.p. is actually being obtained by water-power; an additional 6,000,000 h.p. is readily available within the range of present markets, and will probably be employed in the course of the next two decades. In New Zealand only about 42,000 h.p. is at present being used, but many promising schemes have been worked out and some 3,800,000 b.h.p. is expected. The Committee considers the position in that country to be most favourable, and anticipates the commencement of an era of great electro-chemical and electro-metallurgical activity there. It should be noted also that the potential water-power of New Guinea is estimated at 15,000,000 h.p. Taken in conjunction with its great fertility, its prospect of coal and mineral oil, and its already partially developed metallic mineral resources, this augurs well for its future, and an immediate survey of the more promising localities seems desirable.

The Committee concludes that the prosperity of the Empire may be so influenced by the wise development of its water-power that the matter should not be left to chance, and recommends the formation of an Imperial Water-Power Board, to include a representative from each of the Dominions and Dependencies, which should act in an advisory capacity. It recommends further that, since it is unlikely that private capital will be available for many years for hydraulic development on any large scale, powers should be obtained to enable the State to assist or to undertake such development as is thought to be advisable.

ESSAYS.

OPERATIVE ALGEBRA: OPERATIVE INVOLUTION (Colone Sir Ronald Ross, K.C.B., K.C.M.G., F.R.S.)

10. In 1905 I described a method for the explicit rendering of algebraic operations apart from their subject (1). So far as I know, no attempt of this kind had been made previously, and I therefore called such expressions Verb-Functions; showed that they produce a *formal algebra* of operation apart from quantity; developed some of its elementary rules, homologous to those of numerical algebra; and illustrated the subject by using "operative division" for the solution of algebraic and differential equations (1, 4, 5, 6).

In 1908 I described briefly another method for resolving numerical algebraic equations, namely by iteration—a method which was subsequently found to have been originated by Michael Dary, a friend of Newton, in or before 1674 (2, 3). In 1915-16 I resumed these subjects in SCIENCE PROGRESS (4, 5, 6), and showed that the solution of equations by operative division provides the algebraic expression of their solution by arithmetical iteration. I now give some fundamental theorems of Verb-Functions, mostly connected with "operative involution," and all taken from a lengthy paper completed in July 1916, but not published owing to war-work—of which paper this one is an abstract.*

It should be added that the notation employed really presents no difficulty, while it greatly abbreviates and facilitates the whole algebra of substitution by bestowing upon it a formal process like that of algebra. And practically no other notation can be employed if we wish to denote operation apart from subject (1, 4). We merely define that $\mathbf{O}^n$ is an operator which raises its subject to the nth algebraic power, and that an expression within square brackets is an operator which operates on the matter which follows—as for example in

$$[\mathbf{O}^n]x = x^n\ ;\ [a + b\mathbf{O}^m - c\mathbf{O}^r]x = a + bx^m - cx^r.$$

An index outside square brackets denotes that the operator within those brackets is to undergo operative involution (also called iteration) or evolution, as the case may be. But this notation will be familiar to those who have read the previous papers in SCIENCE PROGRESS (4, 5, 6). It is really based on recognition of the fact that ϕ^0, $\triangle^0$, Σ^0, D^0, etc., are not equal to numerical unity, as generally supposed, but to the *operative unit* or *symbol of substitution*, $\mathbf{O}$. The error that $\phi^0 = 1$, $\triangle^0 = 1$, and so on, has largely vitiated symbolism in the past; and the correction of the error now renders this symbolism as rigid as that of ordinary algebra. Note that $\mathbf{O}$ is not, of course, a number but an operation; that $\mathbf{O}^0 = 1$; and that, when a is a number, $[a]x = [a\mathbf{O}^0]x = ax^0 = a$. The square brackets may be dropped for purely operative symbols, as in writing $\phi\psi$ and ϕ^n for $[\phi]\psi$ and $[\phi]^n$; but then

* Much work was done by me years ago on the application of Verb-Functions to the Calculus, by studying the iteration of operations which vary from step to step—by which I deduced a Calculus of which the additive Calculus generally used is only one case. I also applied them to Determinants, Eliminants, etc., but these researches cannot be published till this one is.

$\phi\,.\,\psi$ and $(\phi)^n$ must be employed to denote algebraic multiplication and involution. I use Ω^n to denote an operator which raises to the nth *operative* power—so that $[\Omega^n]\phi=[\phi]^n=\phi^n$; but this will not be required in the present paper (see end of 12·0).

2·0. The following elementary theorems will also serve for examples.

$$[p^q\mathbf{O}^r]^2=[p^q\mathbf{O}^r][p^q\mathbf{O}^r]=p^q(p^q\mathbf{O}^r)^r=p^qp^{qr}\mathbf{O}^{r^2};$$
$$[p^q\mathbf{O}^r]^n=p^qp^{qr}p^{qr^2}\ldots p^{qr^{n-1}}.\,\mathbf{O}^{r^n}=p^{q(r^n-1)/(r-1)}\,.\,\mathbf{O}^{r^n} \qquad 2·1$$

when n is a positive integer. If m and n are positive integers

$$[p^q\mathbf{O}^r]^m[p^q\mathbf{O}^r]^n=[p^{q(r^m-1)/(r-1)}\,.\,\mathbf{O}^{r^m}][p^{q(r^n-1)/(r-1)}\,.\,\mathbf{O}^{r^n}]=[p^q\mathbf{O}^r]^{m+n};$$
$$[[p^q\mathbf{O}^r]^m]^n=[p^{q(r^m-1)/(r-1)}\,.\,\mathbf{O}^{r^m}]^n=[p^q\mathbf{O}^r]^{mn};$$

from which 2·1 may be proved to hold for all real values of n.

As corollaries,

$$[p^q\mathbf{O}^r]^{-1}=\sqrt[r]{\mathbf{O}/p^q};\ [p^q\mathbf{O}^r]^{1/m}=p^{q(s-1)/(s^m-1)}\,.\,\mathbf{O}^s \text{ where } s\equiv\sqrt[m]{r};$$
$$[\mathbf{O}]^n=[-\mathbf{O}]^n=[\mathbf{O}^{-1}]^n=[-\mathbf{O}^{-1}]^n=\mathbf{O}, \text{ if } n \text{ is an even integer};$$
$$[\mathbf{O}]^n=\mathbf{O},\ [-\mathbf{O}]^n=-\mathbf{O},\ [\mathbf{O}^{-1}]^n=\mathbf{O}^{-1},\ [-\mathbf{O}^{-1}]^n=-\mathbf{O}^{-1}, \text{ if } n \text{ is an odd integer.} \qquad 2·2$$

The operative roots of these expressions involve algebraic roots of unity.

It can easily be shown by substitutions that, whatever n may be,

$$[a+\mathbf{O}]^n=na+\mathbf{O} \quad\text{and}\quad \left[\frac{\mathbf{O}}{1-\mathbf{O}}\right]^n=\frac{\mathbf{O}}{1-n\mathbf{O}} \qquad 2·3$$

If Φ and ϕ are two operations $B\mathbf{O}+C\mathbf{O}^2+D\mathbf{O}^3+\ldots$ and $b\mathbf{O}+c\mathbf{O}^2+d\mathbf{O}^3+\ldots$, then by substitutions or by Maclaurin's Theorem,

$$\begin{aligned}\Phi\phi\equiv[B\mathbf{O}+C\mathbf{O}^2+D\mathbf{O}^3+\ldots][b\mathbf{O}+c\mathbf{O}^2+d\mathbf{O}^3+\ldots]=Bb\mathbf{O}+\{Cb^2+Bc\}\mathbf{O}^2+\\ +\{Db^3+2Cbc+Bd\}\mathbf{O}^3+\{Eb^4+3Db^2c+C(2bd+c^2)+Be\}\mathbf{O}^4+\{Fb^5+4Ebc+\\ +3D(b^2d+bc^2)+2C(be+cd)+Bf\}\mathbf{O}^5+\{Gb^6+5Fb^4c+2E(2b^3d+3b^2c^2)+\\ +D(3b^2e+6bcd+c^3)+C(2bf+2ce+d^2)+Bg\}\mathbf{O}^6+\{Hb^7+6Gb^5c+\\ +5F(b^4d+2b^3c^2)+4E(b^3e+3b^2cd+bc^3)+3D(b^2f+2bce+bd^2+c^2d)+\\ +2C(bg+cf+de)+Bh\}\mathbf{O}^7+\text{etc.}]\end{aligned} \qquad 2·4$$

The analogy between this "operative multiplication" and ordinary algebraic multiplication of polynomials may be examined by the reader. It will be observed on trial that the expansion is much more cumbersome if the subject operation ϕ has an absolute term a.

3·0. We now proceed to find formulæ for the operative involution of multinomials. The first case is easy.

$$[a+b\mathbf{O}]^2=a+b(a+b\mathbf{O});$$
$$[a+b\mathbf{O}]^n=a+ab+ab^2+\ldots+ab^{n-1}+b^n\mathbf{O}=a(1-b^n)/(1-b)+b^n\mathbf{O}; \qquad 3·1$$

which holds for all real numerical values of n.

Attempting similar substitutions for the general case,

$$\begin{aligned}[a+b\mathbf{O}+c\mathbf{O}^2+d\mathbf{O}^3\ldots]^2=\{a+ba+ca^2+da^3\ldots\}+\{b^2+2cab+3da^2b\ldots\}\mathbf{O}+\{bc+\\ +c(b^2+2ac)+d(3ab^2+3a^2c)\ldots\}\mathbf{O}^2+\{c(2bc=2ad)+bd\ldots\}\mathbf{O}^3+\{c^3\ldots\}\mathbf{O}^4+\text{etc.};\\ [a+b\mathbf{O}+c\mathbf{O}^2+d\mathbf{O}^3\ldots]^3=\{1+b+b^2\}a+\{1+3b+b^2\}ca^2+\{(1+b)2c^2+(1+4b+3b^2+\\ +b^3)d\}a^3+\{(1+4b)e+(5+6b)cd+c^3\}a^4+\ldots+\text{terms in }\mathbf{O}.\end{aligned} \qquad 3·2$$

This is already too involved, and becomes unworkable for higher values of n. But the problem will be solved in a certain case by another method in 6·0, and it will be shown that the first term of the expansion (free of $\mathbf{O}$) is the algebraic

expression of a "midaxial root" of the multinomial, as given by operative division, and other terms (7·0).

If, however, the multinomial has no absolute term, its iteration-formula can be obtained more easily by several methods.

The formula of 2·4 gives the operative product of two multinomials $\Phi\phi$, without absolute terms. By interchanging the capital and small letters in this formula we can write out the value of $\phi\Phi$. Now suppose that ϕ is the given multinomial of which we require the iteration formula, and assume that $\Phi = \phi^n$, its coefficients B, C, D, ... being required. Then since $\phi^n\phi = \phi\phi^n$, the expansion of $\Phi\phi$ is equivalent to that of $\phi\Phi$; and by equating the coefficients of the same powers of $\mathbf{O}$ in both, we can determine the required capital coefficients in succession.

First assume that $b = 1$. Then obviously $B = 1$ also; and we have the series of equations

$$C + c = c + C \qquad 2Cc = 2cC \qquad 3Dc + C(2d + c^2) = 3dC + c(2D + C^2), \text{ etc.}$$

The first two equations are identities, but from the third

$$cD - Cd = cC^2 - Cc^2;$$

and so on. Now it is easy to see by commencing a few substitutions that $C = nc$; so that $D = nd + n(n-1)c^2$, and E, F, ... can be similarly found. Hence, writing n, n_2, n_3, n_4, ... for the successive "binomial coefficients,"

$$\begin{aligned}\phi^n \equiv [\mathbf{O} + c\mathbf{O}^2 + d\mathbf{O}^3 \ldots]^n = \mathbf{O} + nc\mathbf{O}^2 + \{nd + 2n_2c^2\}\mathbf{O}^3 + \{ne + 5n_2cd + (n_2 + 6n_3)c^3\}\mathbf{O}^4 + \\ + \{nf + 3n_2(2ce + d^2) + (5n_2 + 26n_3)c^2d + (10n_3 + 24n_4)c^4\}\mathbf{O}^5 + \{ng + 7n_2(cf + de) + \\ + 7(n_2 + 5n_3)cd^2 + 4(2n_2 + 9n_3)c^2e + (n_2 + 71n_3 + 154n_4)c^3d + (8n_3 + 86n_4 + \\ + 120n_5)c^5\}\mathbf{O}^6 + \{nh + 4n_2(2cg + 2df + e^2) + 12(n_2 + 4n_3) + c^2f + 4(5n_2 + 23n_3)cde + \\ + 3(n_2 + 5n_3)d^3 + (3n_2 + 163n_3 + 340n_4)c^2d^2 + 4(n_2 + 31n_3 + 60n_4)c^3e + (94n_3 + \\ + 808n_4 + 1044n_5)c^4d + 4(n_3 + 43n_4 + 189n_5 + 180n_6)c^6\}\mathbf{O}^7 + \text{etc.} \qquad 3{\cdot}3\end{aligned}$$

This series has been given by me previously (5, p. 402). To establish it for all real values of n we easily show by the use of 2·4 that $\phi^m\phi^n = \phi^{m+n}$; and the remainder of the proof follows that of Euler for the Binomial Theorem. Also $[\phi^m]^n = \phi^{mn}$. *These results show that permanence of form exists for operative involution as it exists for algebraic involution.* For verifications, we find that when $n = 1$, $\phi^1 = \phi$, and that when $n = 0$, $\phi^0 = \mathbf{O}$, its correct value (see 6, p. 593, and 1). Also the homographic result of 2·3 is verified by the result

$$\left[\frac{\mathbf{O}}{1 - \mathbf{O}}\right]^n = [\mathbf{O} + \mathbf{O}^2 + \mathbf{O}^3 \ldots]^n = \mathbf{O} + n\mathbf{O}^2 + n^2\mathbf{O}^3 \ldots = \frac{\mathbf{O}}{1 - n\mathbf{O}}. \qquad 3{\cdot}4$$

If $n = -1$, we obtain the invert of ϕ—that is, ϕ^{-1}; namely

$$\begin{aligned}\phi^{-1} \equiv [\mathbf{O} + c\mathbf{O}^2 + d\mathbf{O}^3 \ldots]^{-1} = \mathbf{O} - c\mathbf{O}^2 - \{d - 2c^2\}\mathbf{O}^3 - \{e - 5cd + 5c^3\}\mathbf{O}^4 - \{f - \\ - 3(2ce + d^2) + 21c^2d - 14c^4\}\mathbf{O}^5 - \{g - 7(cf + de) + 28(cd^2 + c^2e) - 84c^3d + 42c^5\}\mathbf{O}^6 - \\ - \{h - 4(2cg + 2df + e^2) + 12(3c^2f + 6cde + d^3) - 60(3e^2d^2 + 2c^3e) + 330c^4d - \\ - 132c^6\}\mathbf{O}^7 - \text{etc.} \qquad 3{\cdot}5\end{aligned}$$

This is one of the inverts obtained by operative division (see 4, 5, 6, especially 4, p. 230) and can always be used for calculating roots of equations, since if $\phi x = y$, $x = \phi^{-1}y$; but we must often shift the origin to a point near the required root (4, p. 231). This particular series is also known from Lagrange's and Burmann's Theorems. It is not the complete invert of ϕ.

If n is a positive integer, the series for ϕ^n ceases (owing to the exhaustion of he binomial coefficients) at the term $\mathbf{O}^{m^n}$, if $\mathbf{O}^m$ is the highest power in ϕ.

Examples.

$[\mathbf{O}+c\mathbf{O}^2]^n=\mathbf{O}+nc\mathbf{O}^2+2n_2c^2\mathbf{O}^3+n_2(2n-3)c^3\mathbf{O}^4+2n_3(3n-4)c^4\mathbf{O}^5+\text{etc.}$

$[\log(1+\mathbf{O})]^n=\mathbf{O}-\frac{1}{2}n\mathbf{O}^2+\frac{1}{2}n(3n+1)\mathbf{O}^3/3\,!-\frac{1}{2}n(3n+1)(2n+1)\mathbf{O}^4/4\,!+\frac{1}{6}n(45n^3+$
$\quad+65n^2+30n+4)\mathbf{O}^5/5\,!$ etc.

$[\epsilon^{\mathbf{O}}-1]^n=\mathbf{O}+\frac{1}{2}n\mathbf{O}^2+\frac{1}{2}n(3n-1)\mathbf{O}^3/3\,!+\frac{1}{2}n(3n-1)(2n-1)\mathbf{O}^4/4\,!+\frac{1}{6}n(45n^3-65n^2+$
$\quad+30n-4)\mathbf{O}^5/5\,!+\text{etc.}$

$[\mathbf{O}\epsilon^{\mathbf{O}}]^n=\mathbf{O}+n\mathbf{O}^2+n(2n+1)\mathbf{O}^3/2\,!+n(12n^2-15n+4)\mathbf{O}^4/3\,!+\text{etc.}$

$[\epsilon^{\mathbf{O}}\log(1+\mathbf{O})]^n=\mathbf{O}+\frac{1}{2}n\mathbf{O}^2+n(3n-1)\mathbf{O}^3/3\,!+\frac{1}{2}n(n-1)(6n+11)\mathbf{O}^4/4\,!+n(45n^3+$
$\quad+65n^2-240n+184)\mathbf{O}^5/6\,!+\text{etc.}$

$[\mathbf{O}\cos\mathbf{O}]^n=\mathbf{O}-n\mathbf{O}^3/2\,!+n(9n-8)\mathbf{O}^5/4\,!-n(15n-16)^2\mathbf{O}^7/6\,!+\text{etc.}$

$[\log(1+\mathbf{O})]^{12}(0{\cdot}01)=0{\cdot}009433\ldots \qquad [\epsilon^{\mathbf{O}}-1]^{12}(0{\cdot}01)=0{\cdot}010638\ldots$ 3·6

The iteration formulæ for $\sin\mathbf{O}$, $\tan\mathbf{O}$, $\sin^{-1}\mathbf{O}$, $\tan^{-1}\mathbf{O}$, are already known (Wolstenholme's *Mathematical Problems*, 3rd ed. 1891, Nos. 620, 621).

4·0. If the absolute term of the proposed multinomial be wanting, but the coefficient b of the first term $\mathbf{O}$ be not unity, we may proceed as before in the case of 3·3, by equating coefficients in the values for $\Phi\phi$ and $\phi\Phi$, giving

$$Bb=bB, \qquad Cb^2+Bc=cB^2+bC,$$
$$Db^3+2Cbc+Bd=dB^3+2cBC+bD, \quad \text{etc.}$$

Hence
$$C(b^2-b)=c(B^2-B),$$
$$D(b^3-b)(b^2-b)=2c^2(B^2-B)(B-b)+d(B^3-B)(b^2-b),$$

and so on. On commencing a few substitutions for the values of ϕ, ϕ^2, . . ., we shall easily see that $B=b^n$; so that the sub-coefficients of c, d, e, . . . in the expansion of $\phi^n(\equiv\Phi)$ are composed of the sums of n-terms of geometric progressions of various powers of b. Writing for brevity

$$G_r\equiv(b^{rn}-1)/(b^r-1) \text{ and } K_{rs}\equiv(G_r-G_s)/(b^{r-s}-1),$$

we have

$$[b\mathbf{O}+c\mathbf{O}^2+d\mathbf{O}^3\ .\ .\ .]^n=b^n\mathbf{O}+G_1c/b\,.\,b^n\mathbf{O}^2+\{G_2d/b+2K_{21}c^2/b^2\}b^n\mathbf{O}^3+\{G_3e/b+$$
$$+(3K_{32}+2K_{31})cd/b^2+(4K_{31}+K_{32}-5K_{21})c^3/b^3(b-1)\}b^n\mathbf{O}^4+\{G_4f/b+(4K_{43}+$$
$$+2K_{41})ec/b^2+3K_{42}d^2/b^2+((3b+11)K_{43}+(6b+10)K_{41}-(9b+9)K_{32}-$$
$$-12K_{21})c^2d/b^3(b^2-1)+(4K_{43}+(2b+10)K_{41}-(4b+4)K_{32}-(12b+12)K_{31}+$$
$$+(14b+2)K_{21})c^4/b^4(b-1)(b^2-1)\}b^n\mathbf{O}^5+\text{etc.} \qquad 4{\cdot}1$$

I am not aware that this series has been given before. It can be recast in several ways, which I have no space for (see however 10·0); but the following theorems are useful for this purpose:

$$G_r=(B-1)(1+B+B^2\ldots B^{r-1})/(b-1)(1+b+b^2\ldots b^{r-1});$$
$$K_{rs}=b^s+(1+b^{r-s})b^{2s}+(1+b^{r-s}+b^{2(r-s)})b^{3s}\ldots(1+b^{r-s}\ldots b^{(n-2)(r-s)})b^{(n-1)s}. \qquad 4{\cdot}2$$

Successive substitutions show that when n is positive, integral, and finite the coefficients in the expansion of ϕ^n must also be finite and integral. Hence, not only must G_r and K_{rs} be so (as just shown), but all the functions of K within small brackets in 4·1 must be divisible by $b-1$, b^2-1, . . . where these denominators occur.

When $n=0$, then $G_r=0$ and $\phi^0=\mathbf{O}$. When $n=1$, $G_r=G_s$ and $\phi^n=\phi$.

If $b\to 1$, $G_r\to n$; and by taking the limits of the coefficients in 4·1, we shall find that the whole series reduces to that of 3·3, as it should do. It is interesting

to observe how the functions of geometrical progressions in the former become degraded to binomial coefficients in the latter.

If $n = -1$, $G_r = -b^{-r}$ and $K_r = -b^{-r}$; and ϕ^{-1} gives the same series as that of 3·5, except that $\mathbf{O}$ and $c, d, e, \ldots$ are now all divided by b. If $n \to \infty$ and $|b| < 1$, $G_r = 1/(1-b^r)$ and $K_r = b^r/(1-b^r)(1-b^r)$.

Examples.

$$[b\mathbf{O} + c\mathbf{O}^2 + d\mathbf{O}^3 \ldots]^3 = b^3\mathbf{O} + \{1 + b + b^2\}b^2c\mathbf{O}^2 + \{(1 + b^2 + b^4)b^2d + (1+b+b^2)2b^2c^2\}\mathbf{O}^3 + \text{etc.}$$

$$\left[\frac{b\mathbf{O}}{1-\mathbf{O}}\right]^n = b^n\left[\frac{\mathbf{O}}{1-\mathbf{O}}\right]^{n_1}$$

$$[b\mathbf{O}+c\mathbf{O}^2]^n = B\mathbf{O} + B\frac{B-1}{b-1}\frac{c}{b}\mathbf{O}^2 + 2B\frac{B-1}{b-1}\frac{B-b}{b^2-1}\frac{c^2}{b^2}\mathbf{O}^3 + \text{etc.} \quad (B = b^n) \qquad 4·3$$

This last example is the solution of the difference-equation $x_n = bx_{n-1} + cx_{n-1}^2$ which denotes the time-to-time variation of malaria in a locality—a problem which led to part of the studies outlined here (see my *Prevention of Malaria in Mauritius*, Waterlow & Sons, 1908, and *Prevention of Malaria*, Murray, 1911).

5·0. The analogy between the algebraic and the operative multinomial theorems, that is between the expansions of $(\phi)^n$ and of $[\phi]^n$, or ϕ^n, respectively, is best shown by evolving both by the same method—which requires only a knowledge of Pascal's Arithmetical Triangle, according to which the binomial coefficient n_r is the sum of the first $n-r+1$ terms of the rth order of figurate numbers. Let

$$(\phi)^n \equiv (\mathbf{O}+c\mathbf{O}^2+d\mathbf{O}^3 \ldots)^n = \mathbf{O}^n + c_n\mathbf{O}^{n+1} + d_n\mathbf{O}^{n+2} + \text{etc.};$$
$$\phi^n \equiv [\mathbf{O}+c\mathbf{O}^2+d\mathbf{O}^3 \ldots]^n = \mathbf{O} + C_n\mathbf{O}^2 + D_n\mathbf{O}^3 + \text{etc.}$$

First, to find $c_n, d_n, e_n, \ldots$, we have $(\phi)^n = (\phi)^{n-1} \cdot \phi$; and multiplying algebraically the two series $(\phi)^{n-1}$ and ϕ, and equating their coefficients with those of $(\phi)^n$, we obtain

$$c_n = c + c_{n-1}; \qquad d_n = d + cc_{n-1} + d_{n-1}; \qquad e_n = e + dc_{n-1} + cd_{n-1} + e_{n-1}; \text{ etc.}$$

Now on adding, for example, $e + e_2 + \ldots e_{n-1} + e_n$, we find that a number of terms cancel out, leaving, for example,

$$e_n = ne + d(c + c_2 \ldots c_{n-2} + c_{n-1}) + c(d + d_2 \ldots d_{n-2} + d_{n-1});$$

with similar results for c_n, d_n, f_n, g_n, etc. Hence in succession,

$$c_n = nc$$
$$d_n = nd + c(c + c_2 \ldots c_{n-1}) = nd + n_2c^2$$
$$e_n = ne + n_2cd + c(d + d_2 \ldots d_{n-1}) = ne + n_2cd + (n_2d + n_3c^2)c; \text{ etc.}$$

Hence $(\phi)^n = \mathbf{O}^n + nc\mathbf{O}^{n+1} + \{nd + n_2c^2\}\mathbf{O}^{n+2} + \{ne + 2n_2cd + n_3c^3\}\mathbf{O}^{n+3} + \{nf + n_2(2ce + d^2) + 3n_3c^2d + n_4c^4\}\mathbf{O}^{n+4} + \text{etc.}$ 5·1

This is the algebraic multinomial theorem. Similarly to find the operative multinomial formula we equate the coefficients of ϕ^n with those of $\phi^{n-1}\phi$, since

$$\mathbf{O} + C_n\mathbf{O}^2 + D_n\mathbf{O}^3 \ldots = (\mathbf{O} + c_1\mathbf{O}^2 + d_1\mathbf{O}^3 \ldots) + C_{n-1}(\mathbf{O}^2 + c_2\mathbf{O}^3 + d_2\mathbf{O}^4 \ldots) + D_{n-1}(\mathbf{O}^3 + c_3\mathbf{O}^4 + d_3\mathbf{O}^5 \ldots) + \text{etc.} = \mathbf{O} + (c_1 + C_{n-1}) + (d_1 + c_2C_{n-1} + D_{n-1})\mathbf{O}^3 + (e_1 + d_2C_{n-1} + c_3D_{n-1} + E_{n-1})\mathbf{O}^4 + \text{etc.};$$

so that the coefficients are the same as those of $(\phi)^n$ except that subscripts are attached to the small letters. Finding ΣC_n, ΣD_n, ... in succession as before, we obtain $C_n = nc$, $D_n = nd + n_2c_1c_2$, etc.; and have

$$[\phi]^n = \mathbf{O} + nc_1\mathbf{O}^2 + \{nd_1 + n_2c_1c_2\}\mathbf{O}^3 + \{ne_1 + n_2(c_1d_2 + c_3d_1) + n_3c_1c_2c_3\}\mathbf{O}^4 + \{nf_1 + n_2(c_1e_2 + d_1d_3 + c_4e_1) + n_3(c_1c_2d_3 + c_1c_4d_2 + c_3c_4d_1) + n_4c_1c_2c_3c_4\}\mathbf{O}^5 + \text{etc.} \qquad 5·2$$

If in this we substitute the values of $c_2, c_3 \ldots, d_2, a_3 \ldots, e_2, e_3 \ldots$ obtained in 5·1, we return to the expansion of ϕ^n given in 3·3. If we suppress the subscripts, we return to 5·1. The same groups of letters occur in both expansions.

Another method for establishing both theorems is by use of the primitive identity often employed in the Finite Calculus. If $a_0, a_1, a_2, a_3 \ldots$ are any quantities, functions, or operations capable of addition and subtraction, then

$$a_1 = a_0 + (a_1 - a_0);$$
$$a_2 = a_0 + 2(a_1 - a_0) + (a_2 - 2a_1 + a_0);$$
$$a_3 = a_0 + 3(a_1 - a_0) + 3(a_2 - 2a_1 + a_0) + (a_3 - 3a_2 + 3a_1 - a_0);$$
$$a_n = a_0 + n(a_1 - a_0) + n_2(a_2 - 2a + a_0) + n_3(a_3 - 3a_2 + 3a_1 - a_0) + \text{etc.}$$

Hence if $\phi^0(=\mathbf{O}), \phi, \phi^2, \ldots \phi^n$ be the given series,

$$\phi^n = \mathbf{O} + n(\phi - \mathbf{O}) + n_2(\phi^2 - 2\phi + \mathbf{O}) + n_3(\phi^3 - 3\phi^2 + 3\phi - \mathbf{O}) + \text{etc.} \qquad 5·3$$

The values of the expressions within brackets containing $\phi^2, \phi^3, \ldots$ are worked out by substitutions or by the use of the formula 2·4, and the coefficients of the successive powers of $\mathbf{O}$ are collected. The result is the formula 3·3. Or we may let the series stand as it is in successive values of $n_1, n_2, n_3, \ldots$ If $\phi^n \equiv [(a + bx + cx^2 \ldots)\mathbf{O}]^n$, the series gives the expansion of $(a + bx + cx^2 \ldots)^n$, that is, the algebraic multinomial theorem. See end of 7·0.

6·0. In 3·2 it was found impossible to obtain the iteration-formula for $a + b\mathbf{O} + c\mathbf{O}^2 + \ldots$, that is, for an operation of a degree higher than one with an absolute term, by means of successive substitutions. We now obtain it by the means of the elementary operative theorem, first used, I believe, by Babbage in his work on periodic functions, namely that if $\phi = \zeta\xi\zeta^{-1}$, then

$$\phi^2 = \zeta\xi\zeta^{-1}\zeta\xi\zeta^{-1} = \zeta\xi^2\zeta^{-1},$$

and

$$\phi^n = \zeta\xi^n\zeta^{-1}. \qquad 6·1$$

First let $\zeta = p + \mathbf{O}$, so that $\zeta^{-1} = \mathbf{O} - p$.* Then if

$$\phi = [p + \mathbf{O}][\xi][\mathbf{O} - p],$$
$$\xi = [\mathbf{O} - p][\phi][p + \mathbf{O}] = \phi(p + \mathbf{O}) - p,$$

and

$$\phi^n = [p + \mathbf{O}][\phi(p + \mathbf{O}) - p]^n[\mathbf{O} - p], \qquad 6·2$$

as an identity. Now let p be, if possible, a number m such that $\phi m = m$. Then if ϕ be any operation which can be expanded in ascending powers of $\mathbf{O}$, the nuclear operation ξ, that is $\phi(m + \mathbf{O}) - \phi m$, will contain no absolute term, and may therefore be iterated by 2·1, 3·3, or 4·1; so that

$$[a + b\mathbf{O} + c\mathbf{O}^2 + \ldots]^n = m + [\phi' m \,.\, \mathbf{O} + \tfrac{1}{2}\phi'' m \,.\, \mathbf{O}^2 + \tfrac{1}{6}\phi''' m \,.\, \mathbf{O}^3 + \ldots]^n(\mathbf{O} - m). \quad 6·3$$

Suppose, for a simple example, that we require to iterate $12 - 6x + x^2$ upon itself $n - 1$ times. Then m must be one of the roots of $12 - 6x + x^2 = x$, that is, must be either 3 or 4. Thus

$$\phi^n x = 3 + [\mathbf{O}^2]^n(x - 3) = 3 + [\mathbf{O}^{2^n}](x - 3) = 3 + (x - 3)^{2^n}; \qquad \text{(by 2·1)}$$

or

$$\phi^n x = 4 + [2\mathbf{O} + \mathbf{O}^2]^n(x - 4)$$
$$= 4 + [2^n\mathbf{O} + \tfrac{1}{2}2^n(2^n - 1)\mathbf{O}^2 + \tfrac{1}{6}2^n(2^n - 1)(2^n - 2)\mathbf{O}^3 + \ldots](x - 4) \qquad \text{(by 4·1)}$$
$$= 3 + [(1 + \mathbf{O})^{2^n}](x - 4) = 3 + (x - 3)^{2^n},$$

as before.

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* To invert simple operations proceed as follows. Suppose $\phi = \dfrac{a + b\mathbf{O}}{c + d\mathbf{O}}$. Operate with both sides on ϕ^{-1}. Then $\mathbf{O} = \dfrac{a + b\phi^{-1}}{c + d\phi^{-1}}$. Now solve algebraically and we get $\phi^{-1} = \dfrac{-a + c\mathbf{O}}{b - d\mathbf{O}}$. Or we may first solve algebraically and then operate on ϕ^{-1}.

What is the relation between 6·3 and the incomplete result of 3·2? On effecting the iteration indicated in 6·2, we have by 4·1

$$\phi^n = m + (\phi' m)^n . (\mathbf{O} - m) + \frac{(\phi' m)^n - 1}{\phi' m - 1} \frac{\phi'' m}{2} (\phi' m)^{n-1} . (\mathbf{O} - m)^2 + \text{etc.}$$

$$= m - (\phi' m)^n m + \frac{(\phi' m)^n - 1}{\phi' m - 1} \frac{\phi'' m}{2} (\phi' m)^{n-1} m^2 + \text{etc} + \text{terms in } \mathbf{O}. \qquad 6·4$$

Now in 3·2 we saw that ϕ^3, as we attempted to evolve it by substitutions, gives a cumbersome series consisting only of the constants $a, b, c, \ldots$ followed by unascertained terms in ascending powers of $\mathbf{O}$. This cumbersome series, then, must be the same as the terms in m indicated above. To show their identity we must therefore find an algebraic expression for m from the equation $\phi m = m$. We proceed as follows:

$$a + bm + cm^2 + \ldots = m,$$

$$[a + (b-1)\mathbf{O} + c\mathbf{O}^2 + \ldots]m = 0,$$

$$m = [a + (b-1)\mathbf{O} + c\mathbf{O}^2 + \ldots]^{-1}(0) = [[a + \mathbf{O}][(b-1)\mathbf{O} + c\mathbf{O}^2 + \ldots]]^{-1}(0)$$

$$= [(b-1)\mathbf{O} + c\mathbf{O}^2 + \ldots]^{-1}[\mathbf{O} - a](0) \text{ (since } [\psi\chi]^{-1} = \chi^{-1}\psi^{-1})$$

$$= -(b-1)^{-1}a - (b-1)^{-3}ca^2 - \{(b-1)^{-4}d - 2(b-1)^{-5}c^2\}(b-1)^{-8}a^3 - \text{etc.}, \qquad 6·5$$

by putting $n = -1$ in 4·1, and remembering that $[\mathbf{O} - a](0) = -a$. The same value of m can be obtained by "operative division" (see my papers 1 and 4). By substituting it in 6·3 we repeat the series commenced in 3·2.

7·0. A word now on the geometrical interpretation of the above. As suggested in my paper (4), the Cartesian expression of curves may be abbreviated by denoting them by operations alone, apart from the subjects—a thing which the present notation allows us to do.* Thus we express the curve $y = \phi x$ by ϕ alone; $a + b\mathbf{O}$ is the straight line; and $\mathbf{O}$ is the midaxis, usually written $y = x$. Obviously m denotes an intersection of ϕ and $\mathbf{O}$, a point of which the co-ordinates are $y = m$, $x = m$, where m is a real root of $\phi m - m = 0$, that is, $m = [\phi - \mathbf{O}]^{-1}(0)$. I call m a *midaxial root* of ϕ, as distinct from its *axial roots.*

The curve $[p + \mathbf{O}]\xi$ or $p + \xi$ is a curve similar to ξ but translated upward to a distance p along the $\infty\mathbf{O}$, or y, axis; and the curve $\xi[\mathbf{O} - q]$, or $\xi(\mathbf{O} - q)$, is one similar to ξ but translated rightward to a distance q along the $0\mathbf{O}$, or x, axis; or, what is the same thing, these expressions may denote the transference of the origin by the same distances reversed. Hence $[m + \mathbf{O}]\xi[\mathbf{O} - m]$ is the curve ξ shifted to a distance m both upward and rightward, simultaneously, without rotation.

The curve $b\mathbf{O} + c\mathbf{O}^2 + \ldots$ passes through the origin, where it has the tangential slope b; and it possesses a midaxial root $y = 0$, $x = 0$, and can be iterated by 4·1. The curve $\phi = a + b\mathbf{O} + c\mathbf{O}^2 + \ldots$ does not pass through the origin; and in order to iterate it by the method of 6·2, we find a midaxial root of it (if it possesses one), namely $y = m$, $x = m$, and then we find a curve ξ which is the same as ϕ but translated so as to pass through the origin and be iterable by 4·1. To do this, we have by 6·2, $\xi = [m + \mathbf{O}]^{-1}\phi[m + \mathbf{O}]$. Then after iterating ξ we pass back to ϕ^n by the retransformation indicated in 6·2, that is,

$$\phi^n = [m + \mathbf{O}]\xi^n[m + \mathbf{O}]^{-1}.$$

Of course there are many curves (*e.g.* $\epsilon^{\mathbf{O}}$) which have no real midaxial roots at all. Others have more than one real midaxial root.

If we draw the graph of ϕ and ψ, it is easy to draw graphs of $\phi\psi$, and therefore of $\phi^2, \phi^3, \ldots$ and of ϕ^{-1}, ψ^{-1}, etc. It is also easy to make graphs of the iterations denoted by $\phi^2 x, \phi^3 x, \ldots \phi^n x$, and to show how these iterations may under certain conditions approximate to alternate midaxial roots of ϕ denoted by

* Necessarily an inadequate description.

$[\phi - \mathbf{o}]^{-1}(o)$; see especially my papers (4, 5, 6). The geometric interpretation of the theorem of 5·3 can easily be given by such graphs, and the question of convergence frequently decided

8·0. Expansions of ϕ^n by Maclaurin's Theorem are generally intractable owing to the cumbersome expressions of the successive tangentials ; but the expansion of $\phi^n = \phi^n\{m+(\mathbf{o}-m)\}$ by Taylor's Theorem is rendered easy by the simplification due to the definition $m=\phi m$, so that also $\phi^2 m, \phi^3 m, \ldots \phi^n m = m$. Hence

$$\begin{aligned}[D\phi^n]m &= [D(\phi\phi^{n-1})]m = [\phi'\phi^{n-1}\,.\,D\phi^{n-1}]m \quad (\text{where } D \equiv d/d\mathbf{o})\\ &= [\phi'\phi^{n-1}\,.\,\phi'\phi^{n-2}\;\;\phi'\phi^{n-3}\ldots\,\phi'\phi\,.\,\phi']m\\ &= (\phi' m)^n;\end{aligned} \qquad 8\cdot 1$$

since a subject is distributive throughout an algebraic product of operators. Next, taking logarithms of $D\phi^n = \phi'\phi^{n-1}\,.\,\phi'\phi^{n-2}$. etc., and differentiating again,

$$\begin{aligned}D^2\phi^n &= D\phi^n.\left\{\frac{D(\phi\,\phi^{n-1})}{\phi'\phi^{n-1}} + \frac{D\,\phi'\phi^{n-2})}{\phi'\phi^{n-2}} + \ldots + \frac{D(\phi'\phi^0)}{\phi'\phi^0}\right\} = D\phi^n.\left\{\frac{\phi''\phi^{n-1}}{\phi'\phi^{n-1}}D\phi^{n-1} + \text{etc.}\right\}\\ &= \phi''\phi^{n-1}\,.\,(D\phi^{n-1})^2 + \phi'\phi^{n-1}\,.\,\phi''\phi^{n-2}\,.\,(D\phi^{n-2})^2 + \phi'\phi^{n-1}\,.\,\phi'\phi^{n-2}\,.\,\phi''\phi^{n-3}\,.\\ &\quad (D\phi^{n-3})^2 + \text{etc.}\end{aligned}$$

Thus $[D^2\phi^n]m = \phi'' m\,.\,(\phi' m)^{2n-2}\,.\,\{1 + (\phi' m)^{-1} + (\phi' m)^{-2} + \ldots + (\phi' m)^{-n+1}\}$

$$= \phi'' m\,.\,(\phi' m)^{n-1}\,.\,\frac{(\phi' m)^n - 1}{\phi' m - 1}. \qquad 8\cdot 2$$

Hence $\phi^n = \phi^n\{m + (\mathbf{o} - m)\} = m + (\phi' m)^n\,.\,(\mathbf{o} - m) + \text{etc.}$, as in 6·4. 8·3

9·0. It is a very familiar proposition that if $\phi^n x$ approaches a limit when n is indefinitely increased that limit is a root of $\phi x = x$, in other words, a midaxial root of ϕx. But how is it that ϕ^n, which should be an operation, becomes a number? The fundamental rules of this approximation are obvious from graphs, and are :

9·1. $\phi^n x$ ultimately converges to m if $|\,\phi' m\,| < 1$. If $\phi' m > 0$, the iterants $\phi^2 x, \phi^3 x \ldots$ progressively increase or decrease. If $\phi' m < 0$, they are alternately greater or less than m. Otherwise they recede from m, "stagnate" round it, or diverge altogether.

9·2. If $\phi x > x$, the iterants tend to converge to a midaxial root greater than x, or to increase without limit. If $\phi x < x$, they tend to converge to a midaxial root less than x, or to decrease without limit.

9·3. If the conditions hold good, the iteration will converge towards m whatever value, within certain ranges, the *base* of the iteration, x, may have.

9·4. If ϕ is a continuous one-branched curve with a series of midaxial roots $\ldots m_{-2}, m_{-1}, m_0, m_1, m_2, \ldots$, numbered in ascending order of magnitude and m_0 being the least positive root, then $\phi^n x$ can converge only to the even roots m_{-2}, $m_0, m_2, \ldots$ if $\phi(o) > o$, and to the odd roots if $\phi(o) < o$. In both these cases obviously $\phi' m \leqq 1$ at each of the appropriate roots, but also by 9·1, we must have $\phi' m > -1$ for convergency.

9·5. Which particular midaxial root, if any, is approached depends upon the figure of the curve and the position of the base x ; but generally $\phi^n x$ approaches m_r if $m_{r-1} < x < m_{r+1}$.

By 8·1, $[D\phi^n]m = (D\phi m)^n$. Hence if $|\,D\phi m\,| < 1$ as required by 9·1 for successful convergence, $[D\phi^n]m$ vanishes when n is indefinitely increased ; and the same thing happens to the succeeding tangentials. Thus in 6·3 and 8·3, all the terms in the expansion of $\phi^n x$ except the first ultimately disappear, leaving only $\phi^n x = m$. Or, to put it slightly differently, all the terms except the first in the expansion of $\phi^n(m + x)$ vanish, so that this function becomes in the proximity of m

a straight line parallel to the axis of x and at a distance m from it. For a somewhat different proof, let $x_0, x_1, x_2, \ldots$ denote the iterants $x, \phi x, \phi^2 x, \ldots$. Then $D\phi^2 x_0 = [\phi'\phi \cdot \phi']x_0 = \phi' x_1 \cdot \phi' x_0$, and $D\phi^n x_0 = \phi' x_0 \cdot \phi' x_1, \phi' x_2 \ldots \phi' x_{n-1}$. Hence if $|\phi' x| < 1$ for the whole region containing these iterants, or even at the point of condensation m, then $D\phi^\infty x_0$ vanishes, that is, the slope of the curve ϕ^∞ is zero within that region. The same thing may occur at all the midaxial roots where the necessary conditions hold in which case ϕ^∞ becomes a series of straight lines parallel to x (see 5, p. 411). A good example is $\phi = \mathbf{O} + \mathbf{O}(\mathbf{O}-1)(\mathbf{O}-2)\ldots(\mathbf{O}-r+1)/r$.

10·0. This concludes most of the matter proposed for this paper; but some further examples of iteration will be useful.

(1) The following method gives the result of 4·1 as a Dirichlet series:

Let $\phi^n = [b\mathbf{O} + c\mathbf{O}^2 + d\mathbf{O}^3 \ldots]^n = Pb^n + Qb^{2n} + Rb^{3n} + \text{etc.}$,

where $P, Q, R, \ldots$ contain $\mathbf{O}$ but not n. Then

$$\phi^{n+1} = Pbb^n + Qb^2b^{2n} + Rb^3b^{3n} + \text{etc.}$$

But also $\phi^{n+1} = \phi\phi^n = b(Pb^n + Qb^{2n}\ldots) + c(Pb^n + Qb^{2n}\ldots)^2 + \text{etc.}$

$$= bPb^n + (bQ + cP^2)b^{2n} + (bR + 2cPQ + dP^3)b^{3n} + \text{etc.}$$

By equating coefficients,

$$Q = \frac{c}{b^2 - b}P^2 \qquad R = \frac{2c^2 + (b-1)bd}{(b^3-b)(b^2-b)}P^3 \quad \text{etc.}$$

Hence $Q, R, S, \ldots$ may be written $qP^2, rP^3, sP^4, \ldots$ where $q, r, s, \ldots$ are composed of $b, c, d, \ldots$ but contain neither P, $\mathbf{O}$, nor n. Now to find P,

$$\phi^n = Pb^n + qP^2b^{2n} + rP^3b^{3n} + \text{etc.} = [\mathbf{O} + q\mathbf{O}^2 + r\mathbf{O}^3 \ldots]b^nP;$$

$$\phi^0 = [\mathbf{O} + q\mathbf{O}^2 + r\mathbf{O}^3 \ldots]P = \mathbf{O};$$

and $$P = [\mathbf{O} + q\mathbf{O}^2 + r\mathbf{O}^3 \ldots]^{-1};$$

the invert being given by 3·5. Substituting these values of $P, q, r, s, \ldots$ we obtain the required expansion (connecting Dirichlet's series with iteration). 10·1

(2) We may now write ϕ^n also in the form

$$\phi^n = [\mathbf{O} + q\mathbf{O}^2 + r\mathbf{O}^3 \ldots][b\mathbf{O}]^n[\mathbf{O} - q\mathbf{O}^2 - (r - 2q^2)\mathbf{O}^3 - \text{etc.}],$$

which is simply the $\zeta\xi\zeta^{-1}$ form given in 6·1. Or we may assume this form from the first. Expanding by substitutions or by 2·4, and first putting $n = 1$, we have

$$\phi = b\mathbf{O} + c\mathbf{O}^2 + d\mathbf{O}^3 \ldots = b\mathbf{O} + (b^2 - b)q\mathbf{O}^2 + \{(b^3 - b)r - 2(b^2 - b)q^2\}\mathbf{O}^3 +$$
$$+ \{(b^4 - b)s - (3b^3 - 3b + 2b^2 - 2b)qr + 5(b^2 - b)q^3\}\mathbf{O}^4 + \text{etc.} \qquad 10·2$$

By equating coefficients (or by the previous theorem) we find $q, r, s, \ldots$, and place them in the series without disturbing the functions of b already there. Then in order to find ϕ^n we have only to put b^n for b in these last-named functions.

(3) An elegant way to put a power series in the form $\zeta\xi\zeta^{-1}$ is to assume the form of $\zeta\xi$ as a power series, and then to divide it operatively by ζ as shown in my papers (1, 4, 5). Suppose we wish to iterate $b\mathbf{O} + c + d\mathbf{O}^{-1} \ldots$ Assume that it $= [\mathbf{O} + p + q\mathbf{O}^{-1} \ldots][b\mathbf{O}][\mathbf{O} + p + q\mathbf{O}^{-1} \ldots]^{-1}$. Then since $\zeta\xi\zeta^{-1} = \zeta\xi // \zeta$, we divide operatively as follows:

$$\mathbf{O} + p + q\mathbf{O}^{-1} \ldots]\, b\mathbf{O} + p + qb^{-1}\mathbf{O}^{-1} + rb^{-2}\mathbf{O}^{-2} \ldots [b\mathbf{O} + p(1-b)\mathbf{O}^0 + q(b^{-1} - b)\mathbf{O}^{-1} +$$
$$\underline{b\mathbf{O} + pb + qb\mathbf{O}^{-1} + rb\mathbf{O}^{-2} \ldots} \qquad + \{r(b^2 - b) + pq(b^{-1} - b)\}\mathbf{O}^{-2}$$
$$p(1-b) + q(b^{-1} - b)\mathbf{O}^{-1} + r(b^2 - b)\mathbf{O}^{-2} \ldots \qquad + \text{etc.}$$
$$\underline{p(1-b)}$$
$$q(b^{-1} - b)\mathbf{O}^{-1} + r(b^2 - b)\mathbf{O}^{-2} \ldots$$
$$\underline{q(b^{-1} - b)\mathbf{O}^{-1} - q(b^{-1} - b)p\mathbf{O}^{-2} \ldots}$$
$$\{r(b^2 - b) + pq(b^{-1} - b)\}\mathbf{O}^{-2} + \ldots$$

The quotient being the required operation $\zeta\xi\zeta^{-1}$. Here each term of the quotient *operates* on the divisor instead of being multiplied into it as in algebraic division. Now, equating coefficients, inserting values in the quotient, and then putting b^n for b in the functions already in the quotient, we obtain

$$\psi^n=[b\mathbf{O}+c+d\mathbf{O}^{-1}+e\mathbf{O}^{-2}\dots]^n=B\mathbf{O}+G_1c+G_2bd/B\,.\,\mathbf{O}^{-1}+\{G_3b^2c/B^2-$$
$$-(G_2b/B-G_1b^2/B^2)cd/(b^3-1)\}\mathbf{O}^{-2}+\text{etc.}\quad(B=b^n,\ \text{see } 4\cdot1). \qquad 10\cdot3$$

Similarly, but not by the above method,

$$[\mathbf{O}+c+d\mathbf{O}^{-1}\dots]^n=\mathbf{O}-c-d\mathbf{O}^{-1}-(e+cd)\mathbf{O}^{-2}+(f+d^2+2ce+c^2d)\mathbf{O}^{-3}+\text{etc.}$$

We can also obtain both series by the process of sections 3·0 and 4·0. But we can also derive the value of ψ^n from that of ϕ^n and *vice versa* by employing another $\zeta\xi\zeta^{-1}$ transformation in which $\zeta=\mathbf{O}^{-1}=\zeta^{-1}$. Then

$$[b\mathbf{O}+c+d\mathbf{O}^{-1}\dots]^n=[\mathbf{O}^{-1}][(b\mathbf{O}^{-1}+c+d\mathbf{O}\dots)^{-1}]^n[\mathbf{O}^{-1}]$$
$$=[\mathbf{O}^{-1}][b^{-1}\mathbf{O}-b^{-2}c\mathbf{O}^2-(b^{-2}d-b^{-3}c^2)\mathbf{O}^3\dots]^n[\mathbf{O}^{-1}].$$

This last operation can be iterated by 4·1, giving $b^{-n}\mathbf{O}-G_1$, $b^{-2n}c\mathbf{O}^2-$etc.; so that finally ψ^n obtains the value shown above. 10·4

(4) If in the first example in 3·6 we put $c=z/n$ and suppose n becomes infinite, we have

$$\left[\mathbf{O}+\frac{z}{n}\mathbf{O}^2\right]^n=\mathbf{O}+z\mathbf{O}^2+z^2\mathbf{O}^3+z^3\mathbf{O}^4+\dots$$
$$=\frac{\mathbf{O}}{1-z\mathbf{O}}=\left[\frac{\mathbf{O}}{1-\mathbf{O}}\right]^z, \qquad 10\cdot5$$

if the subject of the operations is numerically less than unity. This result appears to be the homologue of the algebraic exponential theorem, and I therefore denote $\mathbf{O}/(1-\mathbf{O})$ by η. Operative logarithms are also possible and may be denoted by the abbreviation *oplog*.

11·0. *Conclusion.* This paper does not pretend to deal with iteration in general, including vortices, etc. It only outlines a chapter in operative algebra; but such a chapter is necessary for the application of operative algebra to the Calculus, which deals with the iteration, not of constant operations, but of operations which vary at each step. I hope to publish some day my work on this and some other developments of the present theme.

12·0. *Examples.*

$$[a+b\mathbf{O}]^n=\left[\mathbf{O}+\frac{a}{1-b}\right][b\mathbf{O}]^n\left[\mathbf{O}-\frac{a}{1-b}\right]=G_1a+b^n\mathbf{O} \qquad \text{(compare 3·1 and 4·1)}$$

$$[\mathbf{O}^3-\tfrac14\mathbf{O}^5]^n=\sqrt2+[\mathbf{O}-2\sqrt2\mathbf{O}^2-4\mathbf{O}^3-\tfrac94\sqrt2\mathbf{O}^4-\tfrac14\mathbf{O}^5](\mathbf{O}-\sqrt2) \qquad 6\cdot3$$

$$[a-\sqrt{a}\cdot\mathbf{O}]^n=a-1+[\mathbf{O}+\tfrac14\mathbf{O}^2+\tfrac{3}{24}\mathbf{O}^3+\tfrac{15}{192}\mathbf{O}^4\dots]^n(\mathbf{O}-a+1) \qquad 6\cdot3$$

$$[\mathbf{O}-\cos\mathbf{O}]^n=\pi/2+[\mathbf{O}+\sin\mathbf{O}]^n(\mathbf{O}-\pi/2) \qquad 6\cdot3$$

$$[a+b\mathbf{O}^{-1}]^n=[\mathbf{O}^{-1}]\left[\frac{1}{a+b\mathbf{O}}\right]^n[\mathbf{O}^{-1}]$$

$$\left[\frac{\mathbf{O}^3}{a+b\mathbf{O}+c\mathbf{O}^2+d\mathbf{O}^3}\right]^n=[\mathbf{O}^{-1}][d+c\mathbf{O}+b\mathbf{O}^2+a\mathbf{O}^3]^n[\mathbf{O}^{-1}]$$

$$[\sqrt[m]{a+p+b(\mathbf{O}^m-p)}]^n=[\mathbf{O}^m-p]^{-1}[a+b\mathbf{O}]^n[\mathbf{O}^m-p]=\sqrt[m]{G_1a+p+b^n(\mathbf{O}^m-p)}$$

$$[(a+b\sqrt[m]{\mathbf{O}+p})^m-p]^n=[\mathbf{O}^m-p][a+b\mathbf{O}]^n[\mathbf{O}^m-p]=(G_1a+b^n\,.\,\sqrt[m]{\mathbf{O}+p})^m-p$$

$$[\sqrt{\mathbf{O}+p}]\left[\frac{\mathbf{O}}{1-\mathbf{O}/p}\right]^n[\mathbf{O}^2-p]=\left[\sqrt{\frac{p}{2-\mathbf{O}^2/p}}\right]^n=\sqrt{\frac{np-(n-1)\mathbf{O}^2}{1-n(\mathbf{O}^2-p)/p}}$$

$$\eta^n=[\mathbf{O}/n][\eta][n\mathbf{O}];\ \text{or}=[\mathbf{O}^{-1}][1+\mathbf{O}]^n[\mathbf{O}^{-1}]$$

$$[b\eta]^n=[(b-1)\eta][b\mathbf{O}]^n[(b-1)\eta]^{-1};\ \text{or}=[\mathbf{O}^{-1}]\left[\frac{1+\mathbf{O}}{b}\right]^n[\mathbf{O}^{-1}]$$
$$=b^n\,.\,\eta^{G_1}. \qquad 4\cdot3$$

$[b\eta^m]^n=[(b-1)\eta^m][b\mathbf{o}]^n[(b-1)\eta^m]^{-1}=b^n\,.\,\eta^{G_1 m}]$

$\left[\frac{b}{p+q\mathbf{o}}\right]^n=\left(\frac{b}{p}\right)^n.\,\eta^{q(b^n-p^n)/p^n(b-p)}$

$[(\eta)^m]^n=[\mathbf{o}^{-1}][(1+\mathbf{o})^m]^n[\mathbf{o}^{-1}]$

Evaluate $\left[\frac{a+b\mathbf{o}}{p+q\mathbf{o}}\right]^n$ by several methods.

Show that $[\zeta\xi]^n=\zeta[\xi\zeta]^n\zeta^{-1}=\xi^{-1}[\xi\zeta]^n\xi$ and that $[\phi\psi\chi]^n\phi\psi=\phi[\psi\chi\phi]^n\psi=\phi\psi[\chi\phi\psi]^n$

$[\phi(a+b\mathbf{o})]^n=\left[\frac{\mathbf{o}-a}{b}\right][a+b\phi]^n[a+b\mathbf{o}]$

$[a+b\mathbf{o}^{-1}+c\mathbf{o}^{-2}\ldots]^n=1/[a^{-1}-a^{-2}b\mathbf{o}-(a^{-2}c-a^{-3}b^2)\mathbf{o}^2\ldots]^n\mathbf{o}^{-1}$

$[(a+b\mathbf{o})^m]^n=[\mathbf{o}^m][a+b\mathbf{o}^m]^n\sqrt[m]{\mathbf{o}}$

$[\mathbf{o}-log(1+\epsilon^{\mathbf{o}})]^n=log[\eta]^n\epsilon^{\mathbf{o}}=\mathbf{o}-log(1+n\epsilon^{\mathbf{o}})$

$[\mathbf{o}^{1/(1+log\,\mathbf{o})}]^n=\mathbf{o}^{1/(1+n\,log\,\mathbf{o})}$

$[\epsilon^{\mathbf{o}-1}]^n=[\epsilon^{\mathbf{o}}][\epsilon^{\mathbf{o}}-1]^n log\,\mathbf{o}$

$[1+log\,\mathbf{o}]^n=[\epsilon^{\mathbf{o}}][log(1+\mathbf{o})]^n log\,\mathbf{o}$

$[tan[\pi+\mathbf{o}]tan^{-1}\mathbf{o}]^n=\left[\frac{tan\pi+\mathbf{o}}{1-\mathbf{o}\,.\,tan\pi}\right]^n=[\mathbf{o}]^n$, where n may be fractional, and

$\quad=tan[\pi+\mathbf{o}]^n tan^{-1}\mathbf{o}=tan(n\pi+tan^{-1}\mathbf{o})=\frac{tan\,n\pi+\mathbf{o}}{1-\mathbf{o}\,.\,tan\,n\pi}$

$[tan(\pi/4+tan^{-1}\mathbf{o})]^n=\left[\frac{1+\mathbf{o}}{1-\mathbf{o}}\right]^n=\frac{tan\,n\pi/4+\mathbf{o}}{1-\mathbf{o}\,.\,tan\,n\pi/4}$

$[sin[\pi+\mathbf{o}]sin^{-1}\mathbf{o}]^n=[-\mathbf{o}]^n=sin\,n\pi\,.\,\sqrt{1-\mathbf{o}^2}+cos\,n\pi\,.\,\mathbf{o}$

$[cos(\pi+cos^{-1}\mathbf{o})]^n=[-\mathbf{o}]^n=cos\,n\pi\,.\,\mathbf{o}-sin\,n\pi\,.\,\sqrt{1-\mathbf{o}^2}$

$[cot(\pi/2+cot^{-1}\mathbf{o})]^n=[-\mathbf{o}^{-1}]^n=\frac{cot\,n\pi/2-1}{cot\,n\pi/2+\mathbf{o}}$

$[sin(\pi/2+sin^{-1}\mathbf{o})]^n=[\sqrt{1-\mathbf{o}^2}]^n=sin\,n\pi/2\,.\,\sqrt{1-\mathbf{o}^2}+cos\,n\pi/2\,.\,\mathbf{o}$

$[cos(\pi/2+cos^{-1}\mathbf{o})]^n=[-\sqrt{1-\mathbf{o}^2}]^n=cos\,n\pi/2\,.\,\mathbf{o}-sin\,n\pi/2\,.\,\sqrt{1-\mathbf{o}^2}$

$[cos\,\mathbf{o}+\iota\,sin\,\mathbf{o}]^{-1}=cos^{-1}\frac{1}{2}(\mathbf{o}+\mathbf{o}^{-1})$—which is unreal. Hence

$[[cos\,\mathbf{o}+\iota\,sin\,\mathbf{o}][\pi+\mathbf{o}][cos^{-1}\frac{1}{2}(\mathbf{o}+\mathbf{o}^{-1})]]^n=[-\frac{1}{2}(\mathbf{o}+\mathbf{o}^{-1})\pm\frac{1}{2}(\mathbf{o}-\mathbf{o}^{-1})]^n=$

$\quad=[-\mathbf{o}]^n$ or $[-\mathbf{o}^{-1}]^n$

$=cos\,n\pi\,.\,\mathbf{o}+\iota\,sin\,n\pi\,.\,\mathbf{o}^{-1}$; or $=cos\,n\pi\,.\,\mathbf{o}^{-1}+\iota\,sin\,n\pi\,.\,\mathbf{o}$,

Derive numerous periodic operations by using the $\zeta\xi\zeta^{-1}$ method when $\xi=-\mathbf{o}$.

Study the iteration of circles, ellipses, and hyperbolic functions.

If Ω^n is an operator which raises ϕ to the nth operative power (see 1·0), show that $-\Omega^{-1}$ is a versor which turns ϕ through a right angle clockwise; and explain how it is that Ω^0 converts ϕ into the midaxis, $\mathbf{o}$.

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PROBLEMS OF ARRANGEMENT OF AN INFINITE CLASS.
(Philip E. B. Jourdain, M.A.).

If we consider a class formed by three different members a, b, and c, we find that we can arrange them in six ways:

$$abc,\ acb,\ bac,\ bca,\ cab,\ cba.$$

These various ways are known as "permutations three at a time," and, in general, the number of permutations of any finite number (n) of members of a class, n at a time, is $n!$ or "factorial n." It is easy to see that the total number of such permutations of the various classes of first 1 and then 2 and then 3, . . . , members selected from a class of n members is

$$n+n(n-1)+n(n-1)(n-2)+\ldots+n(n-1)(n-2)\ldots 2 . 1.$$

For the first term is the number of ways we can select one member out of the n members, the second term is the number of ways we can select two members out of the n members if we regard, for example, ab as a different selection from ba; and so on.

Among these permutations certain ones thus consist of the same members, and they merely differ in the order given to these members. If we disregard the various orders, we get "combinations"; thus bac is a different permutation from bca, but is the same "combination." The number of combinations of n members r at a time is obviously less than that of the analogous permutations, and we easily find that the total number of combinations of 1, 2, . . . , n members from the given class of n members—that is to say, the total number of sub-classes—is

$$n+\frac{n(n-1)}{2!}+\frac{n(n-1)(n-2)}{3!}+\ldots+\frac{n(n-1)\ldots 3}{(n-2)!}+n+1.$$

This last sum is easily seen to be the binomial expansion of $(1+1)^n-1$ or 2^n-1.

When we come to consider classes of an infinity of members, such as the class of all integer numbers or the class of all rational numbers between 0 and 1, we enter the theory of "transfinite numbers," of which the foundations were laid by Georg Cantor. In the first place, the distinction between a "class" of members and a "series" of members then becomes of very much greater importance than it is in the case of finite classes. When we speak of a "class" of members, we do not imply any particular *order* among the members, whether or no a particular member x "precedes" another member y in virtue of some quality like size or position or colour or loudness of tone. If a class is put in a certain order, whether or not this order is linear or multi-dimensional, we have what is called a "series." The series used in mathematical analysis are generally made up by adding or multiplying real or complex numbers so as to form a sequence S_1, S_2, S_3, . . . , S_n, . . . in an order like that of magnitude of what we call the "natural" numbers 1, 2, 3, . . ., n, . . . ; but, in the extended use of the word, a "series" can be in any order: thus the rational numbers form a "series," although no single term of it has an immediate predecessor or an immediate successor.

Now Cantor, about 1882, was led by severe meditations on such questions as that, if we represent values of a variable approaching a limit by points x_1, x_2, . . ., x_n, on a straight line, there is some reason for "numerating" the limiting point to which all the x's tend by another x to which is affixed a suffix (ω) which we consider as representing not a number just as any one of the previous suffixes does, but a new, non-finite number which is *the least number greater than all the finite numbers n* which are used up for the suffixes of the x's representing the

and the couples are such that in each chain no m or a occurs more than once, and, if a occurs, all ordinal numbers less than a occur also. Thus, a chain is not strictly a part of M, though it obviously determines such a part in a very simple way, and we will say that a chain "exhausts" M if the class of left-hand members (m) of the couples of the chain consists of all the members of M.

Of course, we do not assume that one of the chains of M exhausts M, or, for example, M lacking some one member: this is what we have to prove. All that is necessary for the validity of what follows is that there are *some* chains of M, and this is evidently so if M has any members at all, for we can then select arbitrarily chains of M of, say, one and two members.

A chain carries with it an obvious rule by which it can be well-ordered: this rule is that the couples are to be arranged in the order of magnitude of the right-hand members (a) of the couples of the chain. We shall always assume that a chain is ordered in this way. In conformity with Cantor's language (*op. cit.* p. 141), a chain P is said to be a "segment" of a chain Q if P is identical with the chain whose members precede some member of Q. In this case, we will also say that Q "continues" or "is a continuation of" P.

If a class of chains has members of all types less than γ, we cannot, in general, conclude that it has one of type γ. But if we have a class of chains such that, if x and y are members of it, and the type of x is greater than that of y, then y is a segment of x; we can obviously conclude, from a knowledge that the class has members of all types less than γ, to the fact that it has a member of type γ, provided that γ, provided that γ has (like the number ω) no immediate predecessor. For, in that case, all the members of type less than γ build up a chain of type γ. We will express the fact that a class of chains is of the nature just considered, but where γ need not necessarily lack an immediate predecessor, by saying that it is a class of "direct continuations." If, then, (a_1; a_1, a_2; a_1, a_2, a_3; . . .) is a class of direct continuations, the chains being of all types less than ω, the class defines (if we take the nth member of the nth chain in the above order) the chain a_1, a_2, a_3, . . ., and this last chain defines the former class as the class of its segments.

The whole class of chains of M falls into classes each of which contains all those chains which have the same ordinal type. Now, our first object is to rearrange all these chains in classes of direct continuations such that no chain of M continues the single chain defining and defined by (cf. the end of the last paragraph) any one of these new classes. We then prove that there are ordinal numbers greater than the types of the chains so defined, and yet that each such chain exhausts M. This stage of the proof is necessary if we are to be sure that there is really a chain of M which exhausts M; and seems to me unavoidably to depend on an argument previously given by me in 1904, which was stated by Russell (1905)—apparently on no grounds save the delusive ones of appearances—to be of a nature quite different from that of Zermelo's principle.

Firstly, we will see how all the chains of M fall, in a way that is throughout uniquely determined, into classes of direct continuations which we may call "complete," since there is no chain of M which continues the chain defined by any one of these classes.

Think of all the chains of M of type 1 as forming a class which may be denoted by u_1. With each of these chains of type 1 put, for the moment, all those chains of M which are of type 2, and are also such that the chain of type 1 mentioned is a segment of them. Imagine a chain identical with this chain of type 1 put with each of the chains of type 2 which are thus correlated with it. Though I speak,

for the sake of what seems to me ease of visualisation, of *many* chains identical with a certain chain, it must not be forgotten that there is really only one chain which is identical with a given chain—namely that chain itself.

We do this for all the chains of M of types 1 and 2, and thus get a class u_2 such that, if y is a member of u_2, y is a class of direct continuations, and the continuations are certain chains of types 1 and 2. It is to be noticed that there is, throughout this construction, no choice of particular members of the u's; and each u except the first is formed from the preceding u or u's and the chains of M in a completely non-arbitrary and uniform way. Also notice particularly that the u's are such that the members of u_n contain those of u_{n-1} in such a way that those of u_n grow out of those of u_{n-1} by addition to all the latter ones, on a perfectly definite system, of all the chains of type n. We can thus see that the u's mentioned mark stages in one and the same process of building up, out of chains of M, classes of direct continuations; each u_n, namely, contains all possible classes of direct continuations where the chains are of all types from 1 to n.

Suppose that those chains of M whose types are all the ordinal numbers less than γ, where γ has an immediate predecessor $\gamma-1$, thus form classes of direct continuations; the class $u_{\gamma-1}$ being such that, if x is a member of $u_{\gamma-1}$, x is a class of direct continuations where the continuations are of types $1, 2, 3, \ldots, \gamma-1$. Of the chains of M of type γ—if there are any—put in each x all those which continue the chains in that x. Then imagine that chains identical with all those previously in x are put with the appropriate chain of M of type γ, so that a new class of direct continuations of types $1, 2, 3, \ldots, \gamma$ arises; the class of these classes may be denoted by u_γ. Here again, this u_γ is determined uniquely by the preceding u's and the chains of M of type γ—if, indeed, there are any. Also we imagine several sets of chains identical with a given set, just as above we imagined several chains identical with a given chain.

Suppose that no u of suffix less than ω is null, and consider all those classes of direct continuations formed by the above definite process which are such that all u's of suffixes less than ω appear in the process. We can consider the whole class of the classes thus formed by this infinite process, because it is a complete and well-defined mathematical object. In general, if γ has no immediate predecessor, we can evidently form a chain of type γ out of a class of direct continuations where the continuations are of all types less than γ. It must again be emphasised that we are able to conclude here from chains of types less than γ to a chain of type γ, without a use of Zermelo's principle, only because all these chains are direct continuations. Thus, if γ has no immediate predecessor, and if no u of suffix less than γ is null, u/γ is not null.

This unique determination of a whole transfinite series of u's, continuing as long as there are any chains of M left, is the most important part of this construction. It will be seen that the method of generation of the series of u's corresponds to the two "principles of generation" spoken of by Cantor (*op. cit.* pp. 56-60, 169); the combined action of these two principles carries us to all the number-classes of Cantor in succession.

Secondly, any one of these complete classes of direct continuations determines, and is determined by, a single chain of M.

Thirdly, we will prove below that, for this chain, there is at least one ordinal number ξ such that no segment of the chain or the chain itself is of type ξ; and yet the chain exhausts M. This chain is, then, obtained without a use of Zermelo's principle. Indeed, the class of all chains of M, where we do not assume that any of its members exhausts M, falls, without any arbitrary choice

on our part, into a class of chains, each of which can be proved to exhaust M for some definite ordinal type.

Consider any one of the complete classes of direct continuations. The chain defined by it must exhaust M; for otherwise there would be a member of M which was not a member of the chain, and in that case the class of direct continuations in question would not be complete. Hence we see that the chain just referred to cannot be a segment of any other chain of M, and, now that we know that such chains exist, we can say that the chains which exhaust M make up the remainder of the class of chains of M which is obtained when we cancel all those chains which are segments of other chains.

But there still remains an apparent possibility: may it not happen that, however great the ordinal number ξ may be, a complete chain is always such that it has a segment of type ξ? But this is impossible, for a chain of such a nature that, however great ξ may be, it always has a segment of type ξ, must be of the type (θ) of "the series of all ordinal numbers." Now, we can prove that this series is well-ordered, for any part (P) of it which has any terms at all—say p—has a first term—namely, the first term of the well-ordered series formed by p and those terms of P which precede p. Hence θ is an ordinal number, and hence β is both the ordinal number of a series and a term of the series, so that β is greater than β. This implies, of course, that the series of all ordinal numbers is ordinally similar to a segment of itself, and thus that the series is not well-ordered; and, therefore, that there is no such thing as what we meant to denote by the phrase "the series of all ordinal numbers," which would thus be both well-ordered and not well-ordered. But at present we only need the proof that it is impossible that a complete chain should have segments of all types. This is a form of the argument on which I laid stress in 1904.

It must be noted that the proof given in the last paragraph essentially depends on the fact that a class of direct continuations has been obtained. It is only for a class of direct continuations, unless we use Zermelo's principle, that we can conclude that, if, for any ξ, a member of the class is of type ξ, then there would be a member of the type of "the series of all ordinal numbers"; just as, without a use of Zermelo's principle, we cannot conclude, from the fact that a chain of chains has members of all types which can be defined by "mathematical induction," and which we usually call "finite," that it has a member of type ω, unless it is a class of direct continuations.

We have, then, proved that, for any M there is at least one chain that both exhausts M and is of type less than some type ξ, say. Of the ordinal numbers greater than the type of this chain there is one that is the least, since ordinal numbers in order of magnitude form well-ordered series; let it be denoted by ζ. This ordinal number ζ must have an immediate predecessor; for, if it had not, the chain itself would be of type ζ, and so ζ would not be the least ordinal number that is greater than the type of the chain. Hence ζ is of the form $\zeta' + 1$. Now, ζ' is the first number of one of Cantor's number-classes; for if there were numbers of the same number-class which were less than the ordinal number just mentioned, this ordinal number would not be the least to which would belong chains which were not continued by other chains of M.

So any aggregate M can be well-ordered, and thus Zermelo's principle can be proved. The problem may be considered as one of organisation, and it is rather remarkable that it should be possible so to organise the members of any infinite class that selections of series of any types less than a maximum one can always be made from it. This is very useful for many mathematical purposes.

VITALISM OR MECHANISM?—VIA MEDIA (J. E. Turner).

To bring Life into true relation with the other primary categories of the Universe has long been the aim of Science; and I shall endeavour to show that Vitality is but one form, conditioned by the special nature of its physical substratum, of the Individuality which characterises the Whole—that there is therefore nothing absolutely and exclusively peculiar to it; further, to examine the scientific grounds for the belief that Development, which has so far attained to the Social Mind, will be continued, without fatal interruption from the future physical condition of the Universe.

I.

(1) Recent discussion of the problem of Vitalism[1] has two radical defects—

(*a*) Undue isolation of the subject matter from allied phenomena—perhaps an inevitable result of specialisation;

(*b*) Failure to seize the essential nature of the problem as a whole; so that the facts constitute a mere barren summation whose real significance is ignored, and in whose place we are offered (Dr. Haldane) a dubious metaphysics, or (Driesch) an unknown god in the guise of an entelechy neither physical nor psychical; so that the result is the creation of a wholly false antithesis between the conceptions of Mechanism and Vitalism, these ideas becoming opposed to each other absolutely, and each school insisting on the total failure of the contrary theory adequately to explain the phenomena.[2]

Is, then, any solution of this *impasse* possible? I think only in realising that the current absolute opposition between these two concepts, when properly understood, becomes groundless and misleading, and arises from failure to apprehend the essential character of "Mechanism"—to see that this category actually contains within its very nature the inevitable necessity to develop into "Vitalism' once proper conditions are established;—to trace, in short, the logical continuity of development from "Mechanism" to "Vitalism."

(2) For, so far as the essentials of Life are concerned, it is obvious that the absolute repudiation of all Mechanism is the denial of any real evolution, which postulates an *unbroken* transition, however opposed in character the various stages seem to be; and to assume, at any stage whatever, the operation of an agency whose full potentiality did not already exist in previous actual conditions is plainly the negation of efficient causation. But while this universal postulate cannot logically be rejected, it remains true that the actual continuous transition implied cannot yet be traced in concrete detail; still, this factual ignorance need not prevent its complete acceptance, if it be but supplemented by (*a*) the consideration of the essential nature of Individuality in general, and (*b*) of the gradations which this must necessarily exhibit. The illegitimate antithesis between Mechanism and Vitalism then disappears, and is replaced by the conception of a logically necessary gradation or transition from one to the other,

[1] I define Vitalism as any hypothesis that Life (excluding consciousness) is determined by some non-physical agency. Life really should include consciousness, which, however, I refer to only incidentally; although I think its consideration would lend further support to my main argument.

[2] *E.g.*: "the phenomena of life are such that no physical or chemical explanation of them is remotely conceivable . . . Vitalism raises no objection to physical and chemical explanations applied outside the intimate vital processes of living organisms." (Haldane, *Mechanism, Life and Personality*, pp. 64, 22.)

which depends on the environmental *ensemble*: so that the two categories, no longer mutually exclusive, find places beside each other as diverse manifestations of one natural Individuality; for vitalists and mechanists are no more opposed to each other in reality than are the constructors of a tunnel because they bore from opposite ends;—their respective conceptions are not contradictory, but rather complementary. Our ignorance of organisms, however, is still so great that it is impossible to prove this community of character until we pass beyond Vitality itself, and that in two directions—both above and below the realm of Life[1];—above, to social change, and below to certain chemical and physical systems properly so called.

(3) The most distinctive and the least understood life processes are those of development; and, like organisms, nations too have their embryonic stages; and History traces (as a concrete instance) the British Empire to its origin in the simpler early English community. But this "simplicity" must not be taken to mean that early England was merely the miniature Empire waiting to be enlarged like a photograph, or expanded like a bubble. Its "simplicity" was really a vast potentiality, such that in response to world changes it inevitably became the Empire; but no historian ever dreams of attributing national development to any entelechy, or explaining it by metaphysics; rather the personal and social factors actually operative at each stage are regarded as all-sufficient to account for subsequent changes[2]—a standpoint in marked contrast to that of many biologists. Now it is merely the generalisation of this historical mode of procedure that constitutes the Principle of Individuality, which maintains (*a*) that every instance of such true development,[3] in any of its forms—national, personal, vital, or physical—finds its complete causal explanation in the nature itself of the developing individual concerned as responsive to its total environment, and never in any entity beyond or beneath this; and (*b*) that the particular form of this development—its course, duration, and other special characteristics—is always a function of the individual's original undifferentiated complexity,[4] and of nothing else. In this connection the term "individual" has a meaning which, though certainly wide, is still very closely allied to ordinary usage; for it means merely any indivisible natural system whatever,—any system, *i.e.*, which would in its essentials be destroyed as such by division[5]; *e.g.* the electronic atom, family, nation, organism. The individual, further (thus understood), can never exist in total isolation, but only in relation with its environment—*i.e.* with other "individuals"; since *every* natural entity is in this sense an "individual"—a system, *i.e.*, of some kind—never formless, never perfectly simple, and never isolated.

(4) Every such system or individual, then, has always *some* degree of complexity; and we must now trace the logical implications of the varying degrees

[1] As remarked at the outset, the failure to do this constitutes one main defect of recent controversy.

[2] This is the historic Ideal—I do not mean that it can ever be actually realised—as distinct from that of the philosophic or religious thinker.

[3] By "true development," not to prejudge the issue, I mean where actual continuity or identity can be traced or presumed in spite of no matter how great a change of character; and in organisms this is unquestioned.

[4] This expression, and its "function," are best understood as being analogous to any unexpanded mathematical term—*e.g.* $(a + x)^n = \ldots$

[5] "Individual" means (literally) indivisible; and the ordinary individual person is obviously an indivisible system; of course not all divisions are fatal, some being reproductive. On the relation between Individuality and Natural Law I may refer to Dr. Bosanquet's *Principle of Individuality*, Lecture III.

and kinds of complexity which characterise natural systems in general; we shall find that, while they appear first as "mechanical," this character speedily becomes transformed into some higher category. For to every such system, whatever its specific character, each of its diverse constituents contributes its own distinct active nature and properties, through each of which some quite definite relation is maintained with the environment. It is obvious, therefore, that every increase in the number of constituents will, as a general principle, make more complex the system's relations to other individuals—*i.e.* render the system more sensitive and responsive to external change and influence. Systems of relatively few constituents will thus be capable of but few reactions; their total activities will lie within only narrow limits, and will therefore be easily ascertainable and exactly predictable; their innate energies will readily be wholly absorbed within the system itself,[1] which then reacts as a stable static and insensitive whole;—is, in short, a "mechanism." For in calling any system "mechanical," it is essentially these characteristics which are in principle implied.[2] The constitution of every "mechanism" being (relatively) simple, its actions can have relatively little variety; as the results of the activities of but few constituents, they are easily determined with almost absolute exactness[3]; its equilibrium is stable[4]; indeed it is (somewhat paradoxically) just for these reasons that we often call activities "mechanical" which are normally quite the reverse—*e.g.* human character and action under certain abnormal conditions; and "all modern nations," as Lotze remarks, "speak of the *mechanism* of government, of taxation, of business,"[5] with unquestionable correctness; as, again, we may do of the reflex and instinctive activities of organisms.

(5) This being true as regards the essential meaning of "mechanism," what now are the inevitable results of continuous increase in the number and diversity of the systematic constituents? We shall find that the "mechanical" character, as just defined, slowly disappears, undergoing a gradual transformation into a type which at present we may call (negatively) non-mechanical[6]—a "higher" category which is manifested in many diverse modes, of which one constitutes Vitality—Life; but neither in this nor in any other case is there ever any gap—any sharp division—absolutely separating these "higher" modes from the "lower" grade of mechanism.

For every additional constituent brings to the entire system, and retains within

[1] *Cf.* a "bound" electric charge.

[2] A "machine" is merely an artificial, as distinguished from a natural, mechanical system. The character of purpose, emphasised by Prof. Hartog, is no proper criterion, for *every* system as such implies some purpose.

[3] Thus a *good* machine serves its purpose exactly, and nothing more; this principle is plainly not affected by the constant tendency towards more complex machines, so long as superfluous parts are avoided.

[4] This is true even of explosives, which are unstable only under some extremely limited set of conditions.

[5] *Metaphysic*, vol. ii. p. 116.

[6] But a system, in becoming automatic, becomes not more mechanical, but less. *No* mechanism can be truly automatic, in the strict sense of completely self-directing. The prevailing confusion between automatism and mechanism depends on our actual machines being automatic merely in some relatively simple details, and not on a large scale;—where this does become possible the final effect at once becomes non-mechanical, as *e.g.* in a good pianola; and true automatism—*i.e.* self-direction and determination—is found only in living and conscious individuals.

it, its own independent nature, so that the total range of activities becomes thus step by step more elastic, less definite, and therefore less predictable; each element having its own susceptibility to external influence, the equilibrium becomes therefore less stable, and the free energy greater, permitting many and loose combinations, and an increasingly delicate response to the environment.[1]

(6) And though the most striking examples of what I have called the "non-mechanical" are certainly furnished by vital phenomena, still we shall find, if we follow out these transitions in other realms of Nature where the details are more fully known, that "mechanism," in being thus transformed into "higher" categories, thereby acquires such characteristics that it becomes possible to see that life also is itself but another instance of the same transition, and that vital change is thus not something absolutely opposed and foreign to mechanism.

In the first place, it is possible for a system which in type is purely mechanical—a system whose elements are relatively few and simple—to be so conditioned[2] that its total development presents a very long series of most complex phases, far surpassing (in this respect) any living individual or species;—this occurs when the simplicity of the physical constituents themselves is combined with a great range of variety in the *details* of their relations. Such a combination, however, demands vast space and long duration, so that its best examples are found in the solar and stellar systems. Here the planetary elements are in themselves of the simplest possible physical character—moving, gravitating bodies; and yet so delicately graduated are their mutual attractions that the complex and protracted developments irresistibly suggest Vitality.[3] In this connection, again, the analogies presented by the electronic atom and by radio-activity—phenomena purely mechanical—are obvious.

And conversely, vital phenomena themselves necessarily present one aspect which is undoubtedly "mechanical," and once more disproves any absolute opposition between the two types; for in principle, and apart from the special details of each particular case, every complete vital cycle has a fixed or "mechanical" aspect—every course of development follows a certain regular order, from which any marked departure is an abnormality[4]; so that to a Martian—*ab extra*—the life cycles of a species would appear just as mechanical as a number of clocks—taking the sequences *as wholes* there would be nothing to distinguish one type from the other.

(7) But when Time and Space become limited, complex development is possible only with systems whose constituents are at once many in number and diverse in character,[5] of which (excluding psychical and social phenomena), Protoplasm is undoubtedly the principal. As we have seen (*ante*) so complicated a material system must necessarily possess extreme sensitiveness and instability—it cannot in face of its environment be rigid and unchanging; and therefore the only question is as to which of two possible courses its changes must follow—whether *i.e.* its integrity as a purely material system will (under conditions whose

[1] *E.g.*, increasing internal national differentiation brings about more numerous and delicate international relations.

[2] Owing to the "degradation" of energy—see below, par (9).

[3] *E.g.* Arrhenius, *Life of the Universe*, and Bickerton, *Birth of Worlds and Systems*.

[4] "Reproductive processes in the Protozoa, like those in the Metazoa, tend to run in cycles" (Marshall, *Physiology of Reproduction*, p. 212).

[5] I shall try to show later that such systems are at all possible only as a result of the "degradation" of the universe's energy.

limits are well known) be maintained, or destroyed. Our decision here must be based largely upon presumptions; I would summit, however, that none of the phenomena of Life suffice to support the view that it is antecedently impossible for the organisedly complex protoplasmic system to determine its whole development through its material constitution and nothing beyond. For we are equally ignorant of the internal forces of the electronic atom[1]; but we do not therefore feel called upon to supplement this lack of knowledge by either Metaphysics or Entelechy; and in the other direction we regard the internal and colonial development of the Empire *e.g.* as fully explained by all the social factors operative at any given stage, again without any ulterior agency; and these present a close analogy to growth and reproduction.[2]

Again, the organism's sensitivity to excessively minute quantities of certain substances (on which Dr. Haldane lays such stress), is another necessary character of a material system each of whose many and diverse constituents exists (as in the cell) in very minute quantities; while, on the other hand, it is difficult to imagine why any non-physical entity should prefer such minute, rather than large quantities. Nor, further, is this high sensitivity a peculiarly vital phenomenon; the extreme sensitiveness of spectrum analysis is well known; but even this is exceeded in the application of positive rays to chemical analysis[3]; and the suggestion may be hazarded (though it is highly speculative) that the microscopic proportions of the complex group of reagents in the living cell may facilitate chemical changes at temperatures lower than are possible in laboratory experiments. Prof. Hartog, again, appears to regard the germ cells as simple—"embryonic cells undergo differentiations, losing their simplicity as they do so"[4]; but of all cells the germ cells are surely the most complex as organic systems, whatever view be taken of their development.

The complex of activities which we call Life demands therefore no wholly peculiar type of influence, restricted to vital phenomena and absent from all others. The distinctive characteristics of life are manifested only in all its concrete details, which correspond to its peculiar extremely complex material, but which as a whole are only one instance of a type more inclusive and all-pervading. The terms "Vitality" and "Life" are only legitimate as are electricity, chemistry, or psychology—*i.e.* as overlapping and inclusive, not as watertight and exclusive; they imply nothing more than a complex of simpler and "lower" processes, just as chemistry and electricity, or psychology and sociology, overlap, include, and imply each other. The only thing absolutely peculiar to chemistry, *e.g.*, is the whole of its concrete details; but to say this is obviously at once to invade to some extent the various subdivisions both of physics and of physiology. Similarly a complete sociology includes psychology, and *vice versa*, without these, however, becoming identical or confused with each other. Exactly so is Life nothing beyond a combination, made possible by the extreme complexity of protoplasm, of processes each of which taken alone falls within a simpler category which,

[1] "We know very little about the fundamental structure of the atom" (Richardson, *Electron Theory of Matter*, p. 169).

[2] The character of early societies appears to throw light on the nature of the germ; but the subject falls outside this article.

[3] J. J. Thomson, *Rays of Positive Electricity*, preface, also p. 107.

[4] *Problems of Reproduction*, p. 257. As a single cell, the germ is certainly "simple" compared to the organism; but this is to emphasise its mere external aspect while totally ignoring its internal character.

again, preceded Life in the world's history, just as Vitality itself has preceded consciousness and social change.

Can we further ascertain the general conditions of this evolution, and whether its future course and increased complexity are limited or not?

II. The Relation between Development and the Degradation of Energy.

(8) The relation between vital phenomena and the universal transformation of energy has been considered by Dr. Johnstone,[1] who concludes pessimistically that "the universe tends towards a limit which is the cessation of all phenomena—universal physical death" (p. 650). But I think it is possible to show that such a negation of the evolution of Life and Mind is not altogether justified.

Clerk Maxwell has pointed out that while the dissipation of energy is closely connected with the increase of entropy, it is not identical with it;, and that in calculating dissipated energy we must take account of the final temperature of the system when in thermal and mechanical equilibrium[2]; so that the apparently wasteful exothermic chemical production of heat must be considered together with (*a*) the initial and final temperature of the whole system, and (*b*) the increasing stability of the resultant products.[3]

Is it possible, then, from this standpoint to suggest any *rationale* of the actual distribution of universal heat and matter among bodies varying enormously in volume and temperature, and all in the main losing heat?

(9) Obviously the only conceivable alternative—a system with low or zero temperature—would be mechanically quite valueless; and since every physical[4] process has its optimum range of temperature, any *diversified* system of change plainly necessitates a very extensive scale of temperatures, each optimal to some definite process, and therefore possibly only through a slow fall from a very high initial temperature. Optimal temperatures, again, fall into (*a*) those which make possible any given combination—critical temperatures; and (*b*) those which permit any such combination, once formed, to persist; and, as Dr. Johnstone observes, all exothermic changes tend towards increased stability. Thus the protracted temperature fall, necessary in the first place for *diversified* physical changes, also makes possible (*a*) the initiation and (*b*) the persistence of stable combinations—*i.e.* in short a continuous increase in stable complexity.[5]

But it also brings about other results. For since the sum of the moments of momentum in a system is constant, it must, in face of the continued heat loss, tend to assume a symmetrical instead of any originally irregular form, and this the more rapidly the more heat is lost; while at the same time the resultant contraction tends to accelerate rotational velocity, so that (*e.g.*) the present form and motions of the solar system could only have been brought about through the loss of its initial heat.

(10) Thus the heat loss, which at first sight appears to be a serious defect, is

[1] Science Progress, ix. p. 646.
[2] *Theory of Heat*, p. 192.
[3] Johnstone, *loc. cit.* p. 648.
[4] "Physical" is used here in its widest possible meaning.
[5] But this change is wrongly regarded as being from homogeneity to heterogeneity, for the initial stage must be itself as heterogeneous as the final, since the ultimate elements throughout remain the same; the point is that the resultant systems become more complex and stable.

found to determine in reality all the primary conditions of development; so that we come to have a dual problem—(*a*) How high can the increase in complexity become (*b*) in face of continued heat radiation?

With regard to (*a*) taken in itself, I do not see that we can as yet affirm any limits whatever to the advance—just as any prediction (supposing such possible) of the impossibility of the advent of Life from the inorganic world, or the evolution of Man from Protozoa, would have been falsified by the event,[1] so we cannot yet (if ever) assert that any given natural system[2] has no possible complement elsewhere in the universe.

(*b*) But, admitting this general principle, continued heat radiation may still be held ultimately to terminate all such changes. But against this may be placed the fact that, though the assumed redistributions of temperature are such that instead of a system of localised high temperatures there will be uniformity, it does not therefore at once follow that this final temperature will be fatal to all change; equally probably it may prove to be a temperature optimal for at least some kinds of change. So that the evolution of heat in exothermic reactions has a further advantage, the resultant compounds being stable and persistent, the liberated heat at the same time tends to establish a uniform medium temperature which, being certainly lower than stellar temperatures, is probably optimal for these same stable substances and their combinations. Again, the rate of temperature fall is asymptotic—the lower the temperature the more slowly does any given fall proceed; while at the same time there is substituted for rapid etherial radiation transmission through gaseous media of very low conductivity[3]—two factors plainly tending to maintain a medium uniform temperature.

In short, to regard the collective heat interchanges of the universe as a "loss" is plainly wrong; for the heat, though certainly lost by some systems while gained by others, still remains within the universe itself, only under conditions of uniformity instead of localised extremes; and, as for the solar system, in which humanity is specially interested, it is certainly possible that the sun's failing heat may be supplemented by its entrance into some younger stellar combination, the probability being that stellar motions are regulated somewhat as are the planetary.

THE DENUDATION OF THE WEALD: A Defence of Existing Theories (Henry Bury, F.G.S.).

In the April number of Science Progress there appeared a paper by Major R. A. Marriott, entitled "The Downs and the Escarpments of the Weald: A New View of their Geological History"; and, as it contains numerous strictures on geologists for having overlooked facts and inferences with which they have, in truth, long been familiar, it may be permitted to one who still prefers the older views to offer a few criticisms.

To the student of physical geography the two points in the Wealden Area which most require explanation are its escarpments and its transverse rivers. On all sides except that open to the sea the area is bounded by a range of chalk hills

[1] On purely philosophical grounds I think it can be shown that in a systematic universe this continuous increase in complexity is logically inevitable.

[2] Which need not be material only; as development advances the newer categories—vital, intelligent, social—become more prominent as against the background of the older.

[3] That of air, *e.g.*, is 20,000 times less than that of copper (Maxwell, *op. cit.*, p. 343).

(the North and South Downs) which everywhere present a steep face (escarpment) towards the interior, and, with one brief exception, a much more gentle slope towards the exterior. Precisely similar features are shown by another series of hills, running roughly parallel with the Downs, in the Lower Greensand formation, but the range is not nearly so continuous, and only attains to any conspicuous elevation in the north-western corner of the area. The principal rivers for the most part pursue a "longitudinal" course, parallel with the escarpments; but only one of them escapes directly to the sea; all the others sooner or later join "transverse" streams which pass through the escarpments, in narrow valleys, towards the exterior; and this is the more remarkable because, in the majority of cases, these rivers rise near the axis of the Wealden Area in ground decidedly lower than the range of Downs which they afterwards traverse. In no case does any important stream enter the area from the outside.

What explanation does science offer of these remarkable features?

Up to the middle of last century it was commonly believed that the whole region, in very much its present form, had comparatively recently been invaded by the sea, and that the chalk escarpments were old sea-cliffs; but this view, which offered no reasonable explanation of the transverse rivers, has long been abandoned, and no one now doubts that the existing sculpture of the surface is almost entirely due to subaerial agencies—especially rain and rivers. Opinions differ, however, as to the initial stages of this process.

Of course the strata, which now slope away to the north and south on either side of an approximately east and west axis (roughly corresponding with the main watershed), were originally horizontal, and (*pace* Major Marriott) there is no doubt that all the beds from the Hastings Sands to the Chalk (which is as far as we need carry our inquiries) spread over the whole of the district, and for many miles beyond it. At the close of the Cretaceous period there seems to have been some upheaval and some denudation, which probably, in places, exposed the Lower Chalk, and possibly even some Lower Cretaceous beds; but it is unlikely that the latter were extensively uncovered, since there is practically no Lower Greensand chert in the Eocene deposits.

But it was not until the close of the Eocene (at earliest) that those great earth-movements occurred which heaved up the whole Wealden Area (including the Bas Boulonnais in north-eastern France) into one wide, but comparatively flat arch or dome; and it is at this point that opinions first seriously diverge. It is agreed that the whole crown of the dome, amounting to thousands of feet in thickness, has been removed; but the exact mode of removal is open to question. Some hold that the present river system began at this stage, and that the removal has been effected entirely by subaerial denudation. Others believe that the sea played an important part and wore off the whole summit of the dome to a "plain of marine denudation"; and that it was on this plain, upheaved and slightly arched in, probably, Pliocene times, that our existing rivers originated. The ultimate result, in all its main features, would probably be the same in either case; and it is only by careful inquiry into detail that we can hope to determine the relative merits of the two hypotheses. Such an inquiry would be impossible in a paper like this, but a brief outline of the main features of evolution may be attempted, and for this purpose it will be convenient to take the second, or "Planation Hypothesis," as we may call it, first.

Fig. 1 shows, in a diagrammatic form, and with an exaggerated vertical scale (1) the dome postulated by the first hypothesis; (2) the marine plain of the second hypothesis, uplifted and slightly curved; (3) the present surface, with hill-ranges

and escarpments in the Chalk and Upper Greensand; and it is easy to see that the effect of planation is to bring the successive formations to the surface in a series of roughly parallel or concentric belts.

As the main axis lies approximately east and west, the earliest rivers ran down the slopes to the north and south; and when elevation was complete, they would evidently pass over each of the strata in turn. But of course the uplift was really gradual, and not sudden and abrupt, so that the streams actually began on the oldest (or central) beds, and only reached the others as they successively appeared above the sea. But the different strata offer widely different resistance to the action of rain and rivers, hardness being perhaps the main factor, but porosity being also important; and the result of this is that the less resistant beds (clays and some sands) are quickly removed and worn into valleys, while the more resistant (Chalk, and chert beds of the Lower Greensand) stand up as ridges; and since, as we have seen, the strata are arranged in roughly parallel belts, running mainly east and west, it follows that the main trend of hills and valleys is also in this direction. It is in these "longitudinal" valleys that most of the principal rivers run, but practically all of them sooner or later join one or other of the original transverse rivers, and these, continuing to deepen their beds as the land rose higher above the sea, now run, in narrow steep-sided valleys, right through the longitudinal hill-ranges, which owe their very existence to the excavation of the valleys on either side.[1] The process of wearing away of

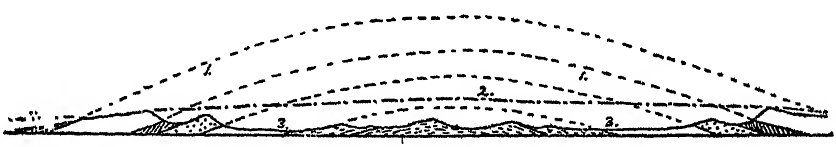


FIG. 1.—Diagrammatic section across the Weald, showing (1) former continuation of strata; (2) plain of marine denudation; (3) present surface of the ground, with escarpments in the Chalk and Upper Greensand.

the softer beds may then continue, as it actually has done in most parts of this region, until the land in the centre, where the transverse rivers rise, is actually lower than the peripheral hills which they afterwards traverse.

A word about the escarpments. The Chalk, though highly resistant to direct assault (by rain from above), is much more vulnerable to flank attack. Not only is the Lower Chalk usually softer than the Upper, but rain and rivers, by eating away the soft Selbornian beds, tend to undermine it, and it is to this that the formation of the escarpments is due. The process is in many ways comparable to that by which a sea-cliff is undermined by the waves, only it is so infinitely slower that the Chalk, instead of standing vertically, has time to become disintegrated by atmospheric action and crumbles away to a steep slope instead.

Like sea-cliffs, then, the escarpments (in Chalk, Greensand, or other formation) are receding; and it is this recession which forms the key-note of the first, or "Chalk Dome Hypothesis," which supposes that the summit of the arch of Chalk has, except perhaps for some pre-Eocene denudation, been entirely removed by

[1] The modern theory of river-evolution is admirably explained by Prof. W. M. Davis (*Geog. Journ.* vol. v. 1895, pp. 127 *et seq.*), and more briefly by Mr. Makinder (*Britain and the British Seas*, pp. 144–5), and Lord Avebury (*Scenery of England*, 1902, pp. 364–5).

suoaerial agencies. That some pre-Eocene denudation did occur is certain; and if, as is probable, it exposed the Lower Chalk, and perhaps even Lower Cretaceous strata on the crown of the arch,[1] then there existed, from the moment of final emergence from the sea in post-Eocene times, some of that differential resistance to which the longitudinal hills and valleys owe their origin. And when once this process was started it would be only a question of time before a structure would be evolved practically indistinguishable from that arising after marine planation.

It has been thought necessary to give this brief outline of the older hypotheses because Major Marriott, who rejects them both, seems hardly to have understood their import. He lays the utmost stress on the resistance of the Chalk to erosion, which he describes as the "cue which has been missed" (p. 592); and he also insists strongly on the "differential erosion," which he tells us has not been recognised (p. 603), arising from the lesser resistance from the sands and clays. But these aspersions on geologists are quite unmerited. This differential erosion is in truth, and has been for years, one of the most important factors in all attempts to explain the sculpture of the earth's surface, and the special case of the Chalk Downs was fully recognised by all the pioneers who first taught us the meaning of escarpments.[2] And, after all, is the removal of the Chalk by denudation[3] so extraordinarily difficult as Major Marriott would have us believe? Even on his hypothesis a wedge of Chalk several miles in width has been removed to form the escarpments (fig. 2); and his inference that, because the Upper Chalk appears on the surface here and elsewhere, it has suffered hardly any change since its deposition (p. 600) is unwarranted. This formation is of great thickness (300–500 ft.) and can be separated into zones, each with its characteristic fossils; and, as it is seldom that anything higher than the lowest zone is found on the crest of the escarpment, it is clear that a great deal is missing; all the evidence, however, goes to show that the absence of the higher zones is not due to irregular deposition, but to removal by denudation—how far marine and how far subaerial we cannot say. But the most obvious proof that great masses of Chalk can be eroded away is to be found in the cliffs of England and France, once clearly joined together, but now widely separated. What is the floor of the Straits of Dover but a "plain" of marine denudation?[4] And yet this plain is of very recent origin.

The hypothesis which Major Marriott attempts to substitute for the older ones is that "the chalk never covered the Weald, but that the sandstones, sands, clays, etc., of the latter once formed *an island or bank laved by the sea* in the depths of which the chalk was deposited" (p. 595. The italics are mine), and he gives a diagram (p. 596) showing the position of this fringe of Chalk after

[1] More information is required about this pre-Eocene denudation; but the argument would not be materially affected if there had been none of it at all.

[2] See quotations in Topley's *Geology of the Weald*, pp. 273-4.

[3] Major Marriott says (p. 523), "denudation, being a technical term in geology, does not express the process illustrated" in his paper. But Prof. Marr (*Scientific Study of Scenery*, 2nd edition, p. 24) defines it as "the stripping of portions of rock from one place and their removal to another"; while Prof. W. M. Davis (*Physical Geography*, p. 105) says, "the general process of wasting and washing, by which surface structures of the earth's crust are attacked, is called *denudation or erosion*." I therefore fail to grasp Major Marriott's meaning.

[4] Major Marriott (p. 594) attributes the formation of the Wealden portion of the Channel to depression in a syncline, but I am not aware of any evidence pointing in this direction; and in any case there must have been much erosion.

upheaval. A glance at the conditions, under which this island is supposed to have existed, will show that this position is quite untenable. All the strata, from the Hastings Beds to the Chalk, bear witness that a series of undulatory movements of the sea-level took place during their deposition, with a general balance in favour of greater and greater submergence. This is part of a world-wide series of changes, which, by the time the Chalk was deposited, produced all over this area, and for miles beyond it, a sea of considerable depth, and evidently far removed from any land surface; and it may be affirmed, without fear of contradiction, that it is impossible for an island to have existed within a few miles of the present escarpments without some of its sands and clays being washed down into the Chalk and thereby altering its composition.

It is, however, only fair to say that Major Marriott has privately informed me that an island is not essential to his hypothesis, and that a submerged bank will serve as well. But even with this important modification the theory is open to the general objection that the Weald does not stand alone, and that there are many other anticlines from which, as most of us believe, the Chalk has been removed. It is these that form, by analogy, one of the *primâ facie* obstacles in the way of the planation hypothesis; but they are even more formidable to the submerged bank. In the south we have the Isles of Wight and Purbeck, in which the Chalk attains a thickness of 1800 ft. or more, and at times stands almost vertical; so that, on any hypothesis, vast quantities must have been removed; and further north we find the series of Vales of Pewsey, Ham, and Kingsclere, of which the two last are of very minute dimensions. Now, either the Chalk once covered these anticlines, or it did not: if the former, we must admit a removal of it on such a vast scale as to undermine the very foundations of Major Marriott's Wealden theory; if the latter, then he must show us how a submerged bank is to be fitted into the tiny Vales of Ham and Kingsclere, the Greensand inliers of which are only four and five miles long respectively.

But the real test of all such theories is not so much their *primâ facie* probability, as the degree to which they explain existing facts; so we will pass on to consider those features which Major Marriott regards as proofs of his hypothesis (p. 592).

(1) "The absence in the area of the Weald of flints, the product of the chalk, except in the neighbourhood of the escarpments." From further remarks on p. 598 it would seem doubtful whether Major Marriott was aware that both Ramsey and Topley had noted this distribution and accepted it as evidence in favour of planation. As figs. 2 and 3 show, both theories reduce the amount of Chalk to be removed by subaerial forces to a mere wedge, the thick part of which corresponds with the escarpment, while the width depends in the one case (fig. 2) on the gradient of the hypothetical bank, and in the other (fig. 3) on the angle formed by the dip of the strata with the marine plain, or, in other words, on the amount to which the strata were tilted before planation. In the latter case the Lower Chalk rests conformably on the Upper Greensand throughout, and, as shown in the diagram, the wedge may not even yet be so completely removed as to expose the full thickness of the Chalk; but on Major Marriott's theory there is unconformity all across the floor of the wedge, and therefore the whole of it would have to be removed before existing relations of the strata could be established. But if, as he supposes, the surface of the Chalk has undergone little or no erosion, then the full thickness of it (600—1000 ft.) should be exposed at the escarpment, where, as a matter of fact, we seldom find more than half this amount.

The width of the wedge may vary within very wide limits on either hypothesis, but of the two, planation gives the greater latitude. With such a high dip as that of the Hog's Back, it might reduce the width to a few hundred yards, and only a part of that would be covered with flints (Upper Chalk). If, on the other hand, the submerged bank be credited with such gradients as are ordinarily found round our coasts where the strata are soft, the wedge would be inconveniently wide (20 miles or more), and flints, in any case, would extend over its whole surface. It is only by assuming an exceptional slope such as that of the Continental Edge off the west of Ireland (1 in 25) that we can reduce the width to the three or

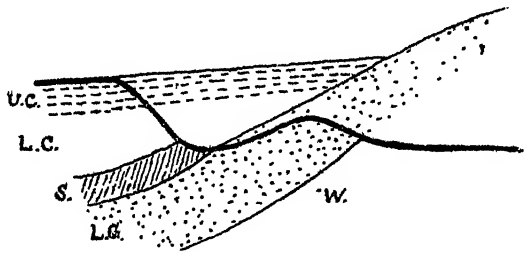


FIG. 2.—Section showing the original limits of the Chalk according to Major Marriott.

The thin line indicates the original surface, and the black one the present surface. U.C., Upper Chalk; L.C., Middle and Lower Chalk; S., Upper Greensand and Gault (Selbornian); L.G., Lower Greensand; W., Weald Clay.

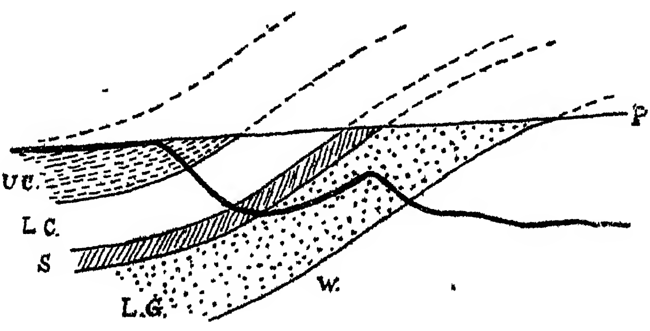


FIG. 3.—Section showing the subjects of marine planation on inclined strata.

P., Plain of marine denudation.

four miles to which the flints commonly extend, and it is doubtful whether even this is small enough to accord with the distribution in all cases.

(2) "The non-existence of any residual flint deposits on the plateaus and summits such as would undoubtedly result from the removal of the soft parts of the Upper Chalk." This is obviously only another aspect of the previous question. Against planation it has no weight at all, for the sea which removed the Chalk, etc., may well have removed the flints also. Nor is it really in the least fatal to the Chalk Dome theory; for *ex hypothesi* thousands of feet of strata must have been denuded from the "plateaus and summits"; the materials being dragged down to lower and lower levels, but always with a tendency to work outwards (as the

rivers do) towards the periphery. In such a long process all traces of the topmost strata may well have vanished, and in some of our dissected gravel-capped plateaux (*e.g.* near Farnham) we have proofs of how quickly the materials of such gravels often disappear. But in fact flints are sometimes found at great distances from the escarpment, having been reported from Horsham,[1] on the very axis of the anticline; and while such rare occurrences are quite in keeping with either of the older hypotheses, they appear to me absolutely fatal to Major Marriott's.

(3) "The presence of chert and ragstone, and ferruginous sandstone peculiar to the Lower Greensand spread over parts of the highest of the chalk plateaus." And we are told on p. 598 that "the significance of this appears to have escaped notice." Now, if there had been a Wealden Island (which we have seen to be out of the question) in Cretaceous times, there should have been chert, etc., *in* the Chalk itself; and even on the assumption of a submerged bank of Lower Greensand and other strata, it is strange indeed that the lower Eocene beds, which abound in rolled chalk flints, contain no chert. As most of these fragments were admittedly (p. 601) deposited in Pliocene times, all that they really prove is that the existing drainage system was not yet fully established,[2] and this has been adduced as evidence in favour of planation.[3]

(4) "The perfectly simple history of the 'transverse' valleys on this assumption. The explanation of these valleys is avowedly a great difficulty with all other theories." It would be interesting to learn where this "avowal" is to be found, and where these valleys are "so often quoted as presenting a geological difficulty" (p. 602). The general principles governing the formation of such valleys, as laid down more than half a century ago by Greenwood, Jukes, and others, have for many years been accepted all over the world; and, although here and there a dissentient voice may be raised (Mr. F. R. Bennett is quoted as not fully satisfied), I am not aware that any one has definitely formulated any objections. And when we consider the matter more closely, is there really any *physical* (as distinct from *geological*) difference in the starting-points offered by Major Marriott's theory and that of planation? In both of them the valleys originate on the central (Hastings) beds, and when the Chalk emerges it is the thin edge of the wedge which first appears, and across this the transverse streams prolong their courses without opposition. The subsequent deepening of these valleys and concomitant excavation of the longitudinal ones also appears to me to be the same in both cases. I gather, however, that Major Marriott sees a "crucial" difference between the two, and he speaks of the streams descending from the higher ground "making frontal attacks on the chalk and spreading east and west until they found suitable exits" (p. 593). That is a process which I do not profess to understand, but it does not appear to account for the initial stages of the escarpments.

The whole question of these valleys is closely bound up with that of the escarpments, and here again it is necessary to remember that the Weald is not really unique, and that there are many other examples of chalk hills being cut through by transverse rivers. On the one hand we have large rivers like the Thames at Goring, and the Dorset Stour near Blandford, which probably originated on coastal plains closely comparable to the hypothetical marine plain of the Weald; and on the other we see the tiny inliers of Ham and Kingsclere, where the streams

[1] Topley, *op. cit.* p. 200.

[2] See Bonney, *The Work of Rain and Rivers* (Camb. Manuals of Science), p. 106.

[3] *Quart. Journ. Geol. Soc.* vol. lxvi. 1910, pp. 648 *et seq.*

appear to have arisen on the dome of Chalk.[1] Both the older hypotheses are therefore represented; but it does not seem easy in either case to substitute for them the theory of submerged banks. And if such a postulate is considered necessary in the case of Chalk, what of escarpments and transverse valleys in other formations? In the north-west corner of the Weald the Lower Greensand forms an escarpment the summit of which is higher than that of the Chalk, and the river Wey cuts impartially through them both. In many other parts of the world whole series of parallel escarpments are thus traversed, and although they are perfectly intelligible on either of the older hypotheses, I find them very difficult to harmonise with Major Marriott's. Is a submerged bank necessary for one formation (the Chalk) and not for the others, or must we postulate a series of banks, successive in time and place, for each of them in turn?

We have now examined each of the four "proofs" on which Major Marriott rests his theory; and it will be seen that he does not bring forward any new observations in support of it, but relies entirely on its capacity to explain phenomena with which we have long been familiar. In three out of the four cases he hardly seems to realise that other explanations have been offered; and in the fourth he speaks vaguely of "difficulties" in the older interpretations, without attempting to define their nature. It is submitted that in no single case does the new theory meet the facts better than the old, and that in several respects it is distinctly inferior.

[1] *Geol. Survey Memoirs* for "Hungerford and Newbury," p. 79; and "Basingstoke," p. 80.

ESSAY-REVIEWS

SIR W. RAMSAY, by FREDERICK A. MASON, B.A., Ph.D.: on **Sir William Ramsay, K.C.B., F.R.S.** Memorials of his life and work, by SIR WILLIAM A. TILDEN, F.R.S. [Pp. xvi + 311, with 5 illustrations and a frontispiece.] (London: Macmillan & Co., 1918. Price 10*s*. net.)

SOME ONE—Lavoisier, perhaps—once remarked that he felt it his duty to be a good citizen first and a scientist afterwards, and this standpoint appears also to have been that adopted by Sir William Ramsay, for right up to the very end of his life he concerned himself actively with various movements connected with national welfare, particularly with regard to university education and research.

The story of his life is, of course, especially interesting to chemists, but Sir W. Tilden has succeeded admirably in producing a work which can be read and appreciated by all. Ramsay's bent towards science was undoubtedly inherited, in part, at all events, for he himself wrote in a short autobiography, which he contributed as an introduction to a German translation of one of his books (*Vergangenes und Künftiges aus der Chemie*), "Whatever chemical talent I possess is an inheritance from my ancestors on both sides."

His grandfather, William Ramsay, was a chemical manufacturer—junior partner in the firm of Arthur & Turnbull, makers, amongst other things, of the well-known "Turnbull's Blue"; he was an excellent practical chemist, published some original papers on chemistry, and founded the Chemical Society of Glasgow in 1800. He married Elizabeth Crombie in 1809, there being four children of the marriage—Eliza, William (father of Sir William), Andrew Crombie (who later became Sir Andrew Ramsay, the distinguished geologist), and John.

The eldest son, William, was a man of scientific tastes and culture, who devoted himself to engineering until an accident to one of his eyes compelled him to change his occupation. Somewhat late in life, at the age of forty, he married Katherine Buchanan, and on October 2nd, 1852, a son was born to them at Queen's Crescent, Glasgow.

The parents carefully refrained from spoiling their child, but "he was a great deal with them, heard them talk, and insensibly copied them, so that, as a little boy, he used rather grown-up words and ways of speaking. He played quietly with his toys, and carried on his child's life alongside of theirs, thinking his own thoughts and only coming out of his dreams when actually spoken to." He does not appear to have troubled himself much with the usual boys' games, but preferred amusements having a practical interest, such as rigging out toy boats, and so on, always on some plan of his own. He was fond of reading and music—pursuits that preserved their fascination for him right through life. His remarkable powers as a linguist were early developed, and M. M. Pattison Muir, in his reminiscences, recalls Ramsay's method of combining interest and duty during the long sermon in a Glasgow church by reading a French or German Bible, and so developing his knowledge of those languages!

As is, perhaps, well known, the future chemist's introduction to his life-work arose largely from a desire to make fireworks. He broke his leg at school (playing football), and, to enliven his convalescence, he read Graham's *Chemistry*, and performed a few simple experiments to while away the time.

On beginning his studies at Glasgow University, he entered Tatlock's laboratory, and in addition attended the lectures of Prof. T. Anderson. His interests also led him to study physics under Sir William Thomson, and geology under John Young; but his time was chiefly devoted to chemistry and mathematics.

On the later stages in his formal education we need not linger, beyond mentioning that he spent a couple of years at Tübingen, where he graduated under Fittig. Whilst there he met Prof. Remsen, and formed a friendship with him which lasted right to the end. Ramsay was always such an indefatigable worker, that it is amusing to find him writing to his father from Tübingen in 1872: "You appear to think I don't like chemistry as much as I used to. It is quite a mistake. I only object, as I always do, to too much work. I was up this morning, for example, at 5.30 and studied and took my breakfast from 6-7,—a class from 7-8, one from 8-9, from 9-3 laboratory (I lunch now to have more time for work, and don't dine till 6), and from 3-5 I studied, then from 5-6 lecture, and then I dined. And now at 8 I must start again. It is simply all work and no play, except on Thursday afternoons; but Thursday evenings I work as hard as ever."

His later career can be summarised by noting that he became successively assistant in Anderson's College, Glasgow, Professor of Chemistry at Bristol, and then at University College, London, where he remained until his resignation in 1912—four years before his death, in July 1916.

Sir W. Tilden has refrained from devoting too much space to details of the purely scientific labours of Prof. Ramsay (which can, after all, be seen by those interested in the pages of the Chemical Society's journal), but has dealt rather with the personal side of the question: the general account given in Chapter V. of his work on the gases of the atmosphere has, indeed, all the elements of a romance, not omitting the villain in the form of anonymous attacks on the authenticity of his results, and his right to claim them.

The general trend, indeed, of the biography is to show us to some extent Ramsay the man, genial, quick-witted, and far-seeing, rather than simply Ramsay the scientist: his work on the foundation of Bristol University and his unremitting efforts to obtain the much-desired Government Grant, are fully and clearly dealt with, as also his other efforts on behalf of education in this country and abroad.

In some respects the last four chapters, entitled respectively, "Later Years," "Views on Education," "Notes on Travel," and "The End," are perhaps the most interesting in the book.

Sir W. Ramsay was always a strenuous opponent of purely competitive examinations, and, in particular, he hated "the further abuse of awarding scholarships as the results of examinations. The pauperisation of the richer classes is a crying evil; it must some day be cured Let scholarships be awarded to those who need them, not to those whose fathers can well afford to pay for the education of their children. 'Pot-hunting' and philosophy have absolutely nothing in common."

Further, he placed the utmost emphasis upon the need for original investigation. "I would give a degree for investigation. It isn't the originality so much that is required; it is the training in methods of thought and means of executing and realising ideas. That is what tells in life." "It is our carelessness in this respect (of research) . . . which has made us so backward as compared with some

other nations. It is this which has made the vast majority of our statesmen so careless, because so ignorant of the whole frame of mind of the philosopher; and which has made it possible for men, high in the political estimation of their countrymen, to misconceive the functions of a university."

Ramsay always placed great weight upon the necessity for encouraging research both in the universities and in industry, and it is very probable that the scheme for founding a Ramsay Memorial Fellowship would have met with his warm approval.

His views are becoming more and more accepted, and there is yet hope that we may in time produce statesmen and politicians with clear realisations as to the country's needs and, what is more important, with definite scientific plans for carrying them into effect.

An omission we note in the book is the absence of reference to certain of Sir W. Ramsay's last public acts; one, the letter to the *Times* at the beginning of the war, on the cotton controversy, in which he emphasised the necessity for cutting off Germany's supply of cotton for explosives, a question in which he and his old friend, Sir W. Macara, played a prominent part; another—omitted, perhaps, owing to the modesty of the author—namely, the speech made at a lecture given by Sir W. Tilden in 1915 on the supply of chemicals to Great Britain, in which Ramsay discussed the unprincipled business methods of the Germans and the necessity for combatting them; and, lastly, an address on "The National Organisation of Science," given before the British Science Guild in July 1915. They were probably his last public utterances, and show us, if indeed we needed to be shown, that in him we have lost, not only a great scientist, but a man of clear-visioned statesmanship, seeking, above all, the welfare and progress of his country.

REVIEWS

MATHEMATICS

Analytic Geometry and Calculus. By FREDERIC S. WOODS and FREDERICK H. BAILEY, Professors of Mathematics in the Massachusetts Institute of Technology. [Pp. xii + 516.] (Boston, U.S.A., and London: Ginn & Co., 1917. Price 12*s.* 6*d.* net.)

THIS work is a revision and abridgment of the authors' *Course in Mathematics for Students of Engineering and Applied Science*, and the authors have omitted from the previous work such subjects as determinants, much of the general theory of equations, the general equations of the conic sections, polars and diameters related to conics, centre of curvature, evolutes, certain special method of integration, complex numbers, and some types of differential equations. Further, the material is rearranged so that the first part of the book contains methods for the graphical representation of functions of one variable, both algebraic and transcendental, while analytic geometry of three dimensions is treated later when it is required for the study of functions of two variables. "The transition to the calculus is made early through the discussion of slope and area, the student being thus introduced in the first year of his course to the concepts of a derivative and a definite integral as the limit of a sum" (p. iii).

The book is undoubtedly useful for engineering students, for whom it will form a sort of compendium of what pure mathematics is necessary for their purpose. But it would seem that an exception is to made in the way differentials are treated (pp. 141-2). They are "not to be thought of as exceedingly minute" (p. 143); yet, in the treatment of slopes and areas, we naturally find that the finite increments of x and y are to proceed indefinitely to zero (pp. 136, 141, 143 4). Also differentials are "infinitesimals," and may, under certain circumstances, be disregarded (p. 261). It would, it seems, be much better to introduce the calculus as a method of very close approximation. Other points of criticism, if, indeed, the authors' wish consistently to replace guesswork by logic, are that the comparison test for convergence assumes implicitly a previous definition of irrational numbers (p. 407), and that there are difficulties for a student,--an appeal to the authority of future and more rigorous work,—in the method given for finding e (p. 199), and in the treatment of Fourier's series (p. 429). The examples given to illustrate how differential equations present themselves (for example, pp. 438-41, 450-1, 453-4) seem to be very good, and it may be remarked that in many cases they often arise from the actual problems on which mathematicians actually worked in the last years of the seventeenth century.

PHILIP E. B. JOURDAIN.

Theory of Maxima and Minima. By HARRIS HANCOCK, Ph.D. (Berlin), Dr.Sc. (Paris), Professor of Mathematics in the University of Cincinnati. [Pp. xiv + 193.] (Boston, U.S.A., and London: Ginn & Co., 1917. Price 10*s.* 6*d.* net.)

THE first who made a distinction between a maximum and a minimum of a function seems to be Leibniz (p. 3), and it was Maclaurin who first gave a correct

method of distinguishing maxima from minima (p. 4). When Lagrange came to consider the maxima and minima of functions of more than one variable, he made an error in which he was followed by all writers until Peano, in his *Calcolo* of 1884, founded on the lectures of Gennochi and translated into German in 1899, pointed out this error (pp. iv–v, 33). The objection contained in Peano's work showed that the entire former theory of maxima and minima needed a thorough renovation, and in the main this work was the original source of the theories developed by Scheeffer, Stolz, von Dantscher, and others, of which the volume under review gives a detailed and excellent account. It may be noticed that Peano drew attention to the mistaken criterion given by Serret and his followers; the author shows that this criterion goes back to Lagrange (pp. v, 33).

The present volume arose from an outline of the theory of maxima and minima founded on lectures of Weierstrass which was published by Prof. Hancock in 1903. It treated the cases where the functions are everywhere regular and where the algebraic forms are either definite or indefinite. The fuller treatment of the cases where only one-sided differentiation enters led ultimately to the exposition given here (p. iv). The chapters are devoted to functions of one variable, functions of several variables, functions of two variables, the theories of Scheeffer and others, functions of three variables, maxima and minima of functions of several variables that are subjected to no subsidiary conditions, theory of maxima and minima of functions of several variables that are subjected to subsidiary conditions, relative maxima or minima, special cases, certain fundamental conceptions in the theory of analytic functions.

There seems to be no reason for regarding Weierstrass's methods as founded on the work of Bolzano (cf. p. 12). In the interesting discussion of Gauss's mechanical principle as an application of the method of maxima and minima (pp. 151–3), the reason for the fact that the accelerations, and not the positions or velocities, are to be varied might have been given in detail: it is rather puzzling to a student—as it was to Lipschitz. This is a very valuable monograph.

PHILIP E. B. JOURDAIN.

Les Sciences Mathématiques en France depuis un demi-siècle. By ÉMILE PICARD. [Pp. vi + 25.] (Paris: Gauthier-Villars et Cie., 1917. Price 2 francs.)

THIS short and well-written account of the progress in pure mathematics made in France from the time in the middle of the nineteenth century when Fourier, Cauchy, and Galois had opened up fruitful paths which led, to a great extent, to the views of the country of mathematics obtained in the first few years of the twentieth century, contains chapters on the theory of analytic functions, differential equations, theory of numbers, algebra and geometry, and the theories of functions of real variables and aggregates. Although the account is, of course, largely of the nature of a catalogue of names, the names are chosen, with the skill one would expect from the eminent author, so as to give a really good idea, to one who knows something of the work of those men, of the essential features of the progress of mathematics in France. Perhaps the dismissal of the paradoxes of the theory of aggregates as "having made floods of ink to flow," the comparison of these difficulties with those which gave rise to the quarrels of the schoolmen (pp. 20–21), and the complimenting of French mathematicians on their limitation of view to certain parts of mathematics and their avoidance of "excessive symbolisms" (pp. 23–4) seem rather superficial.

PHILIP E. B. JOURDAIN.

CHEMISTRY

Stoichiometry. By SYDNEY YOUNG, D.Sc., F.R.S., Professor of Chemistry in the University of Dublin. Text-books of Physical Chemistry, edited by SIR WILLIAM RAMSAY. Second edition. [Pp. xii + 363, with 93 illustrations.] (London: Longmans, Green & Co., 1918. Price 12*s.* 6*d.* net.)

THIS is the second edition of Prof. Young's book. The work is already so well known that we need do little more than draw attention to the appearance of the new edition. The subjects dealt with include the laws of chemical combination, the periodic law, the determination of atomic and molecular weights, the properties of gaseous, liquid, and solid systems, including mixtures and dilute solutions. As regards new material, an account is given of recent work upon the accurate determination of atomic weights, the position of the radio-active elements in the periodic table and Soddy's theory of isotopes, the application of the falling drop method to the determination of surface tension, the measurements of osmotic pressure, by Morse and his collaborators in America, and by the Earl of Berkeley and F. G. J. Hartley in England, and the work of McBain upon "sorption" of gases by solids.

Considerable space is given to the consideration of gaseous and liquid systems and the principle of the continuity of state. In these subjects the author gives us an account of developments in which he himself took a large share. For this reason it will be regarded by many as the most attractive and interesting portion of the book.

The subject of Stoichiometry is admittedly not one which it is easy to define with precision. In this matter, the scope of the work as planned by Prof. Young—himself one of the greatest living authorities upon the subject—is instructive. There is little that calls for criticism. One would have liked to see, perhaps, some account of the elucidation of crystal structure by X-rays, and a fuller consideration of the recent work on the atomic heats of solids in the light of the quantum hypothesis. But this would have added considerably to the length of the work.

The book may be warmly recommended to all interested in physical chemistry.

W. C. McC. LEWIS.

Practical Organic and Biochemistry. By R. H. A. PLIMMER. [Pp. xii + 636, with coloured plate and other illustrations in the text. New and revised edition.] (London: Longmans, Green & Co., 1918. Price 18*s.* net.)

THE previous edition of this book was published in 1915, and was fully reviewed in this Journal. The author states in the preface that "several sections have been rewritten and some new methods of preparation and analysis have been incorporated, "but as the general arrangement and scope of the subject matter has not been materially altered, further comment is perhaps unnecessary. That the last edition was exhausted in so short a time shows that the book is in good demand, but it is to be regretted that the publishers have found it necessary to raise the price so much above the previous one.

P. H.

A Handbook on Antiseptics. By HENRY DRYSDALE DAKIN, D.Sc., F.I.C., F.R.S., and EDWARD KELLOGG DUNHAM, M.D. [Pp. x + 129.] (New York: The Macmillan Company, 1917. Price 7*s.* net.)

THIS little handbook aims at giving a concise account of the chief chemical antiseptics which have been employed and found useful in the present war; fore-

most among these stand the chlorine antiseptics which comprise both the inorganic hypochlorites and the organic chloramines. Much of the progress in our knowledge of these substances is due to the researches of Dr. Dakin, and a description of their preparation, properties, and uses from his pen is naturally to be welcomed. The book is divided into eight chapters dealing with the various different types of antiseptics, the methods of testing them, as well as the application of certain of them to the disinfection of carriers, of water, and of hospital ships. The authors frequently emphasise the necessity for scientific treatment of the subject, pointing out that disinfection is really a chemical reaction obeying the law of mass action, and having a definite reaction velocity. Failure to recognise the instability of a given antiseptic or its tendency to react with the tissues may lead to erroneous ideas as to its efficacy. To quote the author's words, "A knowledge of the speed with which a disinfectant acts is essential to an understanding of the conditions under which it may be appropriately used." Thus, while rapidly acting aqueous hypochlorites may be excellent for intermittent instillation into large wound cavities, they would, if applied as wet dressings, which are seldom removed, be practically useless, owing to their decomposing after only a few seconds' contact with the skin.

The book is addressed primarily to surgeons and others in the United States who are about to take up military duties connected with the care of the wounded, but no doubt many people in this country will gladly avail themselves of this handy little book for obtaining what would otherwise be rather scattered information from a number of different sources.

P. H.

Monographs on Industrial Chemistry. Edited by SIR EDWARD THORPE, K.C.B., F.R.S., Emeritus Professor of General Chemistry in the Imperial College of Science and Technology, South Kensington, and formerly Principal of the Government Laboratory, London.

(1) **Organic Compounds of Arsenic and Antimony.** By GILBERT T. MORGAN, D.Sc., F.R.S., etc., Professor of Applied Chemistry, City and Guilds Technical College, etc. [Pp. xx + 376.] (London: Longmans, Green & Co., 1918. Price 16*s*. net.)

(2) **Edible Oils and Fats.** By C. AINSWORTH MITCHELL, B.A., F.I.C. [Pp. xii + 159.] (London: Longmans, Green & Co., 1918. Price 6*s*. 6*d*. net.)

THOSE familiar with the developments of chemistry and chemical industry at home and abroad are fully cognisant of our dependence on Germany for a very large part of our technical literature, particularly of the bibliographical variety.

On the one hand the Teuton mind is in some ways better adapted to the slow and measured labour involved in compiling such works than is the more straightforward and "hit-or-miss" Englishman, and on the other hand the enormous oversupply of trained chemists in the Fatherland willing to work for a very modest wage has facilitated the production of chemical works of reference, handbooks and monographs ranging from the ponderous tomes of Friedländer's *Fortschritte in der Teerfarbenfabrikation* or Winther's weighty volumes to the paper-covered publications of the Ahren's "Sammlung" type. In this country but little has been done hitherto to fill the gap; but now, at last, owing to the energy and initiative of Sir T. E. Thorpe, a new series of up-to-date Monographs on Industrial Chemistry is making its appearance, and in so doing is sounding the knell of the German quasi-monopoly.

It is hardly a coincidence that, of the twenty-eight titles announced, sixteen deal with organic chemistry, and of these ten are on the chemistry of dyes, and should prove of immense value in hastening the renaissance of the colour industry in the land of its birth. The immediate object of the series may be best explained in the editor's own words:

"During the last four or five decades the applications of chemistry have experienced an extraordinary development, and there is scarcely an industry that has not benefited, directly or indirectly, from this expansion. Indeed, the science trenches in greater or less degree upon all departments of human activity. Practically every division of Natural Science has now been linked up with it in the common service of mankind. So ceaseless and rapid is this expansion that the recondite knowledge of one generation becomes a part of the technology of the next. Thus the conceptions of chemical dynamics of one decade become translated into the current practice of its successor; the doctrines concerning chemical structure and constitution of one period form the basis of large-scale synthetical processes of another; an obscure phenomenon like catalysis is found to be capable of widespread application in manufacturing operations of the most diverse character. This series of Monographs will afford illustrations of these and similar facts, and incidentally indicate their bearing on the trend of industrial chemistry in the near future. They will serve to show how fundamental and essential is the relation of principle to practice. They will afford examples of the application of recent knowledge to modern manufacturing procedure. As regards their scope, it should be stated that the books are not intended to cover the whole ground of the technology of the matters to which they relate. They are not concerned with the technical *minutiæ* of manufacture except in so far as these may be necessary to elucidate some point of principle. In some cases, where the subjects touch the actual frontiers of progress, knowledge is so very recent and its application so very tentative that both are almost certain to experience profound modification sooner or later. This, of course, is inevitable. But even so such books have more than an ephemeral interest. They are valuable as indicating new and only partially occupied territory; and as illustrating the vast potentiality of fruitful conceptions and the worth of general principles which have shown themselves capable of useful service."

(1) Prof. Morgan's authority to write on the subject of the organic compounds of arsenic and antimony is too well known to need emphasis, and in the present volume he has not only produced a readable and comprehensive book on the subject, but in so doing he has set a standard for the series which augurs well for its success. As the author reminds us in the preface, the organic derivatives of arsenic appeal to the scientific public for two widely different reasons. From the historical standpoint these substances are of considerable interest because they have been under investigation throughout a period of time coeval with the birth and development of modern chemistry. Successive generations of chemists have examined these compounds from points of view which varied with the gradual evolution of chemical science, and the results of their researches have played an important point in the establishment of current theories of the molecular constitution of matter. Additional importance is conferred on the subject by the circumstance that very early in the study of organic arsenical compounds it was realised that in these synthetic products the physician has at his disposal substances of great physiological potency. It is chiefly this medicinal attribute of organic arsenicals which has evoked the more recent activities in the synthesis of organo-metalloidal compounds. These utilitarian investigations have not been restricted to organic arsenicals, but have extended to the corresponding derivatives

of antimony, and accordingly these related products are also discussed in the present monograph.

The subject is treated to a certain extent in an historical manner from the earliest investigations, such as Cadet's "fuming arsenical liquid" and Bunsen's Cacodyl, right up to the work of Ehrlich and Benda on Salvarsan, and the later work of Bertheim, Karrer, Danysz, Mouneyrat, and others, not omitting Prof. Morgan's own valuable contributions to the knowledge of this most important group of organic substances. The whole book is a complete and satisfactory reply to those somewhat shallow-minded critics who look upon organic chemistry as a mere "molecule-juggling"—to use the half-contemptuous expression of Prof. Donnan. There are few branches of human activity which have not, during the last few decades, learnt valuable lessons from the study of organic chemistry, and no profession, perhaps, has profited more than the medical calling, on which the organic chemist has showered riches beyond compare.

Prof. Morgan's work should prove of the greatest value to all chemists and physicians, to all of whom the excellent bibliography given at the end should be very useful.

(2) Mr. Mitchell's work on the Edible Oils and Fats possesses a very topical interest at the moment not only to chemists but to every one who has a butter ration-paper! The author has endeavoured with success to give a concise outline of the chemical composition and properties of the more important oils and fats, together with a description of the methods of extracting them from the crude materials, and of purifying and preparing them for food purposes. A chapter dealing with the physical and chemical methods of examining edible oils is also included, and tables of typical so-called "constants" are given with the descriptions of the individual fats, with the object of enabling any one who has no specialised knowledge of the subject to understand the technicalities of an analysis. With this end in view, the principles rather than the working details of well-known analytical methods have been described. The first three chapters deal with the nature, properties, and composition of fats, their extraction and purification, and their constants. In Chapter IV. a good summary of the most important methods of examination is given, whilst the remaining five chapters deal with the characteristics of individual fats and oils, with hardened oils, and with butter and margarine. A useful bibliography is given on pp. 124-53, and the book appears to be well indexed.

It is somewhat difficult to criticise a work of this kind in which a meritorious attempt is made to condense a very large subject in a little space. When one recalls the size of Lewkowitsch's standard work on fats and oils, the task of condensing it is seen to be no light one. Naturally enough, the book tends to suffer a little from the inevitable compression, and shows some signs of congestion here and there, but that is, perhaps, no disadvantage, bearing in view the particular purposes for which the book was compiled. With regard to the question of hydrogenised fats discussed upon pp. 108-15, no new patents appear to be discussed of later date than 1912; one might perhaps expect some account of the more recent patents on the subject during the last five years; in certain other respects also the book does not appear to be quite as up to date as might be hoped. Various illustrations of apparatus for the extraction and analysis of fats are given. The book should prove of service to those who have to deal with edible fats and oils.

FREDERICK A. MASON.

The Alkali Industry. By J. R. PARTINGTON, M.Sc.: being one of a series of volumes giving a comprehensive survey of the Chemical Industries, edited by SAMUEL RIDEAL, D.Sc., F.I.C. [Pp. xvi + 304, with 63 figures in the text.] (London: Ballière, Tindall & Cox, 1918. Price 7*s*. 6*d*. net.)

THE rapid development of chemical industry in this country during the war has made the production of up-to-date volumes on the subject a matter of necessity, as it is not every one who has either the time or opportunity to consult technical journals and the larger works of reference.

For many chemists, in particular advanced students and works chemists, the volumes comprised in this series should be of great use.

Mr. Partington's work on the alkali industry is one of the first of the series to be issued, and appears to represent a good deal of careful work, particularly with regard to the compilation of the lists of literature references at the end of each chapter. The subjects dealt with are the Salt Industry, Sulphuric Acid, Soda, Electrolytic Processes, Chlorine and Derived Products, Nitric Acid, Ammonia and Ammonium Salts, Oxidation of Ammonia, Utilisation and Economy of Sulphuric Acid, Potassium Salts, Iodine and Magnesium.

One cannot help regarding the book with somewhat mixed feelings as, on the one hand, much care has evidently been devoted to the writing of the volume, which is certainly readable; but, on the other hand, the mistake appears to have been made of attempting too much; the subjects considered are really too large and numerous to be dealt with in detail within the pages of a moderate-sized book, so that whilst sulphuric acid, for instance, is treated fairly adequately, a vitally important question, such as the supply of potash from other sources than Stassfurt—*e.g.* blast-furnace gases, etc.—is dismissed in a couple of pages, and electro-chemical processes have to be satisfied with eight pages—hardly a sufficient treatment.

Again, the information given is not always quite up-to-date; thus, in discussing the cost of producing synthetic nitric acid, no mention whatever is made of the important paper published by the United States Government, in which, for the first time, really reliable figures of costs are given; again, no mention is made of the new method which is being adopted in Germany of preparing ammonium sulphate from ammonia and gypsum.

The book should serve a useful purpose as an introduction to the chemistry of the alkali industry, but one cannot help feeling that rather too much has been undertaken in endeavouring to compress even an outline of the entire alkali industry into one volume, and that it would have been better if the subject had been allowed somewhat fuller treatment.

F. A. M.

The Zinc Industry. By ERNEST A. SMITH, Assoc. R.S.M., Deputy Assay-Master, Sheffield. [Pp. viii + 223, with 4 plates and diagrams in the text.] (London: Longmans, Green & Co., 1918. Price 10*s*. 6*d*. net.)

THE recent history of the zinc industry is too well known to need emphasis: the story of how, on the outbreak of war, we found ourselves face to face with a famine' in the spelter so necessary for munitions, the discovery of the German strangle-hold on the industry—the efforts made both in Australia and in this country to break this monopoly, culminating in the recent Non-Ferrous Metal Bill—are matters that concerns us all, scientists and laymen alike, and a book such as that under review, which deals with the whole subject from the technical, scientific,

and economic points of view, is well worth perusal by all who are interested in the national welfare.

Although, of course, the greater part of the book deals with the technical side of zinc extraction, other chapters, such as those on the physical and chemical properties of zinc, and on the Future of the Zinc Industry in Great Britain, are given, and should make the volume of value to the chemist and economist as well as to the metallurgist. Several clear photo-micrographs are included and a useful bibliography is appended. The index might have been made a trifle fuller.

F. A. M.

The Manufacture of Intermediate Products for Dyes. By JOHN CANNELL CAIN, D.Sc., Editor of the *Journal of the Chemical Society*, etc. [Pp. xii + 263, with 25 illustrations.] (London: Macmillan & Co., 1918. Price 10*s*. net.)

THOSE who hope to find in Dr. Cain's book "*The* Secret of the Aniline Dyes" will be doomed to disappointment, yet there can be no doubt that there is contained within the 263 pages of this modest-sized work far more valuable information than could be hoped for in any "257 secret recipes" or other alleged short cut to the successful exploitation of the dye situation.

It will be recalled that one of the first problems which faced the manufacturer of dyes in this country on the outbreak of war was the provision of the so-called "intermediates" from which the final dyes are made. When it is remembered that about 85 per cent. of the capital outlay in a dye-works is in plant for the production of intermediates, it will be realised that a work such as Dr. Cain's, which summarises the patent literature on the subject, is of the greatest value.

The special objects of the book are summed up in the preface: "It is . . . of prime importance to the would-be manufacturer to have in a convenient form detailed information as to the preparation of the materials he proposes to make, so that he may be sure he will not spend time and money in rediscovering, perhaps, some process that may have been already elaborately described in an obscure book or periodical. It appeared to the author that a concise account of the literature dealing with the manufacture of intermediate products for dyes might therefore be of considerable use, and in this book he has endeavoured to present such an account in detail so as to render it unnecessary to refer to the original descriptions. It may, perhaps, be stated that he doubts whether any single library in England contains the whole of the literature consulted."

It is unnecessary to discuss the various chapters as the entire subject has been covered in great detail, the information appears to be very up-to-date, and a perusal of the pages is likely to be of interest to purely academic chemists—if any such exist now—as much as to manufacturers and students. As an example we may quote the various patented processes for the catalytic reduction of nitrobenzene to aniline on pp. 44, 45, though we may note that the author has not included certain less important patents on the subjects.

Here and there Dr. Cain appears to have overlooked one or two patents, such as on p. 139, where, in referring to the production of *o*. nitrobenzaldehyde from *o*. nitrophenylnitromethane he makes no reference to the original patent (E.P. 24871/1910). Surely, also, it would be as well to give the patent references (E.P. 6076/1911 and 17985/1911) to the production of *o*. nitrophenylnitromethane itself? Again, one seeks in vain for any reference to the production of Isatine, which is of some importance in connection with modern vat-dyes.

The illustrations are clear and concise and will be recognised by many who are familiar with the subject: at the same time one cannot altogether agree with Dr. Cain when he says (p. vi) that it is "superfluous to reproduce figures of ordinary plant . . . which are illustrated in the advertisement columns of periodicals." It would undoubtedly be of great interest, particularly to the student, to see photographs or diagrams of the actual standardised plant made by various manufacturers, and which cannot always be found in the advertisement columns just when wanted; these, however, are minor points.

The book should prove of great value as a time saver at the present moment, and one may look forward with confidence to seeing it run through many editions.

F. A. MASON.

Chemical Combination among Metals. By DR. MICHELE GIUA, Professor of Chemistry in the Royal University of Sassari, and DR. CLARA GIUA-LOLLINI; translated by GILBERT W. ROBINSON, University College, Bangor. [Pp. xiv + 341, with 207 figures in the text.] (London: J. and A. Churchill, 1918. Price 21*s*. net.)

UNTIL recently, the ability of the metallic elements to combine with each other was hardly considered seriously, and, indeed, their inability to do so was looked upon as one of the most obvious characteristics of this class.

Since the beginning of the present century, however, many advances, both on the scientific and technical sides of the question, have been made, largely due to the introduction of Tammann's thermal method, and, above all, to the application of W. Gibbs' Phase Rule to the examination of metallic systems.

The present volume, translated from the Italian original, which was awarded the prize of the Cagnola Foundation by the Royal Lombardy Institute of Science and Literature, affords a useful summary of the present state of our knowledge of the subject.

Chapters are given on equilibrium diagrams, thermal analysis, and the nature and physical properties of inter-metallic compounds, the latter part of the book being devoted to the individual homopolar and heteropolar compounds, many useful diagrams illustrative of their properties being included.

In view of the ever-increasing demand for new alloys for special purposes, this book should prove to be of considerable value to those interested as showing, on the one hand, what is already known, and indicating, on the other, possible ne lines of research.

F. A. M.

AGRICULTURE

The Potato. By ARTHUR W. GILBERT, Ph.D., Professor of Plant Breeding, New York State College of Agriculture at Cornell University. Assisted by Mortier F. Barrus, Ph.D., Professor of Plant Pathology, New York State College of Agriculture at Cornell University, and Daniel Dean, formerly President of the New York State Potato Association. (Rural Science Series.) [Pp. xii + 318, with 29 text-figures and 16 plates.] (New York: The Macmillan Company, 1917. Price $1.50.)

IN the decade immediately preceding the war the potato was the world's largest crop, and the effect of the food shortage produced by the war has been, as every one is aware, to increase the importance of the potato, for, as is pointed out on the first page of this book, "a greater weight of potatoes can be produced to a unit

area than any other food crop." A book dealing successfully with such a subject is, then, to be specially welcomed at this time.

That the authors have succeeded in their object there can be no doubt. Prof. Gilbert states in the preface that the work is written especially "for the practical farmer and the student who may wish for concise information on the potato." It may also be recommended to the purely botanical student and teacher, and its perusal will scarcely be a waste of time to the research worker in plant physiology, who should find much in it that is stimulating and suggestive.

The introductory chapters dealing with statistics and history are laudably brief, but interesting. The next chapter, which deals with classification, contains a long table of varieties. Here the American standpoint of the book is very evident, for one looks in vain for many well-known British forms, such as Arran Chief and King Edward VII.

In Chapter IV. the principles of potato breeding are presented in considerable detail, the desirable attributes are discussed, and the methods of attaining improvement by bud selection, mutation (bud-sports), and crossing are considered. Prof. Gilbert considers the first two methods are within the scope of the practical grower, but wisely recommends leaving the more difficult question of hybridisation to the experiment station.

One way of bringing about increased productivity is thus by paying attention to hereditary factors; another way is to influence the environment. The latter is dealt with by Mr. Dean. The most important factors of the environment are temperature and rainfall, which in themselves are largely responsible for the lower yield of the potato in America as compared with Northern and Central Europe. Besides these climatic factors are the soil factors, texture, available plant food, etc., any one of which may act as a limiting factor, inasmuch as it may "place a limit of the size of the crop which cannot be removed by the others being favorable."

Other chapters deal with tillage and planting, the care of the growing crop, including the control of insects and plant diseases, and with harvesting, marketing, and storage. The various uses of the potato are also discussed, and the book closes with a chapter on the cost of growing potatoes. This latter part of the book is of course particularly applicable to American conditions.

The book is generally clearly written, although in a few cases there are sentences which leave one wondering what meaning the author intended to convey, as, for example, at the top of p. 7. Such instances are, however, few and negligible.

W. S.

The Book of the Rothamsted Experiments. By A. D. HALL, M.A., F.R.S., late Director of the Rothamsted Experimental Station, First Principal of the South-Eastern Agricultural College, now Secretary of the Board of Agriculture. Second edition, revised by E. J. Russell, D.Sc., F.R.S., Director of the Rothamsted Experimental Station. Issued with the authority of the Lawes Agricultural Trust Committee. [Pp. xl + 332, with 49 figures and 101 tables.] (London: John Murray, 1917. Price 10*s.* 6*d.* net.)

THE first edition of this book, published in 1905, was essentially an account of the experiments initiated and organised at Rothamsted by Sir John Lawes and Sir Joseph Gilbert, whose remarkable collaboration, lasting fifty-seven years, not only laid the foundation, but erected the superstructure of scientific agricultural in-

vestigation in Britain. As the author says in his preface, the book does not deal with investigations which only the professional scientific man can hope to follow, but rather with matters which any one concerned with the management of land, as farmers, market-gardeners, land owners or agents, can appreciate. The book is also intended for the agricultural student, to whom it indicates the sources of well-known statements and conclusions he will hear in lectures and see in text-books, and for the teacher and expert, for whom it furnishes a guide to the more detailed reports. Lists of references at the end of each chapter add to this sphere of the book's usefulness.

The systematic field experiments at Rothamsted were commenced in 1843, and complete records have been kept ever since, so that the agricultural history of all the experimental fields and plots is known in detail. It is to the empirical information derived from these field experiments that the present state of manuring science and practice is largely due. The *Book of the Rothamsted Experiments* deals chiefly with these classical field experiments. They form, of course, only a part of the investigations carried out at Rothamsted, and the results of other lines of inquiry also find a place in the book.

The new edition, besides bringing up to date the history of the experiments described in the first edition, contains two entirely new chapters concerned with more recent developments in soil science. The first of these, from the pen of Dr. E. J. Russell, is on "Recent Work on the Biochemical Processes of the Soil," and deals largely with partial sterilisation of soils; the second, by Sir Daniel Hall, is concerned with "Secondary Actions of Artificial Manures upon the Soil."

The whole book is a remarkable testimony to the enthusiasm of the Rothamsted workers, and helps the reader to realise the debt the agricultural community, and hence the whole nation, owes to that remarkable life-long collaboration of Lawes and Gilbert.

The author of the *Book of the Rothamsted Experiments* is not only our greatest authority on the soil and manuring problems, with which the experiments are so largely concerned, but his writings possess a clearness of expression and a literary style which are not wanting in the present work. The excellent manner in which the book is produced adds to the pleasure of reading it.

W. S.

ZOOLOGY

The Origin and Evolution of Life on the Theory of Action, Reaction, and Interaction of Energy. By HENRY FAIRFIELD OSBORN, Sc.D., LL.D., D.Sc., Ph.D. [Pp. xxi + 322, with 136 illustrations.] (New York: Charles Scribner's Sons, 1918. Price $3 net.)

DR. OSBORN, the writer of the present volume, is well known as a prolific writer on palæontology—a branch of science which provides more direct illustrations of Evolution than perhaps any other. His theories of the origin of mammalian teeth and the adaptive radiations of the various groups of vertebrates have in the main received wide acceptance. In this work, however, at any rate in the earlier and more thought-provoking chapters, he quits the domain of palæontology and even biology in an endeavour to discover the origin of life and the evolution of forms. All the most recent advances of physics and chemistry are drawn on to aid in this quest. Although this old, old problem is not solved, and we are afraid we must confess it is not, its restatement in modern scientific terminology is both valuable and interesting. Up to the present no explanation of the origin of living things

has been accepted as satisfactory by the philosophic biologist. Arrhenius's suggestion that life was introduced on to this earth at some remote period from somewhere in space is perhaps the least acceptable of all. It leads us nowhere, but leaves us blindly drifting through the ether in a search for imaginary conditions under which life might conceivably have originated with but the slenderest of data on which to build our concept of the physico-chemical environment on any other planet. It is our bounden duty to do our utmost to seek for life's commencement on this earth until it can be shown beyond doubt either that it could not have originated here or that it did start elsewhere. Darwin's theory, while giving a possible explanation of the origin of form differences, seems to depend entirely on chance, both for the origin of its variations and of life, and the former of these, at any rate, is not borne out by palæontological evidence. Bergson's *Elan vital* is a principle so mystic in its essence that it eludes all our efforts to grasp it.

Not merely does Osborn forsake the biological line of attack but adduces a number of cogent reasons against it ever yielding a solution to the problem which he claims must be sought along physico-chemical lines, and, since all questions of metabolism must be solved in this way, there is a great deal to be said for this point of view. Thus it is that to a biologist the introductory chapters prove provocative, for he is forced to relinquish thinking in terms of form and think in terms of the energy that produces the form. This thinking in terms of form and form transformation in the past has undoubtedly led to the formulation of laws of evolution, but they carry us no further in our search for causes, and to get at these we must think in terms of energy complexes, of which form is but the visible expression.

The idea of Action and Reaction have long been familiar to the physicist, as has the second law of thermodynamics, and the author's main thesis is that in applying these concepts to living matter another factor needs to be considered: "Every physico-chemical action and reaction concerned in the transformation, conservation, and dissipation of energy, produces also, either as a direct result or as a by-product, a physico-chemical agent of interaction which permeates and affects the organism as a whole or affects only some special part." It is this interaction which unifies and co-ordinates the activities of the organism as a whole, so distinguishing it from inorganic substances, and so it is necessary to trace the origin of this interaction. This idea is to a certain extent like the internal adjustment of Spencer.

This quest leads the author to a consideration of the inorganic environment, the age of the earth, the elements essential to life, and the possibility of chemical combinations of some complexity, ultimately leading to the evolution of some form like the very lowliest of the bacteria. Whether one agrees with them or not, it is impossible not to be interested in the speculations so well put forward. Once this territory has been passed, of course the speculations become less, and the way up to the higher animals and plants is indicated in the author's well-known expository manner, so that the latter part of the book forms an interesting account of the general evolutionary trends of the main groups, and is naturally fully illustrated by reference to palæontological discoveries.

We have to note one slip. In the footnote on p. 235 Dr. Elliot Smith is described as of the University of Glasgow, whereas he is at the University of Manchester.

In brief then we have here one of the most interesting biological books that has appeared for some years, and it is assured of a wide welcome from naturalists

and biologists the world over. The author undoubtedly has indicated the lines of advance and frankly faced the difficulties that are still to be solved.

C. H. O'D.

Fresh-Water Biology. By PROF. H. B. WARD and PROF. G. C. WHIPPLE, with the collaboration of a staff of Specialists. [Pp. ix + 1111, with 1547 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1918. Price $6, or £1 8*s.*)

THIS volume treats of the fresh-water life on the North American Continent, and it may be said from the outset that it is quite indispensable to any biological laboratory there and also to any worker in this subject. It should prove invaluable as a work of reference in schools where the teachers wish to pursue biological questions a little more deeply than is done in ordinary text-books of nature-study or natural history. Even in other countries it will be valuable because of its general introductory account of all sides of hydrobiology, the special introductions to the various chapters on groups of organisms and the bibliographies at the terminations of the various sections. By means of its keys it should be possible to run down fairly easily, at any rate, the genus of any of the lower organisms or invertebrates found in North America.

Here it should be remarked that it is a pity that the chapter on the vertebrates is not modelled more on the lines of other chapters. It is, of course, obvious that vertebrates, particularly fish, with their limited distribution, do not present the same problem as the invertebrates, but in spite of this we think greater uniformity might have been secured.

Several points in the book call for commendation. The plan adopted in the chapter on Cladocera of giving with each drawing a line and its measurement is one that might be extended to other chapters with advantage in future editions of the book. In the section on Rotatoria the drawings are on one page and the text facing them, and this too makes the figures more easy to read, and might be adopted with benefit in other chapters.

The difficulty in a compendium of this nature is to keep the book of a reasonable size and yet make it comprehensive, and in this the editors have succeeded admirably. More detail could have been given in some cases, but not without increasing the size of the book; indeed it is remarkable how much has been included in its pages.

The authors, one and all, are to be congratulated on producing a thoroughly useful, readable, and much-needed book.

C. H. O'D.

ANTHROPOLOGY

The Megalithic Culture of Indonesia. By W. J. PERRY, B.A. [Pp. xii + 198, with 4 maps, 4 plates, and 9 other illustrations.] (London: Longmans, Green & Co, 1918. Price 12*s.* 6*d.* net.)

THIS book is a contribution to the discussion of the problem known as culture-mixture. The conception of culture-mixture as the explanation of various puzzling sociological phenomena which are to be observed among many peoples is a theory which has been developed mainly by Dr. W. H. R. Rivers and Prof. G. Elliot Smith. "In the course of an examination of the cultures of Melanesian peoples, Dr. Rivers came to the conclusion that the only way to account for the existence of certain customs among the people of Melanesia was to adopt the hypothesis of

culture-mixture. He assumed that there had swept into that region at least one wave of migration of people possessing customs and beliefs foreign to those of the indigenous population, and that from the interaction of these two systems had resulted the cultures which he was examining." Prof. Elliot Smith approached the subject partly from the side of physical anthropology. He came to the conclusion that the custom of building megalithic monuments had been spread largely through the migrations of a people belonging to a definite Armenoid racial type; and he finally ventured to put forward the generalisation that "megalithic monuments, in whatever part of the world they may be found, showed such similarities of structure and associations that they must have been the work of people sharing a common culture." It is believed that Egypt was the original source of the megalithic culture,—though not the original home of the Armenoid race. "Indonesia occupies a position of peculiar importance in relation to the main argument as to the origin and nature of megalithic monuments, for it forms the sieve through which any extensive migration from the West to Oceania must pass. Any migration into the Pacific of sun-worshipping megalith-builders should have left traces of their passage in Indonesia." With the approval and under the advice of Dr. Rivers, the author set himself the task of finding these traces, and the evidence that he has collected is described very clearly in this volume. The book, though short, is a monument of industry, for the collection of the data must have been extremely laborious. The investigation is naturally concerned with the more primitive peoples of Indonesia, who remain free from the "obtrusive disturbing influences of the Indian, Chinese, and Arabian civilisations." For the purpose of the book the term "Indonesia" has been made to include not only the East Indian Archipelago, but also Assam, Burma, the Malay Peninsula, the Phillipine Islands, and Formosa, which are sociologically associated with Indonesia proper. Facts concerning megalithic monuments, stone graves, sacred stones, stone origin myths, "half-men," the priesthood, the "sky-world," and many other matters are set forth. Mr. Perry believes that the evidence establishes the theory that the megalithic monuments, and, indeed, all the stone-work of Indonesia, have originated from the cultural influence of alien immigrants, who, in many places, founded chiefly families which survive to this day. The original immigrants and also the existing chiefly caste are supposed to have an intimate relationship with the "sky-world." Although the author is in some places, perhaps, insufficiently critical of his own theory, the argument as a whole will probably carry conviction to most readers.

A. G. T.

Primitive Man. By G. ELLIOT SMITH, F.R.S. [Pp. 50.] (London: Oxford University Press. Price 3*s*. 6*d*. net.)

THIS pamphlet is a reprint of an address delivered before the British Academy in November 1916, and it appears also in the *Proceedings of the British Academy*, Vol. VII. Prof. Elliot Smith is one of our most stimulating writers on anthropology, and whilst the reader will find him a cautious and conservative guide on controversial problems, he is, at the same time, constantly suggesting new lines of thought. In this paper he covers a wide range of subjects, from the beginnings of the Hominidæ to the relatively very recent Egyptian and "Heliolithic" civilisations. There is no greater authority on the brain than Prof. Elliot Smith, and one is interested to see that he regards the brain-cast of *Pithecanthropus* as entitling that being to rank as a member of the Hominidæ. On the other hand, he classes *Sivapithecus indicus*, from the Indian Miocene, with the apes,

not among the human tribe. The discoverer of *Sivapithecus*, Dr. Guy Pilgrim, took the other view, but Prof. Boule thought the new genus ought to be placed in the Simiidæ, and this latter opinion has always seemed to me to be correct. The latter half of the paper is largely psychological. The author has no belief in the alleged detailed identity of the working of the human mind in different areas. There has, he says, "been no far-reaching and progressive modification of the instincts and emotions since man came into existence" And "at no stage of his career has he acquired highly complex and specialised instincts which impelled him, without any promptings from other peoples, to build megalithic monuments, or to invent the story of the deluge, independently of other people who do the same arbitrary things, as modern speculations would have us believe."

A. G. T.

ENGINEERING

Textbook of Ordnance and Gunnery. By Lieut.-Col. William H. Tschappat, Ord. Dept., U.S.A. [Pp. iv + 705, with 314 illustrations, one folding plate, and 14 tables.] (New York: John Wiley & Sons; London: Chapman & Hall, 1917. Price 30*s*. net.)

This is a further example of the thorough manner adopted in the United States in the preparation of textbooks for a clearly defined class of student.

It is, of course, also indicative of the thorough training of a truly educational character given to specific students—in this case "the senior class at the U.S. Military Academy, or for students who have had an equal amount of mathematical training."

The chapters taken in order deal with: Properties, Manufacture, Care and Use of Explosives, Theory of Explosives, Measurements of Velocities and Pressures, Interior Ballistics, Metals used in Ordnance Construction, Guns, Recoil and Recoil Brakes, Artillery, Exterior Ballistics, Projectiles, Primers and Fuses, Aiming Devices, Fire Control Instruments, Small Arms and Hand Arms, Machine Guns.

Chapter VI.—Guns—is characteristic of the work throughout the book. Under the simple heading of "Guns" we have an excellent treatise on elasticity which points the way to and reason for modern gun construction—the selection of material, the care in manufacture, and the mechanisms for firing.

In the same way, under "Exterior Ballistics," the theory of projectiles is followed by the necessary "corrections" required in practice under the ever-varying conditions following changes in projectiles and changes in wind and general atmospheric conditions. It further indicates how essential it is that the "personal equation," so to speak, of every gun should be known to those in charge.

The chapter which follows on "Projectiles" is equally well treated. In such a textbook it is hardly to be expected that the author, with so much definite matter to cover, would care to wander into the realms of possibilities, and consequently no specific mention is made of projectiles for very long range guns, say seventy-five miles. No similar book to the one under review will be complete in the future without some such very definite treatment, because, without question, the projectile is the most important factor in a long range bombardment—both with respect to obtaining (safely on the projectile itself) the necessary pressure on the base and the conditions of its flight in the air.

This volume should prove most interesting reading to any scientist at the moment.

J. Wemyss Anderson.

Mathematics for Engineers, Part I. By W. N. Rose, B.Sc. Eng. (Lond.). [Pp. xiv + 510.] (London: Chapman & Hall, 1918. Price 8*s.* 6*d.* net.)

A professor of mathematics was recently asked his opinion as to the best textbook on practical mathematics for engineers. His reply was, "any good textbook that is practically mathematics."

With so vast a science as mathematics, it is desirable—in fact necessary—to follow a definite sequence in the training of engineers. In this matter the author has arranged and developed an excellent scheme which is "practically mathematics."

At the same time we greatly doubt the wisdom of keeping exclusively to technical, or even scientific, data for the purpose of example. In this volume too much engineering detail is introduced.

For instance, unless the student has studied and understood his steam engine, and, further, has a very good idea of projection, the example on p. 133, "calculate the weight, in cast-iron, of the D-slide valve shown in fig. 67," will only embarrass—it certainly will not help. On p. 134, fig. 58, is a figure of a plate for a cast-iron tank; p. 135 has two figures—one, a stamping for a dynamo armature, the other, a link for a chain; p. 136 has an illustration of a curved cast-iron pipe; p. 137, a wrought-iron crank; pp. 138 to 143 contain too many details even to catalogue but one of them—a cast-iron plummer block—the weight of which is required. The article in question is 7 inches long and 2 inches wide.

The practical engineer would put it on a scale and weigh it. A bunker of irregular shape, only partially filled with coal, would be a much more practical problem.

The chapter on "the construction of practical charts" is distinctly useful and very thorough.

Plane trigonometry occupies 67 pages and should be taken by the author as a typical section when revising his book, as a very great deal of the 474 pages could be eliminated without loss—including many of the illustrations just named—particularly as a book of reference for engineers—one of the primary objects of the author.

J. Wemyss Anderson.

The Training and Work of the Chemical Engineer. A general Discussion held by the Faraday Society. [Pp. 60.] (Reprinted from the *Transactions of the Faraday Society*, Vol. XIII., September 1917. Price 3*s.* 6*d.*)

The chemical industry and the chemical and engineering departments of our colleges are under a great debt to the Faraday Society for inaugurating this discussion.

Where practically every word is weighty it is difficult to differentiate, but it is impossible to pass over the suggestions (1) of Sir Robert Hadfield that "it is evident, therefore, that no one course of training can cover all the requisite ground"; (2) of Sir George Beilby, particularly where he outlines a new phase of academic activity, "because I hold very strongly that in the later years of the four years' course, which ought to be regarded as the absolute minimum required for all serious chemists, the heads of the department in university and college training laboratories ought to be in a position to differentiate the various leading types of students into investigators, routine workers, and men of administrative ability. The heads of the engineering department might on their part pick out from the

chemical students passing through their hands those who show mechanical aptitudes in any particular way."

The discussion shows that the chemical industry, like many other industries, is built up in the main in three different sections—scientific, commercial, and industrial. It should be abundantly clear "that no one course of training" can possibly serve all three sections. It must be made equally clear that chemists, if they wish to be classed as engineers, must conform to the practical engineering training required by the engineering institutions of this country—no wholly academic training will be either adequate or acceptable.

Many of our leading engineers have had a good chemical training; they would not, however, be classified as "chemical engineers." In the same way many of our leading industrial chemists have an excellent knowledge of engineering requirements, but they would not classify themselves as "engineer chemists."

If the universities and university colleges fixed a definite standard of chemical knowledge as necessary before allowing a student to enter their school of chemistry, a much-needed step in the right direction would have been made. This country's experience during the last few years indicates that we require chemists with a better and higher knowledge of chemistry. It would be much better—on Sir George Beilby's lines—to allow the chemist to keep to his chemistry.

Prof. Donnan's outline scheme is hopeless—the production would be a bad chemist and an impossible engineer.

On the other hand Sir George Beilby's suggestions should be taken most seriously into consideration. In effect he more than hints that the debt left by the war will mean efficiency in the very highest degree—that chemists must be better chemists, that engineers must be better engineers, that some chemists, after a three years' training, *if properly shepherded by their instructors*, may be guided into important work, bridging the gulf between chemistry and engineering. The manager of a chemical works may or may not be a chemist—may or may not be an engineer—he must be a man of "administrative ability."

Sir George points to a "very real danger"—viz. "the development of unbalanced proposals for the training of the future leaders and workers in industry." We quite agree.

J. Wemyss Anderson.

Aeronautics in Theory and Experiment. By W. L. Cowley, A.R.C.Sc., D.I.C. and H. Levy, M.A., B.Sc., F.R.S.E. [Pp. xii + 284, with 130 figures and four folding plates.] (London: Edward Arnold, 1918. Price 16*s.* net.)

Of the great developments—scientific and practical—that have followed the outbreak of war, a leading position would, without question, be given to aircraft of all kinds. The history and details of this particular development must be given at the right time, but it is a matter for congratulation that what can safely be written of the science of aeronautics to-day, has found such able exponents as the authors of the volume under consideration—as the treatment of the subject is both scientific and practical.

The introduction touches on the subdivision of aircraft and the functions of the various parts of an aeroplane together with an outline of aeronautical research. The remainder of the book is divided into five parts—the first part dealing with fluid motion and the problem of applying the results obtained on models to the full-sized machine. The second part deals with the aerofoil and also the

structural parts and controls. Under Part III. the strength of construction is considered; Part IV. has 30 pages devoted to the airscrew—an extraordinary example of concise thoroughness. Part V. has four chapters—(1) Steady Motion in Flight, (2) Stability—Mathematical Theory, (3) Stability—Experimental, (4) The Complete Machine. A useful glossary of aeronautical terms and the folding plates complete the volume.

The authors anticipate that the science of aeronautics "will shortly take its proper position in the curriculum of every university and technical college."

The foundation of the Zaharoff Professorship of Aviation gives practical colouring to this anticipation, and this volume, in addition to being of general value to the man of science, should be of the greatest value to the student of aviation.

J. WEMYSS ANDERSON.

Tidal Lands: A Study of Shore Problems. By ALFRED E. CAREY, M.Inst.C.E., and F. W. OLIVER, F.R.S. [Pp. xiv + 284, with 56 figures and 29 plates.] (London: Blackie & Son, Ltd., 1918. Price 12*s*. 6*d* net.)

FEW, probably, of the vast numbers who spend their leisure by the sea pause to dwell upon the romance that is embodied in the narrow intertidal zone we term the foreshore. For the coastal landowner, however, as for "those who go down to the sea in ships," the word connotes the battle-ground of elemental forces, a scene of never-ceasing change. The volume under review deals with the fascinating problems associated with this region, and is essentially a practical contribution to the subject of coastal maintenance and reclamation.

Hitherto the control of erosion and accretion has been regarded as peculiarly the province of the engineer and, in consequence, protection devices have become almost synonymous with wooden piles or stone and concrete groynes. There is an appeal of the massive to the human mind which has found its expression in the primitive forms of construction both in engineering and in architecture. It is only with the acquisition of knowledge that the ponderous gives place to the flexible, which, because it yields, the oftener remains unbroken. Undoubtedly the most notable feature of the present volume is the development of this evolutionary tendency, in the emphasis laid upon the rôle of plants in protection and their paramount utility in the artificial prevention or direction of the natural forces of erosion and accretion. Full recognition is given to the value of engineering constructions which constitute a useful and necessary adjunct to the utilisation of plants, but it is on these latter, with their remarkable capacity for meeting the many vicissitudes of the environment, that reliance is chiefly placed to curb and govern the forces of wind and water. In the words of the authors, "one of the intentions of the present book is to consider if, and to what extent, the constructions of the engineer may be supplemented, modified, or even replaced, by the employment of vegetation suitable to the purposes in view." The collaboration of engineer and botanist has ensured the maintenance of a true perspective between the two standpoints, and, though the greater part of the subject-matter is devoted to the living implements, constructional works are adequately treated in relation to the varied problems.

The value of vegetation for the fixation and maintenance of dunes has long been recognised, though much neglected in this country. The principle is here, however, extended to the control of shingle beaches, salt marshes, in fact, to all types of tidal lands. The work is one which should be carefully studied by all who are concerned with the conservation of our coastal areas, but even for those

whose interest is not so direct, there is much that will fully repay the expenditure of time in its perusal.

Of the fourteen chapters the first two deal particularly with the physical forces and effects of tide and current. Chapter III. is concerned with the foreshore in its various aspects, not least of which is its legal status. The function of vegetation next receives attention, and this is followed by a treatment of dunes, shingle beaches, and salt marshes, and their fixation by plants. Two chapters are occupied with the description of constructional works in relation to reclamation, erosion, and accretion. The final sections treat of Blakeney Point from the engineering standpoint, State and local control, and complementary problems. Appendices follow which furnish data regarding the characteristic species of the various habitats, and a list of authorities having powers and duties in relation to defence against the sea.

Apart from the major theme the authors touch on several points of practical interest of which we may especially cite the economic potentialities of these waste areas, as exemplified by the value of *Psamma* in paper making and the use of dunes and even shingle for afforestation. In the latter connection the suggestion of *Pinus insignis* for the one and *Alnus incana* for the other is noteworthy.

We cannot conclude this review without reference to the numerous and well-chosen illustrations, many of which are from photographs portraying coastal phenomena in a wide range of localities.

E. J. SALISBURY.

MEDICINE

Tumours: their Nature and Causation. By W. D'ESTE EMERY, M.D., B.Sc. [Pp. xx + 146.] (London: H. K. Lewis & Co. Price 5*s*. net.)

IN this book, which is an essay in support of the parasitic theory of the origin of tumours, Dr. D'este Emery argues with much skill that neoplasms may best be explained as cell reactions in response to the invasion of a specific organism. The author has no new observations to bring forward, but, assuming the presence of a parasite possessed of very definite characters, he attempts to show by analogy with other undoubtedly parasitic diseases that cancer may reasonably be placed in the same category.

For the cancer parasite Dr. Emery postulates the following properties: (1) It must be of so small a size as to escape observation by any means at our disposal at present. (2) For some part, at least, of its life-history it must inhabit the cell, or, perhaps, its nucleus. (3) It must produce a toxin which has the power to induce cell division. It is also assumed that the parasite has a long latent period, and that it infects the cell in very large numbers.

Even if the possibility of the existence of such an organism be granted—and there is, of course, no actual proof of this—its entrance into the body is still to be explained. If the author's conclusions are correct it should surely be possible to effect an artificial infection of healthy tissues by the inoculation of whole or disintegrated cells containing large numbers of parasites. But this has never been done, and it is not even possible to be certain that tumours growing in the body are ever able to infect neighbouring healthy cells. Dr. Emery deals with both these objections, but not, we think, convincingly. He certainly acknowledges the possibility of such an infection, and quotes, as an example of transference by contact, an isolated observation by Borrel, which, however, is itself open to doubt; but he does not explain why, if it can occur at all, it does not do so more frequently.

In several details we find ourselves opposed to Dr. Emery, both in respect to facts, and also to the interpretations he places on them. Nevertheless, we have read his book with great interest. An extremely difficult subject has been treated in a broad and philosophic manner, and we know of no fairer or more stimulating presentation of this aspect of the cancer problem.

E. H. KETTLE.

MISCELLANEOUS

Jacob and the Mandrakes. By J. G. FRAZER. [Pp. 23.] (London: Oxford University Press. Price 2*s*. 6*d*. net.)

THIS small pamphlet is a reprint of a contribution to the *Proceedings of the British Academy*, Vol. VIII., and it was originally read before the Academy in January 1917. It deals with the folklore which surrounds and explains the incident described in Genesis xxx., wherein Leah gives Rachel the mandrakes which Reuben had gathered. The story told in Genesis appears to be an expurgated and modified version of the original Hebrew tradition, for here as elsewhere the monotheistic compiler of the book would seem to have edited his stories freely. The old tradition was that Rachel ate the mandrakes, and that the birth of Joseph was the direct result of the fact that his mother had thus partaken of this magic fruit. Sir J. G. Frazer deals fully with the superstitions connected with the mandrake, of which the chief is, as the Hebrew tradition implies, that the plant has the property of bestowing fertility upon barren women. The mandrake or mandragora has been regarded with awe in many other countries besides Palestine. The superstitions are spread all over the Near East, in Greece and Italy, and even prevailed at one time in Germany and England. In northern countries, however, the "mandrakes" were mostly forgeries, for the plant does not grow north of the Alps. The root would act as a love-charm, would bring its owner wealth, and had many other wonderful properties. It was, however, dangerous to dig up; and it was deemed advisable to utilise an animal to haul it out of the ground. The poor beast used for this purpose was generally killed by the horrible scream which the plant uttered as it was torn from the ground.

A. G. T.

First and Last Things. A Confession of Faith and a Rule of Life. By H. G. WELLS. Revised and Enlarged Edition. [Pp. xviii + 233.] (London and New York: Cassell & Co., Ltd., 1917. Price 6*s*. net.)

THIS is a revised and enlarged edition of a volume first published in 1908. It deals with a variety of philosophical subjects, ranging from abstract Metaphysics to very concrete Ethics. It is not so much a set treatise on any of the subjects which it touches, but is rather an outpouring of Mr. Wells' own sentiments on these subjects—a case of Mr. Wells thinking in public, so to speak. He admits, in the Introduction, that he is an amateur in all these spheres; indeed, it is plain that he has not read widely; nor, perhaps (a more serious matter), is his thinking quite so profound as he is inclined to imagine. It cannot be said, therefore, that the book makes any advance towards the ideal of philosophy—the discovery of truth. It has, however, merits of quite a different kind; for it expresses in remarkably clear language a philosophic outlook very characteristic of a dominant section of the public in our times. Mr. Wells is a demagogue in philosophy; he has all the attributes of a demagogue, the power of vividly expressing what others are thinking in an even vaguer and more formless manner; he can justly claim to

be representative of our age. We may think that his philosophy is half-fledged and immature; unless it were, it would not be representative. If Mr. Wells' book is read in the future, it certainly will not be for any positive contribution to knowledge, or with any reference to the conclusions arrived at, but as a concrete and lively example of a dominant mode of thought in the early twentieth century.

It is, therefore, hardly worth while to criticise the substance of this book. As regards the form, Mr Wells is somewhat inclined to be the slave of words and phrases. This tendency to obsession by language is by no means decreasing in his later works.

HUGH ELLIOT.

Some Questions of Phonetic Theory. Part I. By WILFRID PERRETT, B.A., Ph.D. [Pp. viii + 110.] (London: The University of London Press, 1916. Price 2*s.* 6*d.* net.)

THIS little book forms the first instalment of a work planned to appear in three parts. This part is divided into four chapters: The Position of Rest, Willis on Vowel Sounds, the Wheatstone Test, and the Compass of the Mouth.

The first chapter deals with the resting positions of the vocal organs, much fun being made of the views of other writers on this topic. In the second chapter the vowel sounds are touched upon. Here the criticism breaks out again. Thus, referring to what is called the standard work by Helmholtz *On the Sensations of Tone*, the author writes:

"I consider it to be a very poor standard and a most wasteful and misleading work. I shall endeavour to show that wherever it bears upon phonetics Helmholtz's book has no right to be considered authoritative, and that his influence has been and is constantly bad. The reader must be warned: our path lies across a veritable quagmire of sham science or *Wissenschaft*. It will be heavy going, and we must pick our way. Here and there we shall flounder, but we shall reach firm ground at last."

This is only one of half a dozen similar tirades against one who at any rate thought and investigated, and so justly won for himself an eminent position in the world of science of his day. Some of his views may be open to question and modification after further research. But it may well be doubted whether they can be successfully attacked by the flippant use of strong language unsupported by anything worth calling a scientific basis.

It is unnecessary to proceed in detail. The titles of the chapters give a sufficient idea of the scope of the book. The treatment is throughout of a discursive, descriptive, and utterly unconvincing nature. Much of it is written in a light or sarcastic vein, and though it may possibly prove amusing to some readers, can scarcely be called *science*, or be mistaken for an aid to its *progress*.

E. H. B.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Methods of Measuring Temperature. By Ezer Griffiths, D.Sc. (Formerly Fellow in the University of Wales), Assistant in the Heat Department of the National Physical Laboratory. With an Introduction by Principal E. H. Griffiths, F.R.S. London: Charles Griffin & Co., Exeter Street, Strand, W.C.2. (Pp. xi + 176, with 81 illustrations.) Price 8*s.* 6*d.* net.

Elements of the Electromagnetic Theory of Light. By Ludwik Silberstein, Ph.D., Lecturer in Natural Philosophy at the University of Rome. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. vii + 48.) Price 3*s.* 6*d.* net.

Plant Products and Chemical Fertilisers. By S. Hoare Collins, M.Sc., F.I.C., Lecturer and Adviser in Agricultural Chemistry, Armstrong College, Newcastle-on-Tyne (University of Durham); Formerly Assistant Agricultural Chemist to the Government of India. London: Bailliere, Tindall & Cox, 8, Henrietta Street, Covent Garden, 1918. (Pp. xvi + 236.) Price 7*s.* 6*d.* net.

The Future of Pure and Applied Chemistry. Presidential Address delivered at the Annual General Meeting of the Chemical Society on March 21, 1918. By Prof. William Jackson Pope, C.B E., D.Sc., F.R.S. From the *Transactions of the Chemical Society*, 1918, Vol. CXIII. (Pp. 2.)

Coal and its Scientific Uses. By William Bone, D.Sc., Ph.D., F.R.S., Professor of Chemical Technology in the Imperial College of Science and Technology, London; Formerly Livesey Professor of Coal Gas and Fuel Industries in the University of Leeds; Honorary Member of the American Gas Institute and of the Institution of Gas Engineers; Chairman of the British Fuel Economy Committee, 1915-17. London: Longmans, Green & Co, 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. xv + 491, with illustrations)

Colour in Relation to Chemical Constitution. By E. R. Watson, M.A. (Cantab.), D.Sc. (London), Indian Educational Service; Professor of Chemistry, Dacca College, Bengal; Temporary Research Chemist with British Dyes, Ltd Monographs on Industrial Chemistry. Edited by Sir Edward Thorpe, C.B., LL.D., F.R.S. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. xii + 196, with 4 coloured plates and 65 figures of Absorption Curves, Spectra, etc.) Price 12*s.* 6*d.* net.

Plant Physiology. By Vladimir I. Palladin, Professor in the University of Petrograd. Edited by Burton Edward Livingston, Ph.D., Professor of Plant Physiology and Director of the Laboratory of Plant Physiology of the Johns Hopkins University. Authorised English Edition, based on the German Translation of the Sixth Russian Edition and on the Seventh Russian Edition, 1914. Philadelphia: P. Blakiston & Co., 10-12, Walnut Street. (Pp. xxv + 320, with 173 illustrations.) Price $3 net.

Fundamentals of Botany. By C. Stuart Gager, Director of the Brooklyn Botanic Garden. Philadelphia: P. Blakiston's Son & Co., 10-12, Walnut Street, 1916. (Pp. xix + 640, with 437 illustrations.) Price $1.50 net.

Studies in Electro-Physiology, Animal and Vegetable. By Arthur E. Baines, Consulting Electrician, with 31 original drawings in colour illustrating the Electrical Structure of Fruits and Vegetables. By Gladys T. Baines. London: George Routledge & Sons, 68-74, Carter Lane, E.C., 1918. (Pp. xxix + 291.) Price 12*s.* 6*d.* net.

Principles of General Physiology. By William Maddock Bayliss, M.A., D.Sc., F.R.S., Professor of General Physiology in University College, London. Second Edition, Revised. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. xxiv + 858.) Price 24*s.* net.

The Modern Treatment of Mental and Nervous Disorders. A Lecture delivered at the University of Manchester, on 25th March, 1918. By Bernard Hart, M.D., Fellow of University College, London; Physician for Mental Diseases, University College Hospital; Lecturer in Psychiatry, University College Hospital Medical School. Manchester: at the University Press, 12, Lime Grove, Oxford Road. London: Longmans, Green & Co., 1918. (Pp. iv+28.) Price 1*s.* net.

The Statics of the Female Pelvic Viscera. Vol. I. In which the Evidence of Pathology, Phylogeny, and Ontogeny, and Clinical Investigation, etc., is surveyed. By R. H. Paramore, M.D. Lond., F.R.C.S. Eng.; Major R.A.M.C. (Temp.), Pathologist and Registrar, Hospital for Women, Soho Square. London: H. K. Lewis and Co., 136, Gower Street, W.C., 1918. (Pp. xviii + 383, with 26 illustrations.) Price 18*s.* net.

Annual Magazine Subject-Index, 1917. A Subject-Index to a Selected List of American and English Periodicals and Society Publications. Edited by Frederick Winthrop Faxon, A.B. (Harv.). Compiled with the co-operation of Librarians. Boston: The Boston Book Company, 1918. (Pp. 267.)

Welfare and Housing. A Practical Record of War-time Management. By J. E. Hutton, Manager of the Labour and Catering Department of Vickers, Ltd.; Member of the Food Investigation Committee of the Ministry of Munitions. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. viii + 192, with 12 illustrations from photographs and two plans.) Price 5*s.* net.

Life and Letters of Sir Joseph Dalton Hooker, O.M., G.C.S.I. Based on Materials collected and arranged by Lady Hooker. By Leonard Huxley. London: John Murray, Albemarle Street, W.1. 1918. (Pp., Vol. I. x + 546, Vol. II. vi. + 569, with portraits and illustrations.) Price 36*s.* net.

War. By Ronald Campbell Macfie. London: John Murray, Albemarle Street, W. (Pp. viii + 72.) Price 3*s.* 6*d.* net.

Natural Science and the Classical System in Education. Essays New and Old. Edited for the Committee on the Neglect of Science. By Sir Ray Lankester, K.C.B., F.R.S. London: William Heinemann, 1918. (Pp. ix + 268.)

The Twin Ideals. An Educated Commonwealth. By James W. Barrett, K.B.E., C.B., C.M.G., M.D., M.S., F.R.C.S., Temporary Lieut.-Col. R.A.M.C. London: H. K. Lewis & Co., 136, Gower Street, W.C.1, 1918. (Pp., Vol. I. xxxii + 512, Vol. II. xx + 504, with illustrations, maps, and diagrams.)

National Reconstruction. A Study in Practical Politics and Statesmanship. By J. J. Robinson. London: Hurst & Blackett, Paternoster House, E.C., 1918. (Pp. x + 155.) Price 2*s.* 6*d.* net.

The Processes of History. By Frederick J. Teggart, Associate Professor of History in the University of California. New Haven: Yale University Press; London: Oxford University Press, 1918. (Pp. ix + 162.) Price $1.25.

The Royal Navy, 1815–1915. The Rede Lecture, 1918. By Admiral The Marquess of Milford Haven, P.C., G.C.B., LL.D., etc., Lond. Cambridge: at the University Press, 1918. (Pp. 48.) Price 2*s.* 6*d.* net.

The Athenæum Subject Index to Periodicals, 1916. Science and Technology, including Hygiene and Sport. Issued at the Request of the Council of the Library Association. London: The Athenæum, Bream's Buildings, Chancery Lane, E.C.4. New York: B. F. Stevens-Brown. (Pp. 162.) Price 10*s.* net.

This volume contains 162 pages, and one can form some idea of its accuracy by searching it for papers on one's own subject. Of course the catalogue cannot be as complete as those contained in special bibliographies, but the references are astonishingly numerous, even regarding very detailed subjects.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By Philip E. B. Jourdain, M.A., Cambridge.

Education.—In the number of the *Mathematical Gazette* for March 1918 there are several papers read at the annual meeting of the Mathematical Association in January 1918 which are of great interest in educational questions. T. P. Nunn gives a presidential address on "Mathematics and Individuality," W. P. Milne discusses the graphical treatment of power-series and the uses and functions of a school mathematical library, S. Brodetsky discusses nomography, and G. Goodwill gives some suggestions for a "presentment of mathematics in closer touch with reality."

For use in preparing a subject for discussion in an undergraduate mathematical club is given, in the *Amer. Math. Monthly* (1918, **25**, 276–82), a very full set of references to literature on some integrals and spirals considered by Euler, Fresnel, and others (cf. Science Progress, 1918, **13**, 177).

History.—H. G. Zeuthen published (1917) in Danish a work on the history of mathematics from Plato to Euclid, of which an account is given by D. E. Smith (*Bull. Amer. Math. Soc.* 1918, **24**, 407–10). The same subject also comes into Zeuthen's able and interesting article (*Scientia*, 1918, **24**, 257–69) on the definitions of Euclid.

G. R. Kaye (*ibid.* 53–5) supports Carra da Vaux's theory, given in *Scientia* of 1916, of the Greek origin of our numeration, and of its passage through Persia to India and Arabia, by his own results found in a wholly different way and published in 1907, 1908, 1911, and in his *Indian Mathematics* of 1915 (cf. Science Progress, 1916, **10**, 503–4).

D. E. Smith and J. Ginsburg (*Amer. Math. Monthly*, 1918, **25**, 99–108) give an extensively annotated translation of Rabbi ben Ezra's (Abraham ibn Ezra's) introduction to his translation into Hebrew of a book on the astronomical tables of Mohammed ibn Mûsâ al Khowarizmi, on account of the light it throws on

the introduction of Hindu mathematics into the region of Mesopotamia.

C. J. Keyser (*Bull. Amer. Math. Soc.* 1918, **24**, 321–7) maintains that Lucretius had a correct—judged by modern standards—concept of infinity.

H. S. Carslaw (*Phil. Mag.* 1916, **32**, 476–86) writes on the development of Napier's theory of logarithms (cf. SCIENCE PROGRESS, 1918, **12**, 362).

Hk. de Vries (*Nieuw Tijdskr.* 1916–17, **4**, 145–67) discusses the *Géométrie* of Descartes and the *Isagoge* of Fermat.

G. Loria (*Scientia*, 1918, **24**, 311–12), in a review of J. M. Child's book on *The Geometrical Lectures of Isaac Barron* (Chicago and London, 1916), remarks that a complete edition of the manuscripts of Leibniz formed part of the programme of the international Association of Academies, which worked until August 1914, and that a new edition of the works of Newton is urgently required,—since Horsley's edition is rare and also since much indispensable and unpublished material exists at Cambridge.

The first part of the tenth volume of Gauss's *Werke*, containing his diary from 1796 to 1814, was published at Leipzig in 1917 (*Rev. Sem.* 1918, **26** [1], 49).

G. Loria (*Atti della Soc. Ligustica di Sci. Nat. e Geogr.* 1918, **28**, N. 3) gives a very detailed study of Guglielmo Libri as a historian of science.

A good account and discussion of the proofs of Pierre Laurent Wantzel (1814–48) of the impossibility of solving the general quintic by radicals, of avoiding the "irreducible case" in cubics, and of duplicating the cube or trisecting the angle by ruler and compass, are given by F. Cajori (*Bull. Amer. Math. Soc.* 1918, **24**, 339–47).

An account of the fourth and last volume of Hermite's *Œuvres*, published at Paris in 1917, is given by J. Pierpont (*ibid.* 481–4).

Notices of the life and work of Dedekind are given by E. Landau (*Gött. Nachr.* 1917, 50–70), and of Darboux by D. Hilbert (*ibid.* 71–5) and E. Picard (*Ann. de l'éc. norm.* **34**, 81–93).

Principles and Theory of Aggregates.—C. J. Keyser (*Bull. Amer. Math. Soc.* 1918, **24**, 391–4) concludes that any postulate-system admits of any given infinite number of interpretations.

T. Brodén (*Nyt Tidsskr. for Mat.* 1917, **28**, 21–32) reproduces

his proof of 1915 of the illegitimacy of "classes which contain themselves as members," and replies to certain criticisms.

A. Rosenthal (*Gött. Nachr.* 1916, 305–21) makes some contributions to Carathéodory's theory of measurability, and M. Leau (*Compt. rend.* 1917, **165**, 141–4) writes on the measure of linear aggregates. W. Sierpinski (*ibid.* **164**, 882–4) discusses some problems which imply non-measurable functions, gives (*ibid.* 993–4) an extension of the notion of density of an aggregate, and (*Ann. di Mat.* 1917, **26**, 131–50) finds the necessary and sufficient conditions that an aggregate of points should be an "arc."

E. Guillaume (*Rev. de Métaphys.* 1918, **25**, 285–323) considers the theory of Lorentz as a given logical construction, and tries to introduce into it a parameter which plays the part of universal time (cf. his papers in *Arch. de Genève*, 1917, **43**, 89–112, 185–98). L. de la Rive (*ibid.* 281–4) gives a geometrical construction of the equations of relativity. R. Bricard (*Nouv. Ann.* 1917, **17**, 201–22) gives an elementary sketch of the principle of relativity in space of one dimension. L. Amaduzzi (*Scientia*, 1918, **24**, 239–43, 321–6) gives a short, simple, and excellent account of the principle of relativity.

Theory of Numbers and Algebra.—G. H. Hardy and S. Ramanujan (*Proc. Lond. Math. Soc.* 1918, **16**, xxii; **17**, 75–115) develop their application (cf. SCIENCE PROGRESS, 1918, **13**, 5) to the principal problems of the theory of partitions of the analytic methods which have proved fruitful in the theory of the distribution of primes and allied subjects.

G. Julia (*Compt. rend.* 1917, **164**, 352–5, 484–6, 571–4, 619–22, 910–13, 991–3) continues his researches on arithmetical binary forms (cf. SCIENCE PROGRESS, 1918, **13**, 180).

H. H. Mitchell (*Trans. Amer. Math. Soc.* 1918, **19**, 119–26) gives a proof that certain ideals in a cyclotomic realm are principal ideals.

M. Amsler (*Compt. rend.* 1917, **165**, 102–5) considers the development of a quadratic irrational in a continued fraction.

O. Szász (*Journ. für Math.* **147**, 132–60) solves a general problem relating to the convergence of continued fractions.

G. Humbert (*Journ. de Math.* 1916, (7) **2**, 79–103, 104–54, 155–67) solves some problems on Hermite's (1854) method of approximation to a given irrational number. L. R. Ford (*Trans. Amer. Math. Soc.* 1918, **19**, 1–42) also studies Hermite's

method, gives the (rational, complex) fractions a geometrical interpretation, and then studies approximations by means of continued fractions. These researches bear some analogy to those of Humbert (1915, 1916) for the case of real numbers.

R. W. Brink (*ibid.* 186–204) gives a new integral test of the second kind for the convergence and divergence of infinite series.

H. B. Mitchell (*ibid.* 43–52) obtains some conclusions as to the position of the imaginary roots of a polynomial from the real roots of its derivative.

M. Bauer (*Jahresber. der D.M.V.* 1916, **25**, 294–301) writes on the determination by iteration of the real roots of an algebraic equation.

G. Pólya (*Vierteljahrsschr. Zürich*, 1916, **61**, 546–8) proves a theorem on algebraic equations with real roots alone.

G. Darbi (*Ann. di Mat.* 1917, **26**, 191–7) finds a property of Abelian equations with cyclic groups.

W. L. Hart (*Bull. Amer. Math. Soc.* 1918, **24**, 334–5) proves, after a method due to F. Riess, a theorem on infinite systems of linear equations.

R. D. Carmichael (*ibid.* 286–96) derives numerous elementary inequalities for the roots of an algebraic equation. The results generalise many known inequalities and contain new ones of interest.

Louise D. Cummings (*ibid.* 336–9) draws attention to a neglected paper (1847) of Kirkman, which is of importance in the theory of triad systems.

A. A. Bennett (*ibid.* 477–9) derives, by means of quite elementary considerations, the equation of the probability-curve from the sequence of binomial coefficients.

C. H. Forsyth (*ibid.* 431–7) discusses his (1916) interpolation formula for giving what he calls "tangential interpolation of ordinates among areas."

E. T. Bell (*ibid.* 376–80) proves some properties of certain remarkable determinants of integers, which follow immediately from simple considerations in the theory of numbers.

W. H. Metzler (*Amer. Math. Monthly*, 1918, **25**, 113–15) gives another proof of Pascal's theorem (1915) on certain determinants which are expressible as the sum of two squares.

Sir T. Muir (*Proc. Roy. Soc. Edinburgh*, 1918, **38**, 146–53) has a note on Cayley's (1846) construction of an orthogonant.

L. H. Rice (*Amer. Journ. Math.* 1918, **40**, 242–62) gives an extended definition of a determinant which applies to determinants of more than three dimensions, and enables us to remove the restriction of Cayley's law of multiplication and to set up a new case in Scott's law of multiplication ; and gives an application to transvectants.

O. E. Glenn (*Trans. Amer. Math. Soc.* 1918, **19**, 109–18) shows that the system of covariants of a binary cubic, when transformed by a group of all linear substitutions on x_1, x_2 whose coefficients are least positive residues with the modulus 2, is finite and that the fundamental set consists of twenty quantics.

A. Ostrowski (*Math. Ann.* 1917, **78**, 94–119) finds the necessary and sufficient conditions for the existence of a finite basis with certain systems of whole functions.

C. H. Rawlins (*Amer. Journ. Math.* 1918, **40**, 155–73) derives complete systems of concomitants of the three-point and the four-point in elementary geometry, with some applications.

W. A. Manning (*Trans. Amer. Math. Soc.* 1918, **19**, 127–42) gives the third part of his researches on the order of primitive groups : the former parts were published in 1909 and 1915.

P. Fatou (*Compt. rend.* 1917, **164**, 806–8) has a note on rational substitutions.

H. B. A. Bockwinkel (*Proc. K. Akad. Amst.* **19**, 1100–14) gives an English translation of his paper on transmutations in the *Verslagen* (cf. SCIENCE PROGRESS, 1918, **13**, 10).

Analysis.—K. P. Williams (*Amer. Math. Monthly*, 1918, **25**, 246–9) and E. W. Chittenden (*ibid.* 249–50) find various properties of functions which approach a limit at every point of an interval.

H. Blumberg (*Bull. Amer. Math. Soc.* 1918, **24**, 381–3) proves a general theorem on semi-continuous functions, which is analogous to those of G. C. Young (1914) and A. Denjoy (1915) on sets of points where the four derivatives of a given continuous function are identical.

A. Loewy (*Math. Ann.* 1917, **78**, 1–51) writes on matrices and differential complexes.

A. Denjoy (*Ann. de l'éc. norm.* [3], **33**, 127–222) gives the first part of a continuation of some work of his published in 1915. Here he solves the problem of determining the primitive of a function which is known to be a derivative.

W. Sierpinski (*Giorn. di Mat.* 1916, **54**, 314–34) gives an elementary example of an increasing function which has a zero derivative almost everywhere.

H. B. Phillips (*Amer. Journ. Math.* 1918, **40**, 235–41) shows how the algebraic sign can be directly attached to the element of integration of multiple integrals, so that such integrals can be treated in this respect like curvilinear integrals.

N. Lusin (*Ann. di Mat.* 1917, **26**, 77–129) extends some theorems on integration due to H. Lebesgue and A. Denjoy.

R. D. Carmichael (*Bull. Amer. Math. Soc.* 1918, **24**, 348–55) gives a thorough and useful review of de la Vallée Poussin's recent book on Lebesgue integrals (cf. SCIENCE PROGRESS, 1917, **12**, 9).

J. F. Ritt (*Bull. Amer. Math. Soc.* 1918, **24**, 225–7) fills a lacuna in what is known about the differentiability of asymptotic series; the responsibility for this lacuna being a failure to distinguish between the real and complex domains in this connection.

A. Kienast (*Proc. Camb. Phil. Soc.* 1918, **19**, 129–47) gives extensions of Abel's theorem on a limiting value of power-series, and its converse.

E. Cotton (*Compt. rend.* 1917, **164**, 389–92) obtains a relation between the radius of convergence of a power-series and the "characteristic number" (Liapounoff) of a certain real or complex function of n which determines the coefficient a_n.

E. Landau (*Archiv der Math.* 1916, **25**, 173–8) gives a new proof of a theorem of G. H. Hardy on the mean value of a certain integral in the theory of analytic functions, and M. Pétrovitch (*Compt. rend.* 1917, **164**, 716–18, 780–2) gives some arithmetical theorems on Cauchy's integral.

I. Priwaloff (*Bull. de la Soc. Math. de France*, 1916, **44**, 100–3) proves some theorems on conjugate functions, and G. Valiron (*ibid.* 103–19) obtains some results on the theory of interpolation for whole functions, which seem, from the account given in *Rev. sem.* (1918, **26** [1], 34–5), to be closely connected with the complete solution of the problem published by Jourdain in 1905 (*Journ. für Math.* **128**, 169–210).

H. Bohr (*Gött. Nachr.* 1916, 276–91) obtains some results on the sum of the coefficients of a power-series, and G. D. Birkhoff (*Compt. rend.* 1917, **164**, 942–5) has a paper on a generalisation of Taylor's series.

On conformal representation we may refer to P. Koebe (*Journ. für Math.* **147,** 67–104), G. Faber (*Sb. München,* 1916, 39–42), and P. Montel (*Compt. rend.* 1917, **164,** 879–81).

F. Schottky (*Journ. für Math.* **147,** 161–73) gives a conspectus of the elementary considerations which occur in proofs of Picard's famous theorem.

On the general theory of functions we may also mention the papers of R. König (*Math. Ann.* 1917, **78,** 63–93) on a theory of Riemann's pairs of functions, P. Montel (*Ann. de l'éc. norm.* [3], **33,** 223–302) on certain "normal" families of analytic functions, and G. Pólya (*Vjsschr. Zürich,* 1916, **61,** 531–45) on the rapidity of convergence of a power-series which represents a whole transcendental function satisfying an algebraic differential equation.

R. D. Carmichael (*Amer. Journ. Math.* 1918, **40,** 113–27) continues his work of 1916 and 1917 (SCIENCE PROGRESS, 1918, **13,** 8–9) on functions defined by certain series, and makes a contribution towards solving the problem of representing given functions in the form of series $\Sigma c_n g(x+n)/g(x)$. H. Gronwall (*Ann. de l'éc. norm.* [3], **33,** 301–93) finds the zeros of the functions $P(z)$ and $Q(z)$ associated with the Gamma function. On Hyperfuchsian and Hyperabelian groups and functions and certain total differentials, see E. Picard (*ibid.* 363–72, 373–9) and G. Giraud (*ibid.* 303–29, 330–62; *Compt. rend.* 1917, **164,** 386–9, 487–9). On conditions for developability in Dirichlet's series, see J. F. Steffensen (*Nyt Tidsskr. for Mat.* 1917, **28,** 9–11). On two points in the theory of trigonometric series, see I. Priwaloff (*Compt. rend.* 1917, **165,** 96–9) and H. Hahn (*Jahresber. der D.M.V.* 1916, 25, 359–66).

G. N. Watson (*Proc. Roy. Soc.* 1918, **94A,** 190–206; *Nature,* 1917, **100,** 299) gives some general theorems concerning the zeros of Bessel functions: the theorems are true for functions of any order, and, unlike results previously known, are of particular interest in the case of functions of high order. Watson (*Proc. Lond. Math. Soc.* 1918, **17,** 116–48) publishes a second part to his paper of 1910 on the harmonic functions associated with the parabolic cylinder. In this paper, which is much more closely connected with a paper of 1917 (cf. SCIENCE PROGRESS, 1918, **12,** 368), he investigates various types of asymptotic formulæ and expansions which are to be associated with those previously obtained for Bessel functions.

At the basis of Fréchet's "functional calculus" (1906) is a generalisation of the distance between two points, which associates with each pair of elements of an abstract class a real non-negative number. He was thus able to retain the more important theorems of the theory of sets of points and of real functions. A. D. Pitcher and E. W. Chittenden (*Trans. Amer. Math. Soc.* 1918, **19**, 66–78) simplify the foundations of Fréchet's theory in some interesting respects.

M. Fréchet (*ibid.* 53–65), starting from the fact that we can, in many cases, define convergent sequences and their limits in a class of abstract elements, determines the condition to which a choice must be subject in order that we may define on this class a distance such that convergence already defined will not be altered when it is defined by means of this distance.

R. Schauffler (*Math. Ann.* 1917, **78**, 52–62) treats limit-questions and other questions in the theory of iterated functions, and F. Tricomi (*Giorn. di Mat.* 1916, **54**, 35–42) discusses iteration of functions of a line.

W. C. Graustein (*Bull. Amer. Math. Soc.* 1918, **24**, 473–7) gives a theorem on isogenous complex functions of curves.

T. Fort (*ibid.* 330–4) gives some theorems of comparison and oscillation.

O. D. Kellogg (*Amer. Journ. Math.* 1918, **40**, 145–54, 225–34) connects with the theory of integral equations the condition previously found by him (Science Progress, 1916, **11**, 94–5) for many of the oscillating properties of the more common sets of orthogonal functions.

R. G. D. Richardson (*ibid.* 283–316) makes some contributions to the study of oscillation properties of the solutions of linear differential equations of the second order.

J. Horn (*Archiv der Math.* 1916, **25**, 137–48) shows that the theory of non-linear difference equations has a certain analogy with that of non-linear differential equations.

Various points in the theory of the dynamical equations are dealt with by F. Engel (*Gött. Nachr.* 1916, 270–5), E. Vessiot (*Compt. rend.* 1917, **165**, 99–102), and K. Bohlin (*Journ. de Math.* 1916, [7], **2**, 173–200).

C. E. Wilder (*Trans. Amer. Math. Soc.* 1918, **19**, 157–66) discusses some problems in the theory of a system consisting of an ordinary linear differential equation and auxiliary conditions involving linearly the values of the solution and its derivatives

at interior points, as well as at the end points, of the interval over which the equation is considered (cf. Wilder's paper mentioned in SCIENCE PROGRESS, 1918, **12**, 547).

W. D. MacMillan (*Trans. Amer. Math. Soc.* 1918, **19**, 205–22) investigates the reduction of certain differential equations of the second order.

H. J. Ettlinger (*ibid.* 79–96) extends the methods of Bôcher (1905) and Birkhoff (1909)—which are based on the application of Sturm's theorems—to the most general, real, self-adjoint linear system of the second order.

W. E. Milne (*ibid.* 143–56) discusses the degree of convergence of the series (treated by Birkhoff in 1908) built up out of solutions of an ordinary linear differential equation.

T. H. Hildebrandt (*ibid.* 97–108), in connection with his paper referred to in SCIENCE PROGRESS (1917, **12**, 12), discusses boundary-value problems in linear differential equations in general analysis.

M. J. M. Hill (*Proc. Lond. Math. Soc.* 1918, **17**, 149–83) investigates the singular solutions of ordinary differential equations of the first order with transcendental coefficients.

Hill's classification of the integrals of linear differential equations of the first order (SCIENCE PROGRESS, 1918, **12**, 548) should be compared with that independently introduced by H. Bateman's book, to be reviewed in a future number.

Bateman (*Bull. Amer. Math. Soc.* 1918, **24**, 296–301) discusses the solution of the wave-equation by means of definite integrals.

Questions in the solution of integral equations are treated by A. Korn (*Archiv der Math.* 1916, **25**, 148–73) and A. Hoborski (*ibid.* 200–2). Major P. A. MacMahon and H. B. C. Darling (*Proc. Camb. Phil. Soc.* 1918, **19**, 178–84) obtain some interesting reciprocal relations in the theory of integral equations.

W. H. Wilson (*Amer. Journ. Math.* 1918, **40**, 263–82) undertakes a systematic theory of a very general addition formula for functional equations.

Geometry.—M. Pasch (*Journ. für Math.* **147**, 184–90) discusses fundamental questions in geometry; L. Berwald (*Sb. München*, 1916, 1–18) treats curves that are algebraically rectifiable in non-Euclidean space; and E. Study (*Leipzig Ber.* 1916, **68**, 65–92) considers the "principle of the conservation of number" in enumerative geometry.

Questions in the geometry of the triangle and of the circle are treated respectively by N. Altshiller (*Amer. Math. Monthly*, **25**, 241–6) and H. N. Wright (*ibid.* 250–2).

G. H. Light (*Bull. Amer. Math. Soc.* 1918, **24**, 480–1) gives the intrinsic equation for Euler's resistance-integral in his *Scientia Navalis.*

A. Emch (*ibid.* 327–30) gives some theorems on the invariant net of cubics in the Steinerian transformation.

E. F. Simonds (*Trans. Amer. Math. Soc.* 1918, **19**, 223–50) deals with the question of absolute invariants of differential configurations in the plane.

L. P. Eisenhart (*ibid.* 167–85) applies the results of his paper mentioned in SCIENCE PROGRESS (1917, **12**, 13) to a particular class of conjugate systems, namely those which are applicable to one or more other systems. Eisenhart (*Amer. Journ. Math.* 1918, **40**, 127–44) continues his investigations on transformations of planar nets, contained in the paper of 1917 just referred to.

T. Dantzig (*ibid.* 187–212) makes some contributions to the geometry of plane transformations.

Pauline Sperry (*ibid.* 213–24), starting from Wilczynski's five memoirs (1907–9) on projective differential geometry of curved surfaces, discusses the properties of a certain projectively defined two-parameter family of curves on a general surface.

A. L. Miller (*ibid.* 174–86) applies some of the results of investigations of the projective differential properties of geometric configurations in n dimensions by synthetic methods, of which the foundation was laid by Segre (1910), to the study of families of pencils of lines in ordinary space.

P. R. Rider (*Bull. Amer. Math. Soc.* 1918, **24**, 430–1) generalises a well-known theorem of differential geometry concerning the variation of a function.

F. H. Safford (*ibid.* 384–90) continues work of Wangerin (1878) and Haentzschel (1893) on surfaces of revolution in the theory of Lamé's products.

G. M. Green (*ibid.* 221–5) gives a completion and generalisation of his new characterisation of conjugate nets on a curved surface with equal Laplace-Darboux invariants (*Amer. Journ. Math.* 1916, **38**, 287–324).

On algebraic curves and surfaces, see also F. Gomes Teixeira (*Nouv. ann.* 1917, [4], **17**, 281–9), A. Crespi (*Giorn. di Mat.* 1917, **55**, 48–82), and G. Grimaldi (*ibid.* 1916, **54**, 341–64). On

infinitesimal geometry of space, see R. Occhipinti (*ibid.* 335–40, 373–80; 1917, **55**, 31–4), P. Calapso (*Ann. di Mat.* 1917, **26**, 151–90), L. Bianchi (*ibid.* 199–223), S. Finsterwalder (*Abh. München*, **28**), and E. Cartan (*Bull. Soc. Math. France*, 1916, **44**, 65–99).

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

The Classification of Stellar Spectra.—The first four hours of Right Ascension of the Henry Draper Catalogue have been published as vol. 91 of the *Annals of the Harvard College Observatory*. This is the first instalment of the results obtained from an extensive and laborious piece of work which was planned several years ago. The importance of a knowledge of the spectra of the stars was beginning to be realised, for it was found that many properties of the stars depended upon the type of their spectra. The Director of the Harvard Observatory had the foresight to see that a classification according to the class of their spectra of as many stars as possible would be of great value; moreover, the only material which was available for such an investigation which should cover in a uniform manner the whole sky was contained in the Harvard collection of photographs.

The task would have been a heavy one, even if the classification had been confined to the comparatively bright stars, but it was wisely decided to extend it to stars as faint as possible so as to make its early repetition unnecessary. The photographs available had been taken for the northern stars with the 8-inch Draper telescope of the Observatory, and for the southern stars with the Bache telescope, mounted in Arequipa, Peru, a prism being placed over the objective of the telescope in each case. In this way, spectra of all the stars of sufficient brightness in the field of the instrument were obtained on one plate: the photographic image of each star was dispersed by the prism into a line, and these lines were broadened into bands by slightly altering the rate of the telescope clock. Thus, spectra of all stars of sufficient brightness, without exception, were available.

The classification was entrusted to Miss Annie J. Cannon, and was commenced in 1911 and practically finished by the end of 1915. The classification adopted was that previously

formulated by Miss Cannon and which is now universally accepted, having been unanimously adopted by the International Committee appointed by the Solar Union. It is described in full in *H.A.* **28**, and more concisely in *H.A.* **56.** Over 2,100 plates were examined and 242,093 spectra were classified. The results are to be published in nine volumes, of which the first was referred to above. The revision and preparation of copy for press has occupied two years, and the publication of the remaining volumes will cover a further period of two years. As showing the care with which the work has been carried out, the following extract from the preface is of interest: "To secure the greatest efficiency, it was evidently necessary to employ the methods of scientific management. A loss of one minute in the reduction of each estimate would delay the publication of the entire work by the equivalent of the time of one assistant for two years. It was therefore important to study with care each step in the reduction, including the identification, the preparation of the card catalogue and copy for the printer, the determination of the magnitudes, and the checking of the entire work."

The catalogue itself contains a vast mine of useful information in addition to the spectral type. Successive columns contain the B.D. number of the star; its R.A. and Dec. for 1900; and its photometric and photographic magnitudes on the Harvard scale (from which its colour-index can be obtained). Extensive notes give details of any peculiarities in the spectra, and in the case of variable stars the designation, the type of variability, the maximum and minimum magnitudes, the period and (if known) the parallax are given. A reference is also made to any large proper-motions. The catalogue includes stars as faint as the 11th magnitude, although it is not complete to that faintness.

It will readily be realised that the complete catalogue will prove invaluable as a work of reference, for in practically all modern investigations connected with stellar dynamics a knowledge of the types of the stars is necessary. The volumes are therefore being made to stand plenty of thumbing, the paper used containing 80 per cent. of rags, so that it will be practically permanent. Miss Cannon is to be congratulated on the successful completion of so important and vast an undertaking.

The Laws of Statistical Astronomy.—An important review of "The Determination of the Principal Laws of Statistical Astronomy," by W. J. A. Schouten, being an Inaugural Dissertation at Amsterdam, has been published by W. Kirchner of Amsterdam. The chief statistical laws, which are required to summarise our knowledge of the stellar system, are (1) the law determining the variation of stellar distribution in different parts of space, both in and out of the Milky Way; (2) the law of frequency of the luminosity in any part of space; (3) the law of distribution of the velocities. Various methods have been employed in recent years to determine the nature and form of these laws, the results which have been obtained being in some instances markedly discrepant. Dr. Schouten's dissertation contains a critical examination of the principal methods. The data upon which any determination of the fundamental laws must be based are the same, *viz.* (1) the variation of mean proper-motion with galactic latitude; (2) the variation of colour-index with galactic latitude; (3) the distribution of spectral types with respect to the galaxy; (4) the galactic condensation for stars of different magnitudes.

An extensive account is given of the researches of Seeliger, which are critically discussed, objections to the foundations of his method being pointed out. Seeliger's work has been superseded by the later work of Schwarzschild, Kapteyn, and others, but his critical summary will be found of value for purpose of reference, the more so as Seeliger's researches are scattered through many different publications. The methods used by Schwarzschild and Kapteyn are compared: these methods differ fundamentally in that whilst Schwarzschild expresses his data empirically as mathematical functions, Kapteyn uses a method which is more numerical. The former method possesses the greater mathematical elegance, but the latter has the advantages of being able readily to distinguish within what limits of distance or luminosity the results are based on numerical data and to what extent they are mere extrapolations.

Dr. Schouten, as befits a pupil of Kapteyn, is in favour of the latter's method, and the last portion of his dissertation consists of a discussion of the best modern data on that method. The results obtained, as might be expected, agree fairly well with those obtained originally by Kapteyn; it is known that although the data available to Kapteyn were in places very

slender, his results were substantially accurate and have been supported by more recent investigations. It will be remembered that Kapteyn's determination of the galactic condensation, upon which much doubt was thrown, has recently been proved to be accurate. Dr. Schouten's results still further increase our confidence in it.

The Real Motions of the Stars.—An analysis of the real motions of the stars has been given by B. Boss in the *Astronomical Journal*, **31**, No. 736, 1918, based upon 219 stars contained in the list of Adams and Joy, whose radial velocities and parallaxes are known and whose proper-motions are given in Boss's *P.G.C.* For these stars, therefore, the complete elements of their motions are known. It is found that there is evidence of a definite solar group of stars which is moving in the direction $A = 270^\circ \cdot 3$, $D = + 15^\circ \cdot 7$, at the rate of 22·7 cm. per sec., and that this group is composed mainly of giant stars. It is further shown that the solar motion derived from the giant stars alone is in sufficiently close agreement with that derived from the dwarf stars alone to support the conclusion that they all belong to the same system.

From all stars together the position for the solar apex of $269^\circ + 30^\circ$ is deduced, and for the solar velocity $+ 23 \cdot 8$ km., results in agreement with the mean of the results derived from determinations based respectively upon radial velocity and proper motion determinations.

Since the parallaxes of the stars are known, it is possible to separate the giant from the dwarf stars. When the velocity figures for the two classes are drawn, it is found that they differ considerably, a marked effect in the case of the giant stars being produced by the solar group. On the other hand, the orientation of the velocity figure is found to be independent of spectral type. The axes of the velocity figure are, however, dependent upon both type and the giant and dwarf classification.

It is suggested that these results explain why different results have been obtained by various investigators for the pole of preference in the cases of large and small proper-motions and of early and late types. The large proper-motions and late type stars which have been used have been mainly dwarfs, the small proper-motions and early type stars mainly giants.

The Causes Underlying the Spectral Differences of the Stars.—A revolutionary theory as to the causes which underlie the spectral differences of the stars has been suggested by Prof. C. D. Perrine in the *Astrophysical Journal*, **47**, 289, 1918. The usually accepted theory is that the spectral type is conditioned mainly by the stage in its evolution at which a star has arrived; its mass is also concerned in a lesser measure, for a star of small mass will not attain so high a temperature as a star of large mass. Prof. Perrine's theory is that a large part of the characteristics of spectral class among the stars generally are due to external causes depending upon location in the stellar system.

The theory was suggested by the discovery that, in almost all instances at present known, double stars in which the fainter component is of the earlier spectral type are distant from the sun, and therefore from the centre of the stellar system, whilst those in which the fainter component is of later spectral type are comparatively near. Prof. Perrine considers that the difference is due to a difference in the density of cosmical matter in the two regions, and he suggests that the surface conditions of a star are largely determined by the sweeping up of cosmical matter. In this connection emphasis is laid on the phenomena attending the outburst of Nova Persei, which reached the type B stage at about the time of its maximum brightness. The preference of the B type stars for the Milky Way can be explained by supposing that they have passed through stages similar to Nova Persei. Thus in regions where there is much cosmic matter, the direction of spectral change is towards the nebulæ, whilst in regions where there is not much cosmic matter, the effect of radiation will predominate and the direction of spectral change will be towards the later types.

The theory will need to be examined in all its bearings before it will gain general acceptance in preference to the evolutionary theory, which is now supported by a very large amount of evidence.

The following is a selection of the more important papers, etc., recently published:

The Solar System.—VERONNET, A., Constitution Physique du Soleil et des Étoiles, Étudiée au moyen de la formule des gaz réels, *Bull. Astr.* **35**, 101, 1918.

EPSTEIN, TH., Rotationselemente der Sonne, *Ast. Nach.* 4959, 1918.

BAUER, L. A., Relation between the Secular Variation of the Earth's Magnetism and Solar Activity, *Terrestrial Magnetism*, **23**, 1 and 61, 1918.

CHAPMAN, S., The Influence of Changes in Lunar Distance upon the Lunar-diurnal Magnetic Variation, *Terrestrial Magnetism*, **23**, 25, 1918.

MORITZ, R., On the Literal Development of the Motion of the Lunar Perigee, *M.N., R.A.S.* **78**, 615, 1918.

PUISEUX, P., and JEKHOWSKY, B., Sur les inégalités systématiques du contour apparent de la Lune, *Bull. Astr.* **35**, 161, 1918.

AUDOYER, H., Formules et tables nouvelles relatives à l'étude du mouvement des comètes et à différents problèmes de la théorie des orbites, *Bull. Astr.* **35**, 5, 1918.

PHILLIPS, T. E. R., The Motion in Longitude of the Red Spot on Jupiter, *M.N., R.A.S.* **78**, 630, 1918.

ANHANGEN, W., On the Apparent Apsides of a Satellite as published in the American Ephemeris and Nautical Almanac, *Astron. Journ.* **31**, No. 740, 1918.

MAGGINI, M., La Planète Mars en 1918, *L'Astronomie*, **32**, 261, 1918.

Variable and Double Stars.—LUIZET, M., Éphémérides des Étoiles variables pour l'année 1917, *Journ. des Observateurs*, **2**, No. 8, 1918.

HENROTEAU, F., A Spectroscopic Study of β Canis Majoris, *Lick Obs. Bull.*, No. 311, 1918. A Spectroscopic Study of σ Scorpii: The Radial Velocities of the Stars ϵ and γ Orionis, *Lick Obs. Bull.* No. 314, 1918.

CAMPBELL, LEON, Observations of 323 Variable Stars during years 1911–19, *Harvard Annals*, **79**, pt. i. 1918.

UNION OBSERVATORY, Observations of Variable Stars, *Union Obs. Circ.*, No. 42, 1918.

SANFORD, R. F., The Orbit of the Spectroscopic Binary, ρ Velorum, *Lick. Obs. Bull.* No. 315, 1918.

AITKEN, R. G., The Orbit of Sirius, *Lick Obs. Bull.* No. 316, 1918.

Stellar Motions, etc.—DYSON, SIR F. W., and THACKERAY,

W. G., The Parallaxes of the B Stars which lie between the limits of 4h. to 8h. R.A. and + 24° to + 32° Dec., *M.N., R.A.S.* **78,** 651, 1918.

MacMillan, W. D., On Stellar Evolution, *Astroph. Journ.* **48,** 35, 1918.

Shapley, Harlow, Studies based on the Colours and Magnitudes in Stellar Clusters ; 6th paper : On the Determination of the Distances of Globular Clusters, *Astroph. Journ.* **48,** 89, 1918.

Lense, J., Kinetische Gastheorie und Fixsternsystem, *Ast. Nach.* No. 4958.

Tucker, R. H., The Visual Magnitude Scale and the Counts of Stars, *Lick Obs. Bull.* No. 312, 1918.

Miscellaneous.—Sampson, R. A., On the Measurement of Time to the Thousandth of a Second, *M.N., R.A.S.* **78,** 592, 1918.

Sanford, F., The " Astronomical Atom " and the Spectral Series of Hydrogen, *Astroph. Journ.* **48,** 1, 1918.

King, A. S., The Variation with Temperature of the Electric Furnace Spectra of Ca, Sr, Ba and Mg, *Astroph. Journ.* **48,** 13, 1918.

Haussmann, A., Über die Ausbreitung einer ebenen Lichtwelle in einen Medium mit Kontinuierlich variablem Brechungsindex, *Ast. Nach.* No. 4953, 1918.

Maunder, Mrs. A. S. D., The Date and Place of Writing of the Slavonic Book of Enoch, *Observatory,* **41,** 309, 1918.

The following volume contains much valuable information and the chief contents are therefore summarised :—

Publications of the Astronomical Observatory of the University of Michigan, vol. ii. 1916.

(1) Studies of Class B stellar spectra containing emission lines.
   (*a*) The spectrum of γ Cassiopeiæ.
   (*b*) Changes in the spectrum of f^1 Cygni.
   (*c*) Changes in the spectrum of H.R. 985.
(2) Spectroscopic observations of stars of class Md.
(3) A spectrum of the P Cygni type.
(4) The spectrum of ζ Ursæ Majoris.

(5) An investigation of the spectra of stars belonging to class R of the Draper classification.
(6) A study of β Cephei.
(7) The spectrum and radial velocity of ρ Leonis.

PHYSICS. By JAMES RICE, M.A., University, Liverpool.

IN the *Proc. Roy. Soc.*, June 1918, Professor Strutt gives an account of experiments to test if light is scattered by dust-free air, and finds that by proper arrangement of experimental conditions it is possible to observe the scattering by pure air, free of dust, in a small-scale laboratory experiment. Other gases also scatter, hydrogen much less than air, oxygen about the same, carbon dioxide decidedly more. The scattered light is in all cases blue and is almost completely polarised, illustrating directly the well-known theory of the blue sky. In the *Phil. Mag.* for September last, Prof. Wood, criticising Strutt's research from the point of view of some earlier, unpublished work of his own, suggests that the effect was due to a cloud resulting from the action of the ultra-violet rays in the light on the air. In the October number of the *Phil. Mag.* Strutt replies to the effect that he was aware of the necessity of guarding against spurious results such as Wood suggests, and that the intensity of the light which he used, and the fact that it could not have been rich in ultra-violet rays, were such as to guarantee the soundness of his conclusion. He promises the publication of further results in a forthcoming number of the *Proc. Roy. Soc.*

In the *Phil. Mag.* for September G. A. Hemsalech gives an account of a research on the comparison of the flame and furnace spectra of iron. He summarises his work in the statement that the spectra of iron as given by an electric-tube resistance furnace at atmospheric pressure and up to a temperature of about 2,400° C. are caused by the action of heat on a chemical compound of the metal and not on the free metal itself. Hence these spectra are not of purely thermal origin. An iron spectrum observed at as low a temperature as 1,500° C. is the same as that given by an air flame burning in coal gas. The spectra emitted by iron compounds in flames are identical with those given by the furnace at corresponding temperatures up to about 2,400° C. From this and other considerations the author concludes that the mode of excitation is the same in

the two cases, namely chemical dissociation of an iron compound by the action of heat, a mode to which he gives the name *thermo-chemical excitation.* The character of the spectrum is independent of the nature of the iron compound, which is acted on by the thermal forces in either flame or furnace, chlorides, oxides, etc., all giving the same kind of spectrum in either of these sources at a given temperature. Thermo-chemical excitation is quite distinct from chemical excitation, which takes place at a comparatively low temperature in the explosion region of the air-coal-gas flame, since the spectra emitted by the compounds of iron in this region differ completely from those mentioned above. The author makes some interesting observations on the condition of the iron atom during such thermo-chemical excitation, the gist of which is a belief on his part that the iron atom during such thermo-chemical excitation is not freed from the other atoms of the compound molecule even up to a temperature of 2,700° C., although no doubt the relative positions of these atoms are much disturbed. The spectra are so restricted in range, although very intense, as to suggest that the atoms emitting them are not so free as the atoms which, under the action of the powerful electric forces prevailing in the arc and condenser spark, emit the well-known and wide-ranging iron spectrum. A further paper on the "Origin of the line spectrum emitted by iron vapour in an electric tube furnace at temperatures above 2,500° C.," by the same author, is printed in the October number of the *Phil. Mag.*, and adds further support to the hypothesis, leading to a discussion of five possible modes of excitation of the atoms of iron; one thermal, one chemical, two thermo-chemical, and one electrical.

The July number of the *Proc. Roy. Soc.* contains two short papers by Prof. Fowler and C. C. L. Gregory, on the identification of the ultra-violet band of Ammonia and group P in the ultra-violet of the solar spectrum giving proof of the presence of ammonia in the sun, and on the presence in the solar spectrum of the water-vapour band $\lambda 3064$, as furnishing further evidence of the existence of oxygen in the sun.

In the October number of the *Proc. Roy. Soc.* Prof. Collie and Dr. Watson give an account of some experiments on the appearance of lines due to the cathode metal in an electric discharge through an inactive gas. The metal chosen for the

cathode was cadmium, and the gases were helium, neon, argon, and xenon. By suitable arrangements it was possible to observe the lines of cadmium along with the gas lines excited by the discharge even at comparatively high pressures and low current densities. It is difficult to account for the appearance of the metallic lines. They can appear with very feeble currents and the electrode and tube quite cold. The spectrum may be due to small particles of the metal at a high temperature torn off by the violent impact of positively charged gas ions, but this is hard to reconcile with the behaviour of the lines in different gases and under different pressures, as for instance the fact that not all the spark spectrum of the metal is seen at once, but only certain lines depending on the nature and pressure of the gas in the tube.

The *Physical Review* for June contains a paper by Wick and McDowell on the luminescence of the uranyl salts under cathode-ray excitation. The October number of the same journal contains a paper by T. K. Chinmayanandam on the diffraction of light by an obliquely held cylinder, with an account of experiments made to test certain theoretical results.

The *Phil. Mag.* for November 1918, and the *Proc. Phys. Soc.* for December 1917, Feb. and June 1918, contain papers by T. Smith on such topics as tracing rays through optical systems, use of approximate methods in obtaining data for the construction of telescope objectives, and on multiple thin objectives. Other optical papers in the thirtieth volume of the *Proc. Phys. Soc.* are: "An exhibition of the uses of certain methods of classification in Optics," by T. H. Blakesley. "The primary monochromatic aberrations of a centred optical system," by S. D. Chalmers. "Note on the Pulfrich Refractometer," by J. Guild. "The accuracy attainable with critical angle refractometers," by F. Simeon.

The August number of the *Proc. Roy. Soc.* contains a very interesting account of a statistical research which has been carried out in the colour-perception of about eighty students of the University of Glasgow, by Dr. Houstoun of the staff of that University. The object was to determine if normal colour-vision passes gradually into colour-blindness, or if the colour-blind form a well-defined class by themselves. Thus each person tested was assigned a "mark" which indicated the extent of his colour-vision, the "mark" being 1 for total

colour-blindness, and increasing with the person's perception of colour. By means of a specially designed spectrometer each observer was made to divide the spectrum into a number of "patches," such that each patch appeared to him or her to be of one colour. The more perfect the perception of colour, the greater the number of patches, and this number constituted the observer's "mark." The "mark" was as high as 26 for one student and as low as 5 for two. The results were graphed in the usual "frequency-curve" way, "marks" being plotted along a line as abscissal, the ordinates being the number of observers with the corresponding mark. The important point now arises as to whether we get a frequency-curve of the usual type with one maximum, or not. If so then colour-blindness corresponds merely to an outlying portion of the main curve, near to the origin. But if the curve shows, say, two peaks, a main one over say the mark 20 and a smaller but still definite maximum over say 10, with a decided minimum between the two peaks, say over 12, it would seem that the colour-blind are a definite class by themselves with a definite maximum of their own. This latter result would correspond with the Young-Helmholtz theory, the main class with their large maximum at, say, 20, being "trichromats," the smaller class with their maximum at 10 being the "dichromats," or persons not possessing one of the three nerve sets of the Young-Helmholtz theory. Dr. Houstoun's results certainly show enough of the biometrician's "scatter" to hazard the guess that colour-blindness is merely an outlying portion of normal colour-vision. If this is so the result would be hard to reconcile with the Young-Helmholtz theory. But it is clear from the results that many more observers than 80 are required to disprove the existence of separate maxima for the trichromats and dichromats, and perhaps even for monochromats.

A paper in the May number of the *Physical Review*, by Q. Majorana of Turin, discusses the possibility of testing by direct experiment the validity of the second postulate of the Relativity hypothesis, viz. that an observer who measures the velocity of light finds the value the same whether both he and the source are relatively at rest or whether either he or the source both are in uniform motion. The author makes use of a device in which a number of vertical mirrors are mounted on the periphery of a horizontal brass wheel. By means of these

and a number of fixed mirrors, monochromatic light corresponding to the green mercury line was directed, after a series of reflections, on to a Michelson interferometer. The wheel was now rotated about eighty times per second, and according to the second postulate applied to the Doppler effect so produced, there should have been a displacement of about 0·7 of a fringe for the dimensions of the apparatus used. Majorana asserts that he obtained a displacement between 0·7 and 0·8 of a fringe, and considers this result to be in direct support of the postulate. He promises further work and results in a later publication. In view of the fact that his experiment is a "positive" test and not "negative" like the now classical researches of Michelson, Morley, Trouton, Noble, etc., his further results and criticisms should engage some attention.

The London Physical Society has just published a report on *The Relativity Theory of Gravitation* from the pen of Professor Eddington. In view of the difficulty of obtaining the papers in which, during the years 1915 and 1916, Einstein developed his hypothesis to its present form, this volume of ninety pages is extremely welcome to physicists. Its first chapter is devoted to a brief account of the early or restricted principle of relativity. In Chapter II. the relations of Space, Time, and Force are dealt with, and Einstein's new principle of Equivalence explained. Chapter III. gives sufficient of the absolute differential calculus of Ricci and Levi Civita (a comparatively new mathematical weapon admirably adapted for the development of ideas involved in Einstein's work) to enable the reader to follow the analysis. Chapters IV., V., VI., deal with Einstein's law of gravitation, the resulting equations of motion and the crucial phenomena, such as the motion of the perihelion of a planet and the deflection of a ray of light by the gravitational field of a sun or planet. The seventh chapter deals with the reduction of the mathematical equations to the form required by the principle of Least Action, and the eighth and final chapter treats of the possibility of absorbing "absolute rotation" within the framework of generalised relativity.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Chemical Reaction and Ionisation.—Since it was first recognised that the electron plays a fundamental part in the structure

of the atom, considerable attention has been devoted to the problem of discovering the part played by the electron in chemical affinity and in the mechanism of chemical change. This problem has been investigated from several points of view, with a certain, though limited, amount of success.

One of the most promising of the different modes of investigation is that in which the attempt is made to determine whether chemical reactions in general involve the production of free charged particles, and if so, to determine the constitution of such particles. It has long been recognised that many reactions which occur in solution, especially when the dielectric capacity of the solvent is great, involve the production of charged ions which are capable of taking part in definite chemical reactions. Such processes are already familiar to us in the well-known phenomenon of electrolytic dissociation. This, however, only clears up a part of the problem. There still remains the question of the production of charged particles, possibly as an intermediate stage, in the case of reactions in which no ionising solvent is present, reactions, in short, between pure substances.

During the past ten years numerous researches have been carried out by Haber, Tanatar, Bloch, de Broglie, and others upon the ionisation produced in gases when a gas reacts with a solid or liquid, *e.g.* an alkali metal or an alloy of such metals. Definite evidence of ionisation has been obtained in these cases, but the interpretation of the results is much complicated by the fact that such reactions are heterogeneous, and consequently physical surface effects may be entering into the phenomenon in addition to any effects brought about by the chemical change itself. To obtain the simplest conditions it is necessary to investigate homogeneous reactions; in particular, reactions between gases which are capable of occurring with sufficient rapidity at ordinary temperatures. The first step in this direction has been made by A. Pinkus (*Journ. Chimie physique*, **16**, 201 (1918)). The problem to be settled is simply this: does a chemical reaction *of itself* entail the production of gaseous ions?

Pinkus has investigated the two reactions:

$$2NO + O_2 = 2NO_2$$
$$\text{and } 2NO + Cl_2 = 2NOCl$$

In both cases the gases were prepared and purified with the greatest care so as to eliminate moisture, dust, etc., which might have effected the result. The general experimental arrangement consisted in using three bulbs, which could be evacuated, each bulb being fitted with an electrode of platinum-iridium connected to an electrometer. The electrode could be charged positively or negatively as desired. The two outer bulbs were connected to the separate gas reservoirs, the reaction being carried out in the central bulb. The natural rate of discharge is observed and plotted, and at a certain moment the gases are allowed to mix. If ionisation occurs it will be shown by an abrupt change in the slope of the curve obtained from the electrometer readings. The sensitivity of the method was tested and shown to be satisfactory, by measuring the ionisation produced on bubbling air through water.

The union of oxygen and nitric oxide had been examined previously by several investigators from the point of view of the production of ions, the conclusion finally arrived at being that no ionisation occurs in this reaction. This conclusion has been completely substantiated by Pinkus ; the time curve is perfectly continuous.

In the reaction between chlorine and nitric oxide, however, quite different results are obtained. Pinkus has found that when the chlorine is in excess the reaction is accompanied by ionisation. Positive as well as negative ions are produced. If, however, the chlorine is not in excess, or only in slight excess, there is no break in the curve, *i.e.* apparently no ionisation occurs. It is difficult to suggest an explanation of this phenomenon. It is possible that, in presence of excess chlorine, the unstable compound $NOCl_2$ (assumed by Trautz and his collaborators) may be formed temporarily, and that the ionisation may be due to this. On the other hand, as Pinkus points out, there is no definite evidence that such a compound is formed, and even if it were, it would not necessarily give an explanation of the phenomenon, for in the previous reaction there is undoubtedly formation of intermediate compounds such as N_2O_3, and yet this reaction is not accompanied by ionisation. Pinkus concludes that the observed ionisation is due simply to the gaseous reaction as a whole ; that is, we have a demonstration for the first time that a homogeneous gaseous

reaction is capable of giving rise to ions of both kinds. The nature of these ions has not yet been determined.

Whilst the investigation of Pinkus represents an important preliminary step, it is obvious that much remains to be done. Thus it would be very desirable to know if a simple gaseous dissociation such as that of molecular iodine into its atoms is accompanied by ionisation. That is, whether the reaction goes according to the scheme :

$$I_2 = 2I$$

or according to the schemes :

$$\left.\begin{array}{r} I_2 = I^+ + I^- = 2I \\ \text{or } I_2 = 2I^+ + 2(-) = 2I \end{array}\right\}$$

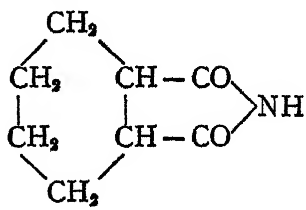
So far as the writer is aware nothing is known about such a point. Undoubtedly the necessary temperature would render the experimental test difficult. In the same connection the question arises as to the relation of the so-called " resonance " potential and the ionisation potential of a gas to the chemical change involved. Even yet we do not appear to be in a position to state exactly what occurs when a gas is ionised.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

A NEW method of preparing aldehydes is described by Rosenmund (*Berichte*, 1918, **51**, 585) ; it consists in reducing an acid chloride dissolved in dry xylene by means of hydrogen bubbled through the boiling solution in the presence of palladinised barium sulphate containing 5 per cent. of metal ; hydrogen chloride escapes through a reflux condenser and the aldehyde may finally be isolated by distillation from the sodium bisulphite compound. The method gives very good yields, and appears to be one of the most convenient methods yet devised for the preparation of aldehydes. Treated in this way benzoyl chloride gave a 97 per cent. yield of benzaldehyde, while butyric aldehyde was obtained in 50 per cent. yield from butyryl chloride.

An interesting example of the influence of conditions upon the catalytic activity of platinum has been recently published by Willstätter and Jaquet (*Berichte*, 1918, **51**, 767). It has been shown by Hesse (*Berichte*, 1913, **46**, 3120) that for the

successful reduction of pyrrole by means of platinum black and hydrogen it is essential that all traces of oxygen should be excluded. The present authors find that for the reduction of phthalic anhydride the exact opposite holds; if phthalic anhydride dissolved in glacial acetic acid is treated with hydrogen in presence of platinum black very little hydrogen is absorbed unless the apparatus is opened from time to time and agitated in contact with air. Under these conditions reduction proceeds smoothly with the formation of phthalide and *o*-toluic acid and subsequently of hexahydrophthalide, hexahydrotoluic and hexahydrophthalic acid. In this reduction of phthalic acid the oxygen ring is attacked first, and the benzene ring subsequently, but in the case of phthalimide, which can only be reduced by the best spongy platinum, not necessarily activated by oxygen, the benzene ring only is attacked with the formation of hexahydrophthalimide.



The peculiar observation has further been made that unless the platinum is thus activated by oxygen many reductions of acids which would normally take place are inhibited by the presence of traces of anhydrides; thus, for example, phthalic acid free from its anhydride can be reduced to hexahydrophthalic acid, but no reduction takes place if there is any phthalic anhydride present.

It was shown last year by Baudisch that when a solution of glucose is boiled with ferrous sulphate and sodium carbonate a dark coloured solution results which contains iron in the form of an internal complex, or so-called "masked iron." This solution will reduce alkali nitrites to nitric oxide and ammonia or nitrobenzene to aniline. Baudisch now finds (*Berichte*, 1918, **51**, 793) that the glucose in the above solution can be replaced by any aldose or ketose in general, such as dihydroxyacetone, glycollic aldehyde, lævulose, maltose, etc., but not by alcohols such as glycerol or mannitol. The same property is also shared by a number of autoxidisable substances such

as catechol, quinol, pyrogallol, gallic acid, quercetin, chrysarobin, etc. ; all these bodies take up oxygen and yield hydrogen peroxide in the presence of water, and can only reduce in the presence of oxygen, whereas in the case of the sugars the necessary oxygen is supplied by the sugar molecule itself. Some of the polyhydric compounds mentioned can make the complex themselves out of ferric hydroxide, but not all of them, as, for example, phloroglucinol, which can only produce it from potassium ferricyanide. It is not necessary for the autoxidisable compound to be part of the iron complex since, for example, the iron complex may be applied with catechol carboxylic acid. It is interesting to note that these autoxidisable substances, which are widely distributed in nature, may act in the presence of oxygen either as oxidisers (*e.g.* converting methylalcohol to formaldehyde), or as reducers, converting nitrites in presence of complex iron into ammonia.

The formation of nitrites from nitrates in aqueous solution forms the subject of a paper by Moore (*Proc. Roy. Soc.* 1918 [B] **90**, 158). Rain water which has been kept for some time contains only nitrates, as all nitrite has been oxidised ; if, however, such rain water is exposed to sunlight a marked nitrite reaction can be obtained from it ; but if leaves are immersed in the solution very little nitrite accumulates. In the author's opinion nitrites which are more active than nitrates are formed from the latter on the green leaf exposed to sunlight, and form the first stage in the synthesis of nitrogenous compounds.

Willstätter and Schudel (*Berichte*, 1918, **51**, 781) describe a new method of extracting organic colouring matters from aqueous solution by means of organic solvents. It was shown some time ago (*Annalen*, 1916, **412**, 113–231) that plant pigments could to a certain extent be extracted by a process depending on the partition of the pigment between water and amylalcohol. Only non-glucosidic pigments could, however, be extracted efficiently by this process, the glucosidic ones remaining in aqueous solution. By adding picric acid to the solution, however, a larger number of pigments become amenable to extraction, as picrates, especially if a mixture of amylalcohol (2 parts) and acetophenone (1 part) is used. It is now found that dichloropicric acid is preferable to picric acid, since the compounds which it forms with dyes are more frequently soluble in organic solvents. Thus in presence of dichloropicric

acid rosaniline, pararosaniline, and methylene blue can be completely extracted from aqueous solution by means of ether, safranine being extracted by means of diethylketone, while glucosidic pigments are completely removed by one extraction with the amylalcohol acetophenone mixture.

A new method of estimating glucose in solution is described by Willstätter and Schudel (*Berichte*, 1918, **51**, 780); the solution whose strength is to be determined is mixed with twice the amount of N/10 iodine solution necessary for the oxidation of the glucose to gluconic acid according to the equation

$$C_6H_{12}O_6 + 3NaOH + I_2 = 2NaI + 2H_2O + CH_2OH(CHOH)_4COONa$$

and decinormal caustic soda is then added. After some time the solution is acidified with sulphuric acid and the excess of iodine is titrated. The error is less than 0·1 per cent. for solutions containing 1 per cent. of sugar, and less than 1·5 per cent. for those containing about 0·1 per cent. Ketoses and cane sugar are not affected by the reaction, and the method may consequently be used for determining glucose in admixture with these substances.

Harden and de Zilwa (*Biochem. J.* 1918, **12**, 93) have published a paper entitled "Differential Behaviour of Antineuritic and Antiscorbutic Factors towards Adsorbents." They find that the antineuritic substance contained in autolysed yeast is quantitatively adsorbed by Fuller's earth or dialysed iron. If equal volumes of autolysed yeast containing antineuritic substance and lemon juice containing antiscorbutic substance are treated with Fuller's earth the former is quantitatively removed, whilst the antiscorbutic substance remains unchanged. Attempts to regenerate the antineuritic substance from its adsorption complex have so far been unsuccessful.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Physical Geology.—Jutson, J. T., Erosion and the Resulting Land Forms in Sub-arid Western Australia, including the Origin and Growth of the Dry Lakes, *Geogr. Journ.* Dec., 1917, 418–37.

——, On the Formation of "Natural Quarries" in Sub-arid Western Australia, *Proc. Roy. Soc. Vict.* 1918, **30** (N.S.), pt. 2, 159–64.

JUTSON, J. T., The Influence of Salts in Rock Weathering in Sub-arid Western Australia, *ibid.* 165–72.

——, The Rock-cliffs and Floors of the "Dry" Lakes in Western Australia, *Geol. Mag.* 1918, **5**, 305–13.

The four papers listed above are a continuation of Jutson's excellent work on the physiographic geology of the sub-arid regions of Western Australia. In them he advances the view that the great "dry lakes" of that country are due mainly to wind erosion. The floors of the lakes are abnormally smooth and level, with billiard-table surfaces of rock especially along their margins. The western boundary is usually a line of steep cliff, on other sides sand tracts and dunes. The cliff boundaries of some of the lakes are rapidly receding under the influence of erosion due to a combination of insolation, exsudation, atmospheric weathering, and wind-erosion. The billiard-table floors are believed to have been produced, concurrently with the recession of the cliffs, by the erosive activity of the wind. Other features of sub-arid erosion, such as the formation of natural quarries, stone fields and pavements, desert-polish, and the process of exsudation (salt-weathering), are considered in this series of papers.

KINDLE, E. M., Notes on Sedimentation in the Mackenzie River Basin, *Journ. Geol.* 1918, **26**, 341–60.

Stratigraphical and Regional Geology.—Another part of the *Handbuch der Regionalen Geologie* has come to hand (*Handbuch der Reg. Geol.* 1916, Bd. 3, Abth. 1, 1–12, 213–52), in which Dr. A. M. Davies briefly describes the tectonics and physiography of the British area; and also deals with the stratigraphy of the Jurassic and Lower Cretaceous of Great Britain.

BRYDONE, R. M., The Thickness of the Zone of *Belemnitella mucronata* in the Isle of Wight, *Geol. Mag.* 1918, **5**, 350–4.

BAKER, H. A., On Successive Stages in the Denudation of the Chalk in East Anglia, *ibid.* 412–16.

HARMER, F. W., The Stratigraphical Position of the Coralline Crag, *ibid.* 409–12.

HOWORTH, SIR H., The Recent Geological History of the Baltic and Scandinavia, and its Importance in the Post-Tertiary History of Western Europe, *ibid.* 354–67; 397–409; 451–61.

A memoir by M. E. Wilson on Timiskaming County, Quebec (*Geol. Surv. Canada, Memoir* **103**, 1918, 197 pp.) is chiefly

remarkable for an outspoken condemnation of the practice of using a common nomenclature for the huge Pre-Cambrian region of the St. Lawrence basin, when only a small part of it has been surveyed in any detail. Especially misleading is the widespread use of the term Keewatin for all occurrence of schistose basic volcanic rocks apparently at the base of the Pre-Cambrian succession, with the implication that these are all of the same age. The author also believes that there are at least three periods of batholithic intrusion in the Canadian Shield ; thus the vexed question of Pre-Cambrian correlation becomes more complicated than ever. Mr. Wilson returns to this subject in a separate paper (*Journ. Geol.* 1918, **26**, 325–33).

Coleman, A. P., Permo-Carboniferous Glacial Deposits of South America, *Journ. Geol.* 1918, **26**, 310–24. (Describes three widely separated areas of tillite in Brazil and Argentina.)

Kirk, E., Palæozoic Glaciation in South-eastern Alaska, *Amer. Journ. Sci.* 1918, **46**, 511–15. (Describes Silurian and Permian glacial deposits.)

The Tuapeka district (New Zealand), described by P. Marshall (*Geol. Surv. New Zealand*, Bull. No. **19**, 1918, 79 pp.) is part of the main folded axis of the South Island of New Zealand. It consists principally of Pre-Jurassic greywackes and schists, followed unconformably by an auriferous conglomerate of Upper Cretaceous or Lower Eocene age. This district was formerly the chief gold-field of New Zealand.

Thomson, J. A., Diastrophism and Other Considerations in Classification and Correlation, and the Existence of Minor Diastrophic Districts in the Notocene, *Trans. N.Z. Inst.* 1917, **49**, 397–413. (Diastrophism discussed in relation to New Zealand stratigraphy.)

Petrography.—Dr. A. Holmes has described a collection of basaltic rocks from the Arctic regions (Greenland to the Yenesei) with the aid of nine full chemical analyses by Dr. H. F. Harwood (*Mineral. Mag.* 1918, **18**, 180–223). He shows that the Brito-Arctic petrographic province is composite, and that it is characterised by basaltic rocks which show a regional variation in composition. In the olivine-free basalts consanguinity is mainly indicated by an antipathetic relation between felspars and titania. There is a well-marked geographical distribution of the last-named constituent. The basalts of the partly submerged North Atlantic ridge, which includes South Green-

land, Iceland, and the Faroes, are much richer in TiO_2 than the areas respectively north and south of it. The paper concludes with a short but valuable discussion of the conception of petrographic provinces.

Balsillie, D., On a Hypersthene Andesite from Pitcullo, Fife, *Geol. Mag.* 1918, **5**, 346–50.

Bartrum, J. A., Additional Facts concerning the Distribution of Igneous Rocks in New Zealand, *Trans. N.Z. Inst.* 1917, **49**, 418–24.

Grout, F. F., The Pegmatites of the Duluth Gabbro, *Econ. Geol.* 1918, **13**, 185–97.

"Lopolith" is a term suggested by F. F. Grout for an igneous form exemplified by the Duluth gabbro, the Sudbury nickel intrusive, the Bushveldt complex (and probably also the Insizwa mass elsewhere mentioned) (*Amer. Journ. Sci.* 1918, **46**, 516–22). It is defined as a large, lenticular, centrally-sunken, generally concordant, intrusive mass, with a thickness approximately one-tenth to one-twentieth of its width or diameter. The type differs widely from a laccolith, with which it has generally been classed, both in form and in its mechanics of intrusion.

In a discussion of the internal structures of igneous rocks, F. F. Grout (*Journ. Geol.* 1918, **26**, 439–58) comes to the conclusion that banding and related fluxion-structures probably develop during crystallisation, while the magma is in convection-circulation. The author cogently criticises the current suggestions of movements of intrusion or deformation for the origin of parallel, alternating, mineralogically-unlike bands.

Economic Geology.—Ore-Deposits and Igneous Magmas.—The subject of the direct relation of ore deposits to igneous magmas has been much discussed of late, as shown by the large number of contributions to the study of the Sudbury nickel intrusive (see Science Progress, April 1918, p. 567, and present article). Among British authors W. H. Goodchild has recently made some notable studies of the general problem. He has just published a paper on "The Evolution of Ore-Deposits from Igneous Magmas" (*Mining Mag.* 1918, **18**, 20–9; 75–82; 131–41; 186–94; 240–9; 296–306; **19**, 21–32; 78–88; 135–44; 188–99), which is really a long and elaborate petrological treatise, shortly to appear in book form. This work is deserving of a much more extended notice than can be given

here. Its aim, briefly, is to apply metallurgical principles to the problem of the separation of ores from igneous magmas, and to petrogenic problems in general.

In a study of the great Insizwa intrusion of Natal (*Inst. Min. & Metall.* 1916, Bull. No. 147, pp. 1–47) Goodchild shows that, while Insizwa has been regarded as the "Sudbury of South Africa," there is really only a distant geological similarity between the two eruptives. They are more or less alike in size, thickness, mode of differentiation, and ore minerals; but the Insizwa intrusion is, on the whole, much more basic than that of Sudbury, its differentiation is due to the sinking of olivine rather than pyroxenes, and its ore minerals show a different sequence in time. The Sudbury basin is believed to have been formed by subsidence subsequent to emplacement; the Insizwa mass, however, is believed to have been intruded into soft clay which yielded under its weight, causing the most decided subsidence towards the centre of the intrusion where the thickness was greatest. The picrite differentiate which carries the ore minerals is thought to be due to gravitative concentration of olivine, which naturally took place with the greatest facility in the thickest part of the mass. The similar concentration of the ore minerals is regarded as due partly to gravitative descent, comparable to the settlement of matte in smelting, and partly to mechanical entanglement of the sulphides with sinking olivines.

Goodchild applies metallurgical conceptions to the problem of the Sudbury nickel eruptive (*Econ. Geol.* 1918, **13**, 137–43). He regards the separation of the sulphide ore as a process similar to "matte smelting" under hydrous conditions. A reaction between the sulphur of the dissolved sulphuretted hydrogen, and the iron oxides of the basic magmatic silicates, results in the formation of metallic sulphides. A volume increase of sulphides relative to silicates takes place, giving rise to silicate replacement, and producing brecciation and expulsion of the matte into the adjacent country rock. Water formed as a by-product of the above reaction gives rise to the local appearance of hydrothermal alteration which has led some investigators to the view that the ores were introduced by circulating waters after the consolidation of the rock. Goodchild thus supports the view that the sulphide ores are of direct magmatic origin. In a quantitative petrological study of the

Sudbury nickel eruptive M. A. Dresser (*Econ. Geol.* 1917, **12**, 563–80) shows that the norite is frequently schistose and brecciated, but the abundant fractures are found in the early-formed minerals. This is regarded as an indication of deformation before complete solidification, probably due to the development of the Sudbury syncline. The sulphides, quartz, and micropegmatite were all late in crystallisation, and were still molten at the time of deformation. It is maintained that the sulphides are chiefly of magmatic origin.

Metalliferous Ores.—The chief factor in the genesis of tungsten deposits (R. H. Rastall, *Geol. Mag.* 1918, **5**, 193–203; 241–6; 293–6; 367–70) is the differentiation of igneous magmas, especially those of granitic composition. Tungsten occurs in association with tin, molybdenum, arsenic, fluorine, and boron, as a concentrate from the final aqueous residuum of granite magmas, and is thus found mainly in pegmatites and quartz veins. While this constitutes the typical paragenesis of tungsten ores, certain other types are characterised by an association with uranium, niobium, and tantalum. In still other occurrences transitions are traceable to groups of ore-deposits containing gold, silver, copper, zinc, and lead.

According to A. L. Parsons the molybdenite deposits of Ontario (*Ont. Bur. Mines*, 1917, 26th Annual Report, 275–313) are contact ores, usually developed in a band of pyroxenite intervening between granite or pegmatite and crystalline limestone. Pyrite and pyrrhotite are almost always present in these deposits.

Thomson, E., A Pegmatite Origin for Molybdenite Ores, *Econ. Geol.* 1918, **13**, 302–13.

Dolmage, V., The Copper-Silver Veins of the Telkwa District, British Columbia, *ibid.* 349–80.

Ball, L. C., The Arbouin Copper Mines at Cardross, North Queensland, *Queensland Geol. Surv.*, Publ. No. **261**, 1918, 70 pp.

During 1917, the Geological Survey of Great Britain (*Summ. Prog. for* 1917, 1918, 55 pp.) carried out an investigation of the iron ore resources of this country. Considerable difficulty was encountered in estimating reserves, especially where the iron ore occupies lodes or pockets; but of reserves "more or less developed" there is estimated to be 2,883,189,160 tons; with a "probable additional reserve" of 8,428,361,600 tons.

Dr. F. H. Hatch has also published an excellent summary

of the geology, distribution, composition, methods of working, and output of the Jurassic ironstones of the United Kingdom (*Journ. Iron and Steel Inst.* 1918, **97**, 71–125), the production of which in 1917 was over eleven million tons, with an average iron content of 27·6 per cent.

An iron formation, consisting of jasper, chert, hæmatite, magnetite, siderite, and greenalite, of Pre-Cambrian age and closely resembling the iron formation of the Lake Superior region, has been found by E. S. Moore in the Belcher Islands of Hudson Bay (*Journ. Geol.* 1918, **26**, 412–38). These rocks are associated with thick sediments and basic lava flows, resembling the Animikee and Keweenawan formations of Lake Superior. There are also beds of a remarkable limestone, consisting of concretions one to fifteen inches in diameter, so similar to the modern concretions formed by blue-green algæ that there can be but little doubt that they are also of algal origin.

Broderick, T. M., The Relation of the Titaniferous Magnetites of North-eastern Minnesota to the Duluth Gabbro, *Econ. Geol.* 1917, **12**, 663–96.

Origin of Laterite.—In a comprehensive paper on laterite J. M. Campbell (*Mining Mag.* 1917, **17**, 67–77; 120–8; 171–9; 220–9) defines laterisation as the process by which the hydroxides of iron, aluminium, and titanium are deposited within the mass of porous rocks near the surface. He believes that unaltered or impermeable rock is incapable of being laterised; and that the process cannot take place when the rock is out of contact with the atmosphere and vadose water. If by reason of the erosion of a river valley a layer of laterite is left above vadose water level it ceases to grow; and this may be the explanation of the frequent terraced arrangement of laterites. On the other hand laterites which, owing to faulting, have passed below vadose water level have their iron leached. Most of the "bauxites" of commerce are believed to have originated in this way.

In a study of West Australian laterite Professor W. G. Woolnough (*Geol. Mag.* 1918, **3**, 385–93) expresses the opinion that laterite is essentially a product of low levels, and can only occur in areas where drainage is practically at a standstill. This condition generally means the existence of a peneplane almost at sea level. Hence laterite upon a plateau, such as

that of the interior of Western Australia, is a criterion of elevation of the land; and difference of laterite levels suggests faulting. The actual deposition of laterite is believed to be effected by the leaching of subsoil salts by rainfall, followed by capillary action during the ensuing drought, with the result that the solution is drawn to the surface and the dissolved matter deposited as concretions within the soil.

The origin of ore deposits by laterisation is discussed by W. G. Miller (*Rept. Ont. Bur. Mines*, 1917, **26**, pt. 1, 318–34). He shows that iron (Cuba), nickel and cobalt (New Caledonia), especially, are concentrated by this means; and also that manganese, aluminium in the form of bauxite, and gold, may be formed as lateritic ores. The nickel ores of New Caledonia, although unquestionably of lateritic origin, do not conform to the accepted definition of laterite, inasmuch as they contain more than five per cent. of combined silica. Similarly it is shown that the term laterite can only be properly applied, on this criterion, to the upper layers of the Cuban iron ores, as the combined silica increases considerably with depth.

Davis, W. M., Metalliferous Laterite in New Caledonia, *Proc. Nat. Acad. Sci., U.S.A.* 1918, **4**, 275–80. (Discusses the relation of the laterite to the physiographic development of the island.)

Before his unfortunate death from wounds the late Lt. R. C. Burton had completed a paper on the origin of the laterite of Seoni, Central Provinces, India, which is now published by the Geological Survey of India (*Rec. G. S. India*, 1917, **48**, pt. 4, 204–18). This laterite is of the high-level type, and occurs at elevations between 1,900 and 2,100 feet above sea level. It is of the usual ferruginous cellular type, and directly overlies the Deccan Trap. It is believed to be partly of chemical origin and partly a detrital lake deposit.

CRYSTALLOGRAPHY. By Alexander Scott, M.A., D.Sc.

Crystal Structure.—Since this subject was last treated in these reports (July 1917) the structure of a number of crystals has been investigated by means of X-ray analysis, many of the results being obtained by a modified method which greatly extends the applicability of this type of investigation. This modification, which is due to P. Debye and P. Scherrer (*Nachr.*

K. Ges. Wiss. Göttingen, Math. Phys. Klasse, 1916, *Phys. Zeit.* **18,** 291, 1917 ; cf. *Engineering,* **104,** 17, 594, 1917), consists in allowing a narrow beam of X-rays to traverse an aggregate of small crystals, the resulting diffraction pattern being photographed. The disposition of the crystals is assumed to be thoroughly irregular so that at any instant a certain number of crystals are giving reflections from, say, a (100) face, others from a (111) face, and so on. The structure can then be elucidated by analysis of the diffraction pattern, due attention being paid to the intensities. This method, therefore, does away with the necessity of using well-developed crystals, as it is applicable to any crystalline aggregate provided the number of crystals is sufficiently great for some of them to be so orientated as to give suitable reflections. The authors have investigated the structures of graphite and amorphous carbon, and conclude that the latter is merely very finely divided graphite. The elementary parallelopiped in the case of graphite is a rhomboid, the carbon atoms being arranged so that they occupy the alternate corners of regular hexagons. It is suggested that graphite is the basis of the aromatic compounds and diamond of the aliphatic, while the apparent trivalency of carbon which occurs in such compounds as triphenylmethyl is ingeniously explained on the basis of the graphite structure.

In a further paper (*Phys. Zeit.* **18,** 481, 1917, *Jour. Chem. Soc.* **112,** ii. 574, 1917) the same authors give the structure of tungsten, which is found to be the centred cube, while Scherrer (*Phys. Zeit.* **19,** 23, 1918; *Jour. Chem. Soc.* **114,** ii. 113, 1918) ascribes the face-centred cube to the structural unit of aluminium. It is pointed out that although the edge of the elementary cube has the same dimension ($4{\cdot}07 \times 10^{-8}$ cm.) for aluminium and gold, these metals do not form a continuous series of solid solutions. By the same method, using material of diameter $0{\cdot}5 - 1{\cdot}0\,\mu$, S. Kyropoulos (*Zeit. anorg. Chem.* **99,** 197, 1917) has investigated the structure of quartz, cristobalite, and amorphous silica. The last shows no evidence of regular internal structure, but after calcination the interference bands characteristic of cristobalite appear. This method is also used by J. Olie and A. J. Byl (*Proc. Akad. Wetensch. Amsterdam,* **19,** 920, 1917) in an investigation of the structures of diamond and graphite. The mathematical basis of the method is dis-

cussed by C. Runge (*Phys. Zeit.* **18**, 509, 1917; *Sci. Abs.* **21A**, 20, 1918). A. W. Hull (*Phys. Rev.* **10**, 661, 1917) has devised, apparently independently, a similar method, and gives the results of investigations of several metallic elements and of silicon, graphite, and diamond. In the case of diamond the structure assigned is the same as that originally given by Bragg.

Several further investigations have also been made by Bragg's method. C. M. Williams has examined cassiterite and rutile, and has obtained results, in the latter case, which differ from those of Vegard (*Phil. Mag.* (6), **32**, 65, 1916). Vegard appears not to have observed a fairly intense reflection at 3° 52′ for the (100) face, and consequently the centred tetragonal lattice suggested by him for the titanium atoms does not hold, but, according to Williams, should be substituted by a more complex structure consisting of eight interpenetrating lattices of the simple tetragonal type. In the case of the basal plane the intensity of the second order reflections indicates the probability of the oxygen atoms lying in the planes parallel to this. The symmetry of this structure is holohedral tetragonal.

L. Vegard and H. Schjelderup (*Ann. Phys.* (iv.), **54**, 146, 1917; *Jour. Chem. Soc.* **114**, ii. 156, 1918) have examined crystals of normal ammonium and potassium alums, chrome alum and ferric ammonium alum, and from the reflections from the cube, octahedron, and rhombic dodecahedron have arrived at a structure for the metallic, sulphur, and oxygen atoms. The twenty-four molecules of water present are apparently all of the same type, and are symmetrically disposed with reference to the sulphur atoms. The crystalline structure is destroyed when the crystals are dehydrated. The effect of dehydration has also been examined in the case of chabazite ($CaAl_2Si_4O_{12}$, $6H_2O$), the X-ray structure being determined before and after dehydration. Although the relative intensities remain the same the absolute intensities diminish. Hence it is assumed that dehydration destroys the crystalline structure, the reflections obtained in the second case being ascribed to the presence of still undehydrated portions. The results would probably have been more definite if the mineral had been completely dehydrated.

The above structure for the alums is criticised by C. Schaefer and M. Schubert (*Ann. Phys.* (iv.) **55**, 397, 1918; *Jour. Chem. Soc.* **114**, ii. 315, 1918), who also claim that the theory regarding

the function of the water has already been advanced, on the results of the investigation of the absorption of light by the alums (*Ann. Phys.* (iv.) **50**, 339, 1916).

A somewhat different structure for the alums is put forward by P. Niggli (*Phys. Zeit.* **19**, 225, 1918), who has determined the point system from a knowledge of the symmetry and the distance between the successive planes in each series of parallel planes of reflection in the crystal. The oxygen atoms whose position is not determinate from the X-ray results can be located from the data mentioned above. The structure so obtained differs from that of Vegard and Schjelderup so far as the location of the oxygen atoms is concerned.

The structure of chalcopyrite ($CuFeS_2$) has been determined by C. L. Burdick and J. H. Ellis (*Jour. Amer. Chem. Soc.* **39**, 2517, 1917), who, by regarding the crystal as pseudocubic ($a : c = 1 : 0·985$), assign a face-centred lattice to the arrangement of iron and copper atoms, the planes perpendicular to the unique axis consisting alternately of copper and iron atoms. Taking the two types of atoms separately, each may be regarded as forming two interpenetrating simple tetragonal lattices. From the masses of the atoms and the distances between the successive planes, the density of the crystal is calculated and found to agree with the observed value.

In a series of papers the work of W. H. and W. L. Bragg is critically reviewed by E. S. Federov (*Bull. Acad. Sci. Petrograd*, **10**, 359, 435, 547, 1675, 1916). According to the available abstracts (*Chem. Abs.* **12**, 8, 1918, cf. T. V. Barker, *Ann. Rep. Prog. Chem.* **14**, 230, 1918) the first paper deals with the substances whose structure has already been determined by means of X-rays and gives appropriate structures for several substances, ammonium chloride, sodium chlorate, and cuprite, the investigation of which has hitherto been incomplete. It is found that the optical activity of sodium chlorate can only be explained on the assumption that the atom is asymmetric. The second paper enunciates the fundamental law of crystallochemistry that the atoms of a crystal are situated at the points of a single lattice, and hence the position of an atom with reference to any other atom as origin may be expressed in terms of co-ordinates which are either small whole numbers or simple fractions, if the distances from the origin to three atoms not complanar with it are taken as units. The appropriate equations for these

co-ordinates are given for a number of substances including calcite, dolomite, hæmatite, ilmenite, corundum, quartz, etc., and in some cases the positions of certain atoms are deduced from the already determined location of other atoms. The above law is analogous to that of Haüy, and the rationality of co-ordinates to that of indices.

The third paper deals with the effect of chemical similarity on the distance between the atoms and with the relations between the crystallographic unit and the chemical molecule. The space between two adjacent atoms of the same kind is greater than that between two dissimilar atoms, while in the case of the latter the distance between the atoms varies inversely as their chemical affinity. The rule has to be modified for certain cases in which bivalent atoms are concerned. The chemical molecule consists of one or more crystallographic units except in the case of the diamond, where the elementary parallelohedron includes two chemical molecules.

The fourth paper discusses the determination of the reticular density of atoms in the more important planes in a crystal. In calculating the reticular density of a possible crystal face the value taken is the maximum for all the planes parallel to it. A distinction is made between principal and fundamental parallelohedra, the former being those which can, by symmetry operations, be brought into coincidence with themselves or with congruent figures, while the latter are those which do not possess this property. Seven cubic substances whose structure has been determined are examined, and the fundamental parallelohedra and the disposition of the atoms with respect thereto determined.

From the theoretical point of view, the results of the X-ray analysis of crystals is discussed by Beckenkamp (*Centr. Min.* 1917, 97, 353, 393) and J. Stark (*Jahrb. Rad. Elektr.* **12**, 280, 1915), both of whom maintain that the individuality of the atom persists in the crystalline state, and by F. Rinne (*Zeit. anorg. Chem.* **96**, 317, 1916), who is of the same opinion, holding that the molecular structure is not necessarily derivable from the elementary cell of the lattice. From a consideration of the density and X-ray structure of the crystalline elements, C. W. Kanolt (*Science*, **47**, 123, 1917) proposes to calculate the atomic weights.

The question of the existence of molecules in the crystalline

state is discussed at considerable length by A. H. Compton (*Jour. Frank. Inst.* **185**, 745, 1918), who concludes, on the basis of the Dulong-Petit law and the results of X-ray analysis, that in the solid state the atoms are so intermingled that it is impossible to define particular molecules, each atom exerting an equal attraction on all neighbouring atoms. From a consideration of the relation between melting-point and "atomic heat of formation," it is stated that in crystals atoms are held together by forces analogous to those existing in chemical molecules.

In a theoretical paper A. C. Crehore (*Phys. Rev.* **10**, 432, 1916) derives mathematically the equilibrium conditions for some types of space lattices, certain assumptions being made regarding the type of motion and velocity of the electrons, and applies the results to a number of cubic crystals.

A partial elucidation of the structure of the garnet is given by S. Nishikawa (*Proc. Tokyo Math. Phys. Soc.* (2), **9**, 194, 1917), and of lead, barium, and strontium nitrates by the same author and K. Hudinuki (*ibid.* 197). Several substances have been examined in the same way by F. M. Jaeger and H. Haga (*Proc. Akad. Wetensch. Amsterdam*, **18**, 1350, 1357, 1916).

Mixed Crystals.—H. R. Kruyt has continued his investigation of the three-phase (solid, liquid, gas) equilibrium in those binary systems in which the two compounds form a continuous series of mixed crystals. In an earlier paper (*Proc. Akad. Wetensch. Amsterdam*, **12**, 537, 1909) the various curves were theoretically discussed, while later (*ibid.* **13**, 206, 1910) the system p-dichloro-benzene—p-dibromo-benzene was experimentally investigated. Recently (*ibid.* **19**, 439, 1916) the system iodine—bromine, which contains the compound IBr., probably miscible in all proportions with its elements, has been examined and a characteristic temperature-pressure curve for the co-existence of three phases has been found. In another paper (*ibid.* **19**, 555, 1917), however, it is shown that this type of curve does not necessarily depend on the existence of solid solution.

C. Viola (*Atti Real. Accad. Lincei* (5), **25**, ii. 285, 1916) has carried out a crystallographic investigation of the mixed crystals of magnesium and zinc sulphates, and concludes that these are composed alternately of each salt and therefore differ from solid solutions which are physical mixtures.

In another paper (*ibid.* (5), **26**, i. 195, 1917), by a mathematical treatment, the equilibrium conditions between mixed crystals and the amorphous phase are examined. The mean growths perpendicular to the crystal faces are found to vary as the capillary constants, and hence the general form is ultimately the mean of the forms of pure crystals of the substances forming the mixed crystals, due regard being paid to the proportions in which they are present. Curie's law regarding surface energy is stated to hold for mixed crystals. Isomorphism of crystals is held to depend upon the mean miscibility as well as similarity of structure and volume.

L. Vegard and H. Schjelderup (*Phys. Zeit.* **18**, 93, 1917, *Jour. Chem. Soc.* **112**, ii. 243, 1917), from an X-ray examination of mixed crystals of potassium chloride and bromide and of potassium and ammonium bromides, maintain that mixed crystals are not composed of alternate layers, but that the atoms are arranged in lattices similar to those of the components, the main difference being that in the first case some of those points which in the lattice of the simple salt KCl would be occupied by chlorine atoms, are now occupied, in the mixed crystal, by bromine atoms, with analogous results in the second case. The unit of the lattice has necessarily a different volume from that of the lattices of the components.

G. Tammann (*Nachr. K. Ges. Wiss. Göttingen*, 1916, 199, *Jour. Chem. Soc.* **112**, ii. 448, 1917) has investigated the effect, on binary mixed crystals, of reagents which attack one of the components, and has found that the action does not vary proportionately to the amount of attackable constituent. There are certain limits of composition between which the resistance to attack is great, and these limits have been determined, for a number of reagents, in the systems copper-gold and silver-gold.

Physical Properties.—P. W. Bridgman (*Amer. Jour. Sci.* (4) **45**, 243, 1918) has published the results of some interesting work on the elastic behaviour of crystals. Circular cylinders of various materials were pierced with cylindrical holes concentric with the outer surface, and the phenomena accompanying the failure of these cavities under high pressure was observed. The method of failure in such materials as quartz, felspar, tourmaline, and so forth, is totally different from that observed in the case of metals, as, instead of the

viscous flow observed in the latter, the walls of the cavity disintegrate with the projection of minute particles, the disintegration increasing as the pressure rises. Neither the mode of disintegration nor the arrangement of cracks which sometimes developed bore any apparent relation to the crystalline structure. Even when subjected to a pressure of 30,000 kg/cm^2, finely powdered quartz, felspar, or talc could not be welded together, and this is explained as due to the existence of thin films of air between adjacent grains, preventing sufficiently close contact. In a second paper (*ibid.* pp. 269–80) the stress-strain relations in crystalline cylinders is treated mathematically. Although a rigorous solution cannot be obtained, it is possible to get a first-order approximation for cubic and tetragonal crystals and a second-order approximation for trigonal crystals.

E. T. Wherry (*Jour. Washington Acad. Sci.* **8**, 277, 319, 1918) has attempted to find a relation between the refractive indices and the axial ratio in the case of tetragonal crystals. It is found that the ratio of the "refractions," as obtained from the Lorentz-Lorenz formula, by substitution of ω and ϵ, is equivalent to the crystallographic axial ratio. The agreement is very good for a number of organic substances, but fails in some cases, while for zircon and xenotime the ratio of the "refractions" is equal to the axial ratio multiplied by 3/2. From this, certain deductions concerning the spacing of the atoms are made.

W. H. McNairn (*Trans. Roy. Can. Inst.* **11**, 231, 1916, *Amer. Min.* **3**, 138, 1918) has investigated the origin and growth of etch figures on certain monoclinic minerals, such as spodumene, diopside, and colemanite. The location and distribution of the etching pits is not due to chance, but bears some relation to the molecular structure.

A. P. Honess (*Amer. Jour. Sci.* (4) **45**, 201, 1918) has made an interesting study of the etch figures of crystals belonging to the dihexagonal alternating class. The materials used were the rhombohedral carbonates, calcite being the subject of investigation. Different solvents gave different types of etch-pits so far as shape is concerned, while the tendency for the formation of pits varied in different crystal faces. For example, with hydrochloric acid, pits formed first on $(10\bar{1}1)$, then on $(10\bar{1}0)$ and $(11\bar{2}0)$, while they did not appear on the

scalenohedron ($21\bar{3}1$). Nitric acid gave similar results. With citric acid the order was ($10\bar{1}1$), ($11\bar{2}0$), ($21\bar{3}1$), and, after prolonged immersion, ($10\bar{1}0$). Citric acid was the only common acid to form pits on the scalenohedron, though the stronger acids attacked it, giving the surface a glazed appearance.

Crystallisation.—Several methods of determining the velocity of crystallisation have been used in recent work. J. Czochralski (*Zeit. phys. Chem.* **92,** 219, 1917) measures the velocity of crystallisation by the maximum rate at which a crystal thread can be drawn from a bath of the molten substance. Data for the metals lead, tin, and zinc are given. R. Nacken (*Centr. Min.* 1917, 191) has observed the growth of crystals of salol, from an undercooled melt, under the microscope, and has measured the rate of growth for different faces. A. Brann (*Jour. Amer. Chem. Soc.* **40,** 1168, 1918) has continued his studies on the effect of dissolved substances on the velocity of crystallisation of water. The effects observed are explained as being due to the influence of hydrates in solution. A further paper (*ibid.* **40,** 1184, 1918) treats of the effect of dissolved substances on the velocity of crystallisation of formamide.

H. N. Holmes (*Jour. Phys. Chem.* **21,** 709, 1917) has investigated the formation of crystals in gels, and has obtained crystals of metals such as copper and gold, as well as many salts, from solutions containing silicic acid. In order to verify the theory that some of the effects observed are due to capillary action through the " pores " of the gels certain substances in solution were allowed to diffuse through closely packed masses of finely divided material such as alundum, well-formed crystals being obtained.

Historical.—In the Hugo Müller lecture to the Chemical Society (*Jour. Chem. Soc.* **113,** 363, 1918), Sir H. Miers summarised recent work in crystallography and mineralogy, emphasising the contrast between the older work, which consisted mainly in direct crystal measurement and mineral description, and the present-day work, such as the X-ray analysis of crystals and the researches of the Geophysical Laboratory. The June number of *The American Mineralogist* (vol. iii. No. 6, 1918) is devoted to a description of the work of Haüy, and the results which proceeded directly and indirectly from it. Attention may also be drawn to two articles in the centenary number of the *American Journal of Science*, where W. E. Ford (*Amer.*

Jour. Sci. **46**, 240, 1918) discusses the mineralogical work of the past hundred years, and R. B. Sosman (*ibid.* 255, 1918) refers to the work of the Geophysical Laboratory.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., East London College, University, London.

Anatomy.—Dr. J. M. Thompson has studied the anatomy and affinity of ferns belonging to the genera *Jamesonia*, *Llavea*, and *Trismeria* (*Trans. Roy. Soc. Edin.* vol. lii. pt. ii.). All three genera exhibit a solenostelic structure, which is most advanced in its construction in *Trismeria*. The sporangial arrangement is "acrostichoid" and the number of spores small in *Trismeria* and large (over sixty-four) in *Jamesonia*. In the former genus the pinna traces are mostly extra-marginal, whilst in the other two genera their origin is marginal. The author concludes that the groups have an affinity, though this is not very close, and have been derived from a Schizeaceous source.

The phlœm of some fifty-five species of woody Dicotyledons has been examined in both the seedling and adult conditions by L. H. Macdaniels (*Am. Jour. Bot.* July 1918). From this study he concludes that there are no fundamental differences in the type of sieve-tube, in the mature phlœm and that of the seedling, except such as can be correlated with the smaller size of the elements in the latter. Moreover, the sieve-tube with single transverse plates (herbaceous type of Hemenway) is probably as frequent as other types in the arboreal forms studied. Even within the same genus *Fraxinus americana* has sieve-tubes with oblique end-walls and several plates, whilst the sieve-tubes of *F. nigra* have but a single plate, which is either transverse or slightly oblique. By contrast the Salicaceæ, Betulaceæ, and Juglandaceæ very constantly exhibit the oblique-ended type with numerous sieve-plates. Companion cells were found in every species studied, but appear to be more prevalent in the higher forms.

In the current number of the *Annals of Botany* (July) Prof. D. H. Campbell describes the structure of three species of *Dumortiera* and *Wiesnerella denudata*. In *D. velutina* there are outlines of air-chambers on the upper surface, and the assimilatory tissue is represented by numerous superficial pupillate cells. A new species, *D. calcicola*, is described which

possesses evident air-chambers, but less numerous papillæ than *D. velutina*. Its most striking feature is, however, the mode of occurrence of the male and female receptacles which are found on the same plant, and of which five or six are developed in succession on a series of terminal adventitious shoots. *Wiesnerella denudata* is regarded as intermediate in character between *Dumortiera* and *Marchantia*.

Cytology and Genetics.—C. H. Farr (*Am. Jour. Bot.* July 1918) describes the division of pollen mother-cells in *Magnolia tripetala*. The cell-wall, it appears, is formed not by equatorial thickening of the spindle-fibres, but by a constricting furrow in which the cell-wall substance would seem to be deposited.

The significance of the chondriome forms the subject of a paper by M. Guilliermond in the *Revue générale de Botanique* for June.

The *Journal of Genetics* for June contains a paper by Miss Saunders dealing with two forms of the common foxglove. Of these the common type exhibits a hairy stem (v. *pubescens*), but there is also a smooth-stemmed variety which the author terms *nudicaulis*. Both the varieties breed true to their respective characters, the smooth behaving as a dominant and the pubescent as a recessive.

The mutations of *Œnothera suaveolens* are described by De Fries (*Genetics*, 1918). Six of these have occurred in the cultures, three of which are represented by parallel mutations in other species. Of the remainder two were narrow-leaved types, whilst the third was frequently apetalous, though exhibiting all conditions up to that with a perfect corolla.

In the same journal Tupper and Bartlett deal with the relation of mutational characters to cell size *à propos* of the fact that the cells of a giant mutant of *Œnothera pratincola* were found to be larger than those of the parent. The pertinent observations are reviewed. A doubling in the number of chromosomes, accompanied by a marked increase in the size of the cells, has been observed in *Amblystegium serpens bivalens* and other mosses belonging to the genera *Mnium* and *Bryum*; in *Œnothera* mutants, of the species *Œ. Lamarckiana* and *Œ. stenomeres*; and in one of the "graft hybrids" of *Solanum lycopersicum* and *Solanum nigrum*. On the other hand, the tetraploid variety of *Phascum cuspidatum* was a dwarf with cells of the normal size, and the giant-celled mutants of

Primula sinensis were unrelated to any augmentation of the chromosome number. R. Hume has studied the chromosome number in *Œnothera scintillans*, and finds that in three successive generations the chromosome number varied from the typical fifteen to as many as twenty-one. The variation is attributed to fragmentation of one or more chromosomes, chiefly on the grounds that whatever the number the sum of their lengths remains constant.

The inheritance of doubleness in the flowers of *Chelidonium majus* forms the subject of a communication by Karl Sax (*Genetics*, pp. 207–307). The character is found to be a simple recessive. There is a high degree of negative correlation between petal number and stamen number, indicating that the double condition is the outcome of petalody of the stamens. The number of the latter ranged from 6–31, most examples showing 16, 14, 24, or 12, whilst the number of the petals varied between 3 and 27, the most frequent conditions being 4 and 12.

Taxonomy.—A new British fresh-water alga is described by W. J. Hodgetts (*New Phytologist*, July). It is an epiphytic member of the Ulotrichaceæ and bears the name *Uronema elongatum*.

New species of Fungi belonging to the genera *Mycosphærella*, *Phyllostica*, and *Cytospora* are described by W. B. Grove (*Jour. Bot.*). Dr. Ellis, under the title "Phycomycetous Fungi from the English Lower Coal Measures," describes *Palæomyces bacilloides n. sp.* and *Palæomyces gracilis*, the latter being referred to the fossil genus *Peronosporites* (*Proc. Roy. Soc. Edin.* vol. xxxviii. p. 130). A number of additional species of Compositæ and other families are contributed by S. F. Blake (*Contrib. Gray. Herb.*) and Francis (*ibid.*) describes new species of *Tricyrtis*, *Atriplex*, *Lotus*, *Lomatium*, and *Cirsium*. Fernald (*Rhodora*) describes a new species of *Litorella* and critical forms of *Epilobium*.

In the *Journal of Botany* for August, Spencer Moore describes new species of Compositæ and O. Paulson an addition to the genus *Cereus*.

In a paper dealing with the genus *Caltha* in the Southern Hemisphere, A. W. Hill describes three new species (*Ann. Bot.*). American willows related to *Salix artica*, with three new varieties, are the subject of a paper by A. C. Schneider (*Bot.*

Gaz. July), and in the same journal M. C. Bliss deals with the interrelationships of the Taxineæ.

P. Vuillemin (*Comptes Rendus*) treats of the interrelationships of the *Incompleteæ* (Amentales), which are held to be primitive, and their connection with the higher plants. Both the Hydnoraceæ and Nymphæaceæ are regarded as related to the Piperaceæ.

Ecology.—Prof. Herdman has studied the plankton diatoms of the Irish Sea (*Jour. Linn. Soc.* No. 297), which consist mainly of members of the genera *Biddulphia, Chætoceras, Coscinodiscus, Rhizosolenia, Thalassiosira, Guinardia,* and *Lauderia.* The first named is represented by two species, viz. *B. mobiliensis* and *B. sinensis,* which appear from September to June, with a maximum development in March or April, and a secondary maximum in November. *Coscinodiscus* is represented by several species, which likewise attain their greatest abundance in March or April. *Chætoceras,* represented by a number of species, exhibits two maxima, viz. a primary in April and a secondary in September. *Lauderia borealis* is most abundant at the end of April, and the species of *Thalassiosira* from then onwards till the end of May. Both *Rhizosolenia spp.* and *Guinardia flaccida* are late diatoms with their maximum in June.

J. B. Pole-Evans, in *The Official Year Book for* 1917, gives an excellent little summary of South African vegetation. This is accompanied by a map showing the geographical distribution of the chief types of plant communities and beautifully illustrated with some forty-eight half-tone plates.

Pedigree cultures of oats have been grown by J. O. Dickson in quartz sand, with culture solutions having identical osmotic concentrations, but deficient in either magnesium, calcium, potassium, phosphorus, or nitrogen (*Am. Jour. Bot.* June). The general development was most adversely affected by deficiency in phosphorus or nitrogen, but the absence of magnesium or calcium increased the vigour of growth, though diminishing the production of grain. The water supply necessary for maximum growth decreases when there is a deficiency of magnesium, whilst it is increased by a deficiency in calcium, and still more when there is an inadequate supply of potassium, phosphorus, or nitrogen.

Two papers of considerable interest, both to the ecologist

and anatomist, are contributed by Prof. Farmer to the *Proc. Roy. Soc.* vol. xc. p. 218). The author has estimated the conductivity of the wood in trees and shrubs by the volume transmitted during fifteen minutes through a length of 15 cms., with a pressure of 30 cms. of mercury. The conductivity per unit area of evergreens (specific conductivity) was found to be relatively low with slight variation, whilst that of deciduous trees is high with marked fluctuation. Thus *Euonymus japonicus* gave an average of 12, with a range of ± 6, whilst its deciduous congener the spindle-tree gave 47 ± 8. Young or immature wood always gave a low conductivity, and the low conducting power of "leaders" as compared with lateral shoots is suggested as a factor inducing sympodial growth. The effect of manuring on fruit trees is to increase the conducting capacity, and root-pruning appears to induce a decrease.

Adamson (*Jour. Ecology*) describes some associations of the Southern Pennines. The cotton-grass moors, characteristic of deep ill-drained peat, are regarded as the outcome of post-glacial conditions, and to be now, generally, in a decadent condition. This latter has brought about a new succession of phases, dominated respectively by *Deschampsia flexuosa*, *Nardus stricta*, *Vaccinium myrtillus*, and *Calluna vulgaris*, exhibiting progressive increase of peat.

In the *Transactions of the New Zealand Institute*, vol. l., Prof. Wall considers the interesting problem of the distribution of the closely allied *Senecio lagopus* and *S. saxifragoides*, which only differ in the relative abundance and distribution of their hairy covering. Despite their close affinity each, in its own rather wide but well-defined area, retains its individuality, *S. lagopus* being found over the main mass of Banks Peninsula, whilst *S. saxifragoides* is absent from this area, but occurs, unaccompanied by its congener, upon the Port Hills, where the peninsula abuts upon the mainland. It is suggested that, since the Akaroa peaks, on which the less hairy *S. lagopus* flourishes, are moister and subject to a higher rainfall than the Port Hills, the two plants may be in the nature of climatic microspecies.

Our scanty knowledge of the ecology of Cryptogams has been enriched by an account of the cryptogamic vegetation of the sand-dunes of the West Coast, particularly Devon and

Somerset, by W. Watson (*Jour. of Ecology*). Conspicuous amongst the lower plants of the psamma dunes are the mosses *Tortula ruraliformis*, *Camptothecium lutescens*, *Brachythecium albicans*, and the lichens *Cladonia furcata*, *Peltigera canina*, and *P. rufescens*. Greater fixity is, as with the phanerogamic vegetation, marked by an augmented flora, with an increase of *Cladonia*, and the presence of abundant *Bryum spp.* and *Barbula spp.* In the fixed stages *Hypnum cupressiforme*, *Brachythecium rutabulum*, and *Hylocomium squarrosum* become prominent features. *Riccia crystallina* is a feature of the dune "slacks," whilst the dune marsh proper exhibits *Hypnum aduncum* and other species of the section *Harpidia* as the cryptogamic dominants.

PLANT PHYSIOLOGY. By WALTER STILES, M.A., University, Leeds, and University College, London. (Plant Physiology Committee.)

Irritability.—Very considerable interest has been displayed recently in regard to irritability phenomena in plants, and perhaps in no branch of plant physiology is such rapid development taking place, especially on the Continent and in America.

A number of important contributions to this subject deal with geotropism. Bannert (*Beitr. z. allg. Bot.* **1**, 1–44, 1916) investigated the curving of the flower stalk and influorescence axis in a number of different species. When the plants were rotated on a klinostat with horizontal axis no curvature takes place, from which it is concluded that the curvature is related to a one-sided gravitational action. *Pelargonium* formed an exception to the rule, for in this genus the influorescence axes curved when rotated on the klinostat with horizontal axis. Demole (*Bull. de la soc. bot. de Genève*, (2) **8**, 277–81, 1916), investigating the curvature of the developing fronds of bracken, came to the conclusion that the change in direction of curvature is due to a change from positive to negative geotropism of the incurved region of the frond. This curvature was found to take place only if the plants were growing in the light, and is hindered by contact excitations. Demole investigated the effect of these last, by stretching silk threads above the fronds. When the fronds reached the threads the tactile excitations so produced hindered the curvature change, but on removal of the source of tactile excitation the change in curvature took place in twenty-four hours. Provided the plant had been in

the light previously, the change in curvature took place equally in the light or in the dark. This is held to show that the phototonus of the frond necessary for the geotropic reaction can be obtained independently of the geotropic reaction itself. An investigation of the reaction to stimuli of the hypocotyls of young mistletoe seedlings has been made by Heinricher (*Ber. deut. bot. Ges.* **34**, 818–30, 1916; *Jahrb. f. wiss. Bot.* **57**, 321–62, 1917), who finds that for three or four weeks after germination the hypocotyls are negatively phototropic, and for about two succeeding weeks negatively geotropic, but that the latter does not become evident to any extent if the attachment of the plant to the substratum takes place during the period in which the hypocotyls are negatively phototropic. Van Ameijden (*Diss. Utrecht*, 1917; *Versl. Verg. Kon. Akad. van Wetensch. Amsterdam*, **25**, 1135–43, 1917), working with oat seedlings, shows that oxygen is necessary for the perception of the geotropic and also of the phototropic stimulus.

In a series of investigations on regeneration in *Bryophyllum*, Loeb (*Science*, **44**, 210, 1916; **46**, 115–18, 1917; *Bot. Gaz.* **63**, 25–49, 1917) has shown that on removal of all the leaves from a stem except one apical one, the rate of geotropic curvature when the stem is placed horizontally is comparatively rapid, and is proportional to the mass of the leaf. If a single leaf is left attached, but towards the base instead of at the apical end, the curvature is less and is confined to the part of the stem near the attached leaf. Root production runs parallel with geotropic curvature. These results lead the author to the conclusion that from each leaf root-producing substances pass out towards the base of the stem, and shoot-forming substances pass out towards the apex. The root-forming substances produced in the leaf are absorbed in the stem and bring about the growth of the cortical cells to which the geotropic curvature is due. It seems likely that in *Bryophyllum* the same substance (hormone) may control both geotropic curvature and root production.

In three papers read to the British Association for the Advancement of Science in 1915 and 1916 (*British Ass. Rep.*, **85**, 722, 1916; **86**, 511, 1917) Miss Prankerd adduced evidence in favour of the statolith theory of the organs of perception of gravity. A statolith is defined as an organ free to move in the cell containing it; in many cases statoliths are starch grains.

The evidence is derived from a microscopic examination of the distribution of these starch grains in a number of species. A somewhat detailed examination was made of the reproductive organs of liverworts in which a good development of statolith starch grains was found to correspond with power of response to the gravitational stimulus. Reference has already been made to Demole's work on the negative geotropism of bracken fronds. Miss Prankerd finds that in ferns a statolith apparatus develops at the physical apex of the fronds. On the contrary in organs where curvature is impossible or of no advantage to the plant, statoliths could not be detected. In some cases chloroplasts function as statoliths, as for example in *Ophioglossum*, *Lunaria*, and *Myriophyllum*.

That geotropic stimulation affects the viscosity of the protoplasm is a view put forward by G. and F. Weber (*Jahrb. f. wiss. Bot.* **57**, 129–88, 1916), who based their conclusion on observations on the rate of fall of starch grains in stimulated cells of the endodermis of the epicotyl of bean seedlings (*Phaseolus multiflorus*) as observed in longitudinal (radial and tangential) sections. This conclusion is destructively criticised by Clara Zollikofer (*Ber. deut. bot. Ges.* **35**, 291–8, 1917), who holds that G. and F. Weber neglected the large experimental error in their crude method, so that the results obtained by them are meaningless.

Small (*Ann. of Bot.* **31**, 313–14, 1917) upholds the conclusion that the Weber-Fechner law holds for perception of geotropic stimulus by plants, a suggestion put forward by Darwin and supported by Fitting. It is further proposed by Small that the perception of geotropic stimulation is correlated with permeability, since changes in the electrical conductance of the stimulated tissue, which are assumed to be a measure of the alterations in permeability, also increase with the intensity of the stimulus according to the Weber-Fechner law. But when we consider that it has yet to be proved that electrical conductivity is a measure of permeability, and that indeed permeability is a term used in a variety of different senses, and that Small has not defined what he means by it, although he uses it in a quantitative sense, it becomes clear that Small's hypothesis that geotropic stimulation is connected with permeability changes rests on no very firm foundation. A remark of Verworn's is apposite in this connection: " Un-

fortunately the innumerable investigations in this field have shown more and more clearly that it is not possible to formulate a general mathematical law which strictly fixes the relations of the intensity of the stimulus and the intensity of the response" (*Irritability*, New Haven, 1913, p. 48).

The problems of contact stimulation and stimulation by wounding have been attacked by Stark. In regard to the former it is shown (*Jahrb. f. wiss. Bot.* **57**, 189–320, 1916; *Ber. deut. bot. Ges.* **35**, 266–91, 1917) that haptotropism, the contact irritability manifested by climbing plants, is widely distributed although slightly developed throughout the plant kingdom, being most evident among non-climbers, in Gramineæ, Cruciferæ, and Caryophyllaceæ. With the exception of the side roots of *Phaseolus*, haptotropism is apparently non-existent in roots. It is in the principal growing region that curvatures begin, and it is in this region that they are most readily induced. The reaction time and the magnitude of the curvature depend upon the species, the age of the organ, and the strength of the stimulus. Continued stimulation results in over-excitation with the consequent failure of curvature. In some seedlings (*e.g.* sunflower, flax, oat) curvatures may appear as a result of wilting if the air is rather dry and if the surface is rubbed with a rather rough surface. This curvature as a result of wilting is simply due to removal of the wax from the cuticle which renders it more permeable to water. If, however, the seedlings are stroked with a smooth surface and in a damp chamber, a curvature due to contact results which is often accompanied by greatly increased growth.

Stimulation due to wounding was effected in various ways by Stark (*Ber. deut. bot. Ges.* **34**, 492–508, 1916; *Jahrb. f. wiss. Bot.* **57**, 461–552, 1917), as for example by incisions, by burning, or by corrosion with caustic substances such as silver nitrate. In all cases the axis curves towards the wounded side, and the reaction occurs in the dark as well as in the light, and under water as well as in air. In a variety of instances it was shown that the reaction region and perception region are different. For example, in Gramineæ if the coleoptile is stimulated by local cauterisation with silver nitrate the reaction occurs and is confined to the apex of the hypocotyl. On wounding an organ narcotised by ether no reaction takes place, but narcosis after stimulation by wounding does not prevent the reaction,

from which it is concluded that ether narcosis only suppresses the power of perception of the stimulus but does not affect the power of reaction. Like contact stimulation, stimulation by wounding often results in more rapid growth, but if the stimulation is too strong cessation of growth may result.

An interesting suggestion is made by Ricca (*Nuovo giorn. bot. ital.*, **23**, 51–171, 1916) as to the mode of propagation of the stimulus in *Mimosa*. After showing by ringing experiments that the stimulus is propagated in the wood, it is further shown that the stimulus is propagated through a damaged zone, or even if the continuity of the tissues is completely interrupted and two parts of a branch joined by a tube of water. This suggests that the stimulus is propagated by means of a substance formed in the protoplasm which is carried through the plant in the water channels by the water current, and which acts as a stimulus when it reaches the pulvini. Further evidence in support of this theory was obtained by cutting sections of the stem and allowing the contents to diffuse out into a small quantity of water, which on absorption by cut branches of the stem, produced the characteristic stimulation. As in Loeb's work with geotropism in *Bryophyllum* cited earlier, a comparison with hormones is suggested.

Some observations of Bose and Das (*Proc. Roy. Soc. London*, B, **89**, 213–31, 1916) indicate that under different conditions the excitability and conductivity in the case of stimulation of *Mimosa* vary. Fatigue may be induced in the same way as in animals.

Leaf movement in the case of insectivorous plants has been investigated by W. H. Brown in the case of *Dionæa*, and by H. D. Hooker in *Drosera*. In the former Brown (*Amer. Journ. Bot.* **3**, 68–90, 1916) concludes that closure of the leaves is brought about by increase in size of the ventral cells owing to stretching of the cell walls. This increase in size is permanent, and opening of the leaf results from a similar growth of the dorsal cells of the leaf. The enlargement of the ventral cells the author explains on the ground of their increased osmotic concentration following on stimulation. In *Drosera* Hooker (*Bull. Torrey Bot. Club*, **43**, 1–27, 1916; **44**, 389–404, 1917) found that during bending of the leaves the osmotic pressure decreases in the cells on the concave side, but remains unaltered on the convex side, and the author concludes that this decrease

is a consequence of, and not a condition for, the increase in size of the cells. This last is considered to result from decreased elasticity of the cell wall. Subsequent growth renders permanent the change in cell size and shape.

A number of investigations have been made recently on leaf movements. Brenner (*Svensk Bot. Tidssk.*, **10**, 374–410, 1916) has been able to show that leaves of *Oxalis* perform two different kinds of movements, one due to change in light intensity at dusk ("sleep" movements), and so-called autonomous movements. These movements are influenced by temperature as well as by light, but variations in humidity are without influence. Leaf movements in Beech and Sycamore have been examined by Lundegårdh (*Svensk Bot. Tidssk.*, **10**, 438–70, 1916). At the beginning of development the leaves are not phototropic, and very little geotropic, but later in their development the geotropism increases and this is found the condition for the horizontal position. Two kinds of movements have also been observed by F. C. Gates in leguminous leaflets (*The Plant World*, **19**, 42–5, 1916; *Bot. Gaz.* **61**, 399–407, 1916), a movement being observed in continuous sunlight in addition to the nyctitropic movement. The former the author terms a "xerofotic" movement, on account of his attributing it to differential water loss and consequent differential turgidity of the two sides of a leaf exposed to insolation. The author was able to induce the movement by application of local drying agents. The movements of the leaves of a leguminous plant (*Phaseolus multiflorus*) form the subject of a very suggestive communication by Rose Stoppel (*Zeitsch. f. Bot.* **8**, 609–84, 1916), who was unable to find any relation between the movements and a number of external conditions, such as temperature, gravity, humidity, but she finds a correspondence between the movements and electrical conditions in the atmosphere, and therefore concludes that change in the atmospheric electrical conditions is the stimulus bringing about the diurnal movement.

A considerable amount of interesting work has been done on irritability of lower plants. In regard to Algæ Buder (*Jahrb. f. wiss. Bot.* **58**, 105–220, 1917), working with a number of unicellular organisms, has investigated phototactic movements. Perhaps his most important conclusion is summed up in the "law of the resultant," which states that with two beams of light the resulting direction of action depends on the

intensity and direction of these beams, the direction of action being given by the resultant of a "parallelogram of forces," in which the direction and length of the sides represent the direction and intensity of the beams at their place of action. The same law can be applied to the case of a number of beams acting in different directions.

In the case of the blue-green algæ Nienburg (*Zeitsch. f. Bot.* **8**, 161–93, 1916) shows that the whole thread is equally sensitive for the perception of light stimulus. The response to the stimulus takes the form of alteration in velocity, the movement slowing down with small light intensities and increasing with high intensities. A sudden change from light to darkness brings about reversion of the direction of movement, but the reverse change has no effect on the direction of movement. It has been shown by Harder (*Zeitsch. f. Bot.* **10**, 177–244, 1918) that stimulation of the Nostocaceæ by mechanical means such as pressure against an obstacle, or by sudden transference from light to dark, also results in a reversal of the direction of movement.

In conclusion mention may be made of some recent work by Miss Parr (*Ann. of Bot.* **32**, 117–205, 1918) on the response of a fungus, *Pilobolus*, to light, in which a curvature of the sporangiophores results from light stimulation. The response takes place to rays of all parts of the visible spectrum, but has a marked relation to wave frequency. The presentation time increases gradually from the violet to the red end of the spectrum, an observation confirming Brefeld's earlier results. The presentation time multiplied by the square root of the frequency gives a product which is nearly constant, but which decreases slightly and gradually with increase in frequency. The presentation time is related to the spectral energy approximately in accordance with the Weber-Fechner rule, "if the wave frequencies are made a function of the constant."

ZOOLOGY. By Prof. Chas. H. O'Donoghue, D.Sc., F.Z.S., University of Manitoba, Winnipeg, Canada.

Invertebrata.—Bourne has published an interesting paper "On Some New Philliinæ from New Guinea" (*Quart. Jour. Micro. Sci.* vol. lxiii, 63, April 1918). The scope of the paper, however, is hardly indicated by its title, for, in addition to providing

a description of new members of the genus Philliinæ, it also includes a useful discussion of their relationships with other Actiniiæ, in which a decidedly novel method of dealing with the question of the systematic position of animals is put forward. It is suggested that certain characters in these forms, *e.g.* the presence or absence of a mesoglœal sphincter or of acontia, etc., should be regarded as "unit characters," and that they "have all the properties of, and may legitimately be identified with, Mendelian units." Having made this assumption the author has ". . . endeavoured . . . to arrange certain limited morphological and systematic data in conformity with the conclusions reached by the Mendelian school of zoologists, and indicated a method that seems to me appropriate to the purpose. In so doing I am aware that I am proposing a revolution in our methods of envisaging and dealing with morphological and systematic problems." Whether the author's premises will be accepted or his method widely adopted the future must decide, but they are obviously points that need to be considered by the systematist.

Other papers include: Hargitt, "Germ Cells of Cœlenterates. 5. *Eudendrium ramosum*" (*Jour. Morph.* vol. xxxi, June 1918); Walton, "Longitudinal Fission in *Actinia bermudensis* Verrill" (*ibid.*).

Essenberg has discussed "The Factors controlling the Distribution of the Polynoidæ of the Pacific Coast of North America" (*Univ. Cal. Pub. Zool.* March 1918). In this a review is given of the known facts concerning the distribution of the fifty-three species of Polynoidæ occurring on this area: some are cosmopolitan, but others restricted to the Pacific Coast and even to small areas of it. The most important determining factors were found to be temperature, currents, winds, chemical composition of the water, food, habits and mode of life, and the plasticity of the animal itself. The greatest number of Polynoids are to be found in the Littoral zone. In view of this and the fact that they are more uniform there than at greater depths, the author suggests that the Littoral zone is at any rate the centre of dispersal and perhaps the centre of origin of the group.

Other papers include: Kepner and Rich, "Reactions of the Proboscis of *Planaria albissima* Vejdovsky" (*Jour. Exp. Zool.* vol. xxvi. May 1918).

Nelson has carried out investigations "On the Origin, Nature, and Function of the Crystalline Style of Lamellibranchs" (*Jour. Morph.* vol. xxxi. June 1918) on material from six marine and fresh-water species. The style develops from the posterior region of the stomach. By its rapid rotation it aids the digestive processes in the stomach very materially, and in it is stored a mass of amylolytic ferment for utilisation at the feeding periods. Undigested food-remains that escape from the stomach are returned by ciliary currents in the intestine through the core of the style.

Other papers include: Packard, "A Quantitative Analysis of the Molluscan Fauna of San Francisco Bay" (*Univ. Cal. Pub. Zool.* April 1918); Whittaker, "The Relationship of the Marl Fauna of Mackay Lake, Ottawa, to the Present Molluscan Fauna of the Lake" (*Ott. Nat.* April 1918).

It has always been tacitly assumed that the larval insect possessed olfactory organs, but until the publication of the paper by McIndoo on "The Olfactory Organs of a Coleopterous Larva" (*Jour. Morph.* vol. xxxi. June 1918) they had never been described. The author describes them as being similar to those in the adult, and distributed to the number of about 1,359 on the antennæ, mouth-parts, thorax, and legs.

Other papers include: Lim, "Period of Survival of the Shore-Crab (*Carcinas mœnas*) in Distilled Water" (*Pro. Roy. Soc. Edin.* 1918); Hay and Shore, "The Decapod Crustaceans of Beaufort, N.C., and the Surrounding District" (*Bull. Bureau Fish. Washington*, April 1918); McLean, "Revision of Some Phacopid Genera" (*Ott. Nat.* April 1918); Sanford, "Experiments on the Physiology of Digestion in the Blattidæ" (*Jour. Exp. Zool.* vol. xxv. April 1918).

Vertebrata.—The "Differentials in Behaviour of the Two Generations of *Salpa democratica*, Relative to the Temperature of the Sea" (*Univ. Cal. Press*, March 1918), by Michael, throws light on the relation of this species to its environment. The main conclusions arrived at are that the differences in distribution between solitary and aggregate forms are as definite as the differences of structure, and the former differences cannot be explained by the seasonal succession of the two generations. Hydrographic changes or variations in viscosity also cannot account for the differences. Finally the author concludes that "Contrary to the prevailing plankton concept, *Salpa*

democratica, a typical plankton organism, controls to a significant extent its own distribution, just as certainly as does any fish or other animal commonly included under the term nekton." Goodrich set out to investigate the question of the segmentation of the head of *Scyllium* in the light of our present knowledge of other forms, and has set forth his results in a paper entitled "On the Development of the Segments of the Head in *Scyllium*" (*Quart. Jour. Micro. Sci.* vol. lxiii, April 1918). Three pro-otic segments were identified corresponding to the profundus, trigeminal, and facial nerves, and whose muscles are supplied by the oculomotor, trochlear, and abducens nerves. The post-otic region of the head comprises 4 somites, *i.e.* 4–7, of which 4 is related to the glossopharyngeal and 5, 6, and 7 to the first three branchial branches of the vagus. "The eighth somite belongs to the first spinal nerve, of which the dorsal root is absent or vestigeal in later stages, and to the vagus. The third and fourth segments, and perhaps also the first and second, contribute to the mesenchymatous sheet below the hind brain in which the parachordal cartilages develop."

Other papers include: Kendall, "The Rangely Lakes, Maine, with Special Reference to the Habits of the Fishes, Fish Culture, and Angling" (*Bull. Bureau Fish. Washington*, May 1918); Crozier, "Tactile Reactions of the De-eyed Hamlet" (*Jour. Comp. Neur.* vol. xxix. April 1918); Johnson, "The Peripheral Terminations of the Nervus Lateralis in *Squalus sucklii*" (*ibid.* June 1918); Norris, "The Morphogenesis of the Thyroid Gland in *Squalus acanthias*" (*Jour. Morph.* vol. xxxi. June 1918); Gilchrist, "Notes on Eggs and Embryos of the South African Myxinoid, *Bdellostoma* (*Heptatretus*) *hexatrema*, Mull" (*Quart. Jour. Micro. Sci.* vol. lxiii. April 1918); Lambe, "On the Remains of a Selachian from the Edmonton Cretaceous of Alberta" (*Ott. Nat.* May 1918); Detwiler, "Experiments on the Development of the Shoulder-Girdle and the Anterior Limb of *Amblystoma punctatum*" (*Jour. Exp. Zool.* vol. xxv. April 1918); Harrison, "Experiments on the Development of the Fore-limb of *Amblystoma*, a Self-differentiating Equipotential System" (*ibid.*); Johnson, "On the Question of Commissural Neurones in the Sympathetic Ganglia" (*Jour. Comp. Neur.* vol. xxix. June 1918); Hooker, "The Effect of Reversal of a Portion of the Spinal Cord at the Stage of Closed Neural Folds on the Healing of Cord Wounds, on the Polarity of the Elements

of the Cord, and on the Behaviour of Frog Embryos" (*ibid.*); Kude, "On the Development of the Nerve End-organs in the Ear of *Trigonocephalus japonicus*" (*ibid.*); Johnson, "The Branchial Derivatives of the Pied-billed Grebe, with Special Consideration of the Origin of the Postbranchial Body" (*Jour. Morph.* vol. xxxi. June 1918); Riddle, "Further Observations on the Relative Size and Form of the Right and Left Testes of Pigeons in Health and Disease, and as influenced by Hybridity" (*Anat. Rec.* vol. xiv. May 1918).

It is pointed out by Hill, "Some Observations on the Early Development of *Didelphys aurita* (Contributions to the Embryology of the Marsupialia, V.)" (*Quart. Jour. Micro. Sci.* vol. lxiii. April 1918), that in the only two Marsupials whose embryology has been fairly fully investigated, namely *Dasyurus* and *Didelphys*, development takes place according to a common plan. Moreover, as far as is known, the development in *Macropus* and *Perameles* is also essentially similar. These facts justify the conclusion that a common mode of development characterises the Didelphia as a group. The outstanding feature of this appears to be the production of a unilaminar blastocyst whose wall is divisible into two polar areas, one formative and the other non-formative. The former gives rise to the embryonal ectoderm and the entire entoderm of the blastocyst, while the latter provides the extra-embryonal ectoderm. Two new terms descriptive of the condition of the blastocyst are proposed: Phanerotypy, to designate the condition in the Ornithodelphia and Didelphia in which "the formative cells are freely exposed and constituted from the first part of the blastocyst wall, just as those of the Sauropsida form part of the general blastoderm"; and Cryptotypy, to designate the condition in the Monodelphia where "the formative cells are completely hidden or enclosed by the tropho-ectodermal mantle." The unsegmented ovum in *Didelphys* is potentially polar in constitution since the first cleavage results in the formation of the parent cells of the formative and non-formative regions respectively. In "The Formation, Rupture, and Closure of Ovarian Follicles in Ferrets and Ferret-Polecat Hybrids, and Some Associated Phenomena" (*Trans. Roy. Soc. Edin.* 1918) Robinson has recorded observations made on a large number of animals whose ovaries were in all stages of activity and also in the ancestrous condition. In the ferret we have an animal that only ovulates

after successful insemination, a fact which eliminates certain of the difficulties encountered in a number of other species. The author holds that the final maturation of a group of follicles is induced by the material produced in preceding groups of follicles which do not attain maturity. Upon reaching maturity it is suggested that the follicles produce a substance that is the cause of the phenomena of pro-œstrum and œstrus. There is practically no loss of follicular cells upon rupture, and they become transformed into luteal cells. Rupture is brought about by the formation of a secondary liquor folliculi which is produced subsequent to successful insemination.

Other papers include: Camp, "Excavations of Burrows of the Rodent *Aplodentia*, with Observations on the Habits of the Animal" (*Univ. Cal. Pub. Zool.* May 1918); Taylor, "Revision of the Rodent Genus *Aplodentia*" (*ibid.* June 1918); Sugita, "Comparative Studies on the Growth of the Cerebral Cortex. Parts 5, 6, 7, 8" (*Jour. Comp. Neur.* vol. xxix. April and June 1918); Stockard and Papanicolaou, "Further Studies on the Modification of the Germ-Cells in Mammals; The Effect of Alcohol on Treated Guinea-Pigs and their Descendants" (*Jour. Exp. Zool.* vol. xxvi. May 1918); Papanicolaou and Stockard, "The Development of the Idiosome in the Male Germ-Cells of the Guinea-Pig" (*Amer. Jour. Anat.* vol. xxiv. May 1918); Hatai, "On the Weight of the Epididymis, Pancreas, Stomach, and of the Submaxillary Glands of the Albino Rat (*Mus norvegicus albinus*) according to Body Weight" (*ibid.*); Meyer, "The Absence of Hemal Nodes in the Domestic Pig," and "Some Observations on Megacytes in Lymphatic Tissues" (both *ibid.*); Hays, "The Influence of Excessive Sexual Activity of Male Rabbits on the Nature of their Offspring" (*Jour. Exp. Zool.* vol. xxv. April 1918); Lloyd-Jones, Orren, and Hays, "The Influence of Excessive Sexual Activity of Male Rabbits. I. On the Nature of the Seminal Discharge" (*ibid.*); Stewart, "Changes in the Relative Weights of the Various Parts, Systems, and Organs of Young Albino Rats Underfed for Various Periods" (*ibid.*); Hatai, "On Several Effects of Feeding Small Quantities of Sudan III to Young Albino Rats" (*ibid.* May); King, "Studies on Inbreeding. I. The Effects of Inbreeding on the Growth and Variability in the Body Weight of the Albino Rat" (*ibid.*); Whiting and King, "Ruby-eyed Dilute Grey, a Third Allelo-

morph in the Albino Series of the Rat " (*ibid.*) ; Harman, " A Probable Cause of Superfetation in the Cow " (*Anat. Rec.* vol. xiv. May 1918) ; Huber, " On the Anlage and Morphogenesis of the Chorda Dorsalis in Mammalia, in particular in the Guinea-Pig (*Cavia cobaya*) " (*ibid.*) ; and Allen, " Studies on Cell Division in the Albino Rat (*Mus norvegicus,* var. *alb.*). III. Spermatogenesis ; The Origin of the First Spermatocytes and the Organisation of the Chromosomes, including the Accessory " (*Jour. Morph.* vol. xxxi. June 1918).

General.—Metcalf, in a paper entitled " Darwinism and Nations " (*Anat. Rec.* vol. xiv. May 1918), has endeavoured to meet the German claim that the struggle for existence between nations must be unrestricted in order that the mightiest nation should become dominant. It is pointed out that that physical force is not the only criterion of survival, and such things as moral law, co-operation, etc., are also important factors. " The north-east Germans are much more defective morally than the southern and western Germans. They are also deficient in intellectual ability and esthetic appreciation. The south Germans are not so." There follow classified quotations from German sources illustrating the deficiencies noted in the body of the paper.

Other papers include: Guyer and Smith, " Studies on Cytolysins. I. Some Pre-natal Effects on Lens Antibodies " (*Jour. Exp. Zool.* vol. xxvi. May 1918) ; Whiting, " Inheritance of Coat-Colour in Cats " (*ibid.* April) ; McClung, " Some Considerations regarding Microscopical Technique " (*Anat. Rec.* vol. xiv. May 1918) ; and Danchakoff, " Cell Potentialities and Differential Factors, considered in Relation to Erythropoiesis " (*Amer. Jour. Anat.* vol. xxiv. May 1918).

ARTICLES

SUR LA LOI DE L'ÉVOLUTION IRRÉVERSIBLE

PAR BRANISLAV PETRONIEVICS, PH.D.

LOUIS DOLLO, le grand paléontologiste belge, a formulé pour la première fois publiquement en 1893 (comp. Dollo, 4) sa célèbre loi de l'évolution irréversible, une des lois les plus importantes de l'évolution organique.[1] On a souvent discuté et appliqué cette loi, mais je ne connais personne qui ait essayé d'en faire l'exposition en s'appuyant sur les travaux respectifs de L. Dollo lui-même. C'est ce que je me propose de faire ici, en ajoutant à mon exposition quelques remarques critiques sur la valeur de la loi en question.

La loi de l'évolution irréversible a été formulée par Dollo de la manière suivante : " Un organisme ne peut retourner, même partiellement, à un état antérieur, déjà réalisé dans la série de ses ancêtres " (comp. Dollo, 4, p. 165).[2]

On conçoit ordinairement que la loi ainsi définie se rapporte seulement aux parties et aux organes réduits ou disparus, ce qui n'est pas exact. Le sens de la loi est plus vaste, il embrasse aussi les organes fonctionnels (comp. Dollo, 16, remarque (2) p. 400).

Pour mieux comprendre le sens vaste de la loi de Dollo,

[1] Auparavant, il a annoncé cette loi en 1892, dans son *Cours autographié*, etc. (comp. Dollo, 1), la même année dans une Note parue dans le *Bulletin de la soc. belge de Géol.*, etc. (comp. Dollo, 2), et dans un article paru dans *Bulletin scientifique de la France et de la Belgique*, d'A. Giard (comp. Dollo, 3).

[2] Plus tard, Dollo a donné une formule encore plus rigoureuse de sa loi :

"Un organisme ne reprend jamais exactement un état antérieur, même s'il se trouve placé dans des Conditions de l'Existence identiques à celles qu'il a traversées. Mais, en vertu de l'Indestructibilité du Passé, il garde toujours quelque trace des étapes intermédiaires qu'il a parcourues" (comp. Dollo, 17, p. 107, et 10, p. 443 s). Remarquons, que Dollo admet expressément la réversibilité des conditions d'existence : " L'évolution est irréversible, au point de vue de la structure des Organismes . . .,—mais réversible, au point de vue des conditions d'existence (Ethologie)" (Dollo, 7, p. 15).

il nous faut faire certaines distinctions dans le concept de l'évolution organique et en donner quelques définitions.

L'évolution organique peut être, comme on le sait, progressive, régressive et mixte.[1] Si dans l'évolution mixte (qui est le type le plus répandu dans le domaine de l'évolution organique) la progression prédomine, ou, autrement dit, si l'état final réalisé représente un progrès par comparaison à l'état initial, alors nous appellerons une telle sorte d'évolution mixte évolution ascendante, l'évolution progressive pure, sans ou avec l'addition des parties nouvelles, représentant le cas-limite de cette évolution. Mais si dans l'évolution mixte la régression prédomine, ou, autrement dit, si l'état final réalisé est un recul par comparaison à l'état initial, nous appellerons une pareille évolution évolution descendante, l'évolution régressive pure représentant évidemment le cas-limite d'une pareille évolution. Le pied du cheval, composé d'un seul doigt, qui provient d'un pied pentadactyle par l'atrophie des doigts latéraux et l'accroissement relativement plus grand du doigt médian, est l'exemple le plus connu de l'évolution ascendante mixte, tandis que le crâne du Ceratodus vivant représente, par comparaison au crâne de Dipterus, son ancêtre probable devonien, l'exemple d'une évolution mixte descendante.

En tenant compte d'une part des définitions que nous venons d'énoncer, et d'autre part, des exemples cités plus loin, que Dollo avait invoqués en faveur de sa loi, nous devons séparer les cas d'évolution ascendante de ceux d'évolution descendante, ce que Dollo lui-même n'a pas fait. Evidemment, si la structure d'un organe ou si les parties d'un organe sont perdus par l'évolution descendante, et s'il n'est pas possible, comme on l'accorde presque unanimement, de rétablir la structure perdue ou les parties perdues par une nouvelle évolution ascendante, il ne s'ensuit nullement—au moins *à priori*—qu'une réversibilité de l'évolution ne serait pas possible dans le cas contraire, c'est à dire quand la structure

[1] Dans mon cours libre (*Sur l'évolution universelle*) professé à la Sorbonne cette année-ci, qui sera publié plus tard en volume, j'ai défini l'évolution en général de la manière suivante : "l'évolution, c'est le devenir d'une chose par des degrés successifs de changement." Quand chaque état suivant du processus évolutif contient quelque chose de plus que l'état précédent, l'évolution est progressive, et régressive quand l'état suivant contient quelque chose de moins que l'état précédent. L'évolution est mixte, quand dans un tout qui évolue, l'une partie évolue progressivement, et l'autre régressivement.

d'un organe a été perdue par l'évolution ascendante de cet organe.

Nous devons donc remplacer la loi unique de Dollo par trois lois différentes, dont l'une, la première et la plus fondamentale, exprimera l'impossibilité d'une réacquisition des parties perdues, dont la seconde se rapportera aux cas où la structure d'un organe a été perdue par l'évolution ascendante, et la troisième aux cas où la structure a été perdue par l évolution descendante.

Voici ces trois lois :

1. Les organes et les parties des organes, réduits ou perdus par l'évolution régressive, ne peuvent pas être regagnés par une nouvelle évolution progressive.[1]

2. Si la structure d'un organe a été perdue par l'évolution ascendante (sans ou avec l'addition de parties nouvelles, sans ou avec la perte de quelques parties), la structure perdue ne peut pas être regagnée :

(*a*) Ni par la réacquisition des parties perdues, cette réacquisition étant impossible d'après la première loi ;

(*b*) Ni par l'évolution régressive des parties nouvelles, la disparition totale de ces parties étant impossible ;

(*c*) Ni par l'évolution ascendante de ces parties nouvelles dans une direction nouvelle.

3. Si la structure d'un organe a été perdue par l'évolution descendante (sans ou avec la perte de quelques parties), cette structure ne peut pas être regagnée :

(i) Ni par la réacquisition et l'évolution progressive des parties perdues, cette réacquisition étant impossible d'après la première loi ;

(ii) Ni par l'évolution ascendante des parties non-réduites dans une direction nouvelle ;

(iii) Ni par l'évolution ascendante des parties tout à fait nouvelles.

Nous voulons maintenant expliquer les différents cas tombant sous ces trois lois par des exemples que nous trouvons dans les écrits de Dollo.

Les exemples pour la première loi sont très nombreux. Les oiseaux ont perdu leurs dents pendant la période crétacée ;

[1] Pour la première loi comp. Dollo, 7, p. 5 (à propos de l'interclavicule perdue chez Dermochelys), Dollo, 9, p. 130 (à propos de l'œil pinéal atrophié de Pliopatecarpus), et Dollo, 16, rem. 2, p. 400.

aucun oiseau postérieur n'a pu regagner ces parties perdues. La mâchoire des mammifères consiste en une seule pièce homologue à la partie dentaire de ses ancêtres reptiliens : aucun mammifère n'a pu regagner les autres parties perdues de la mâchoire reptilienne, etc.

Mais ce sont surtout les exemples, où le retour à l'adaptation respective des ancêtres exigerait la réapparition des parties perdues chez un organisme, qui sont démonstratifs pour la validité de la première loi. Comme ces exemples sont en même temps des exemples pour les deux autres lois, nous voulons les exposer en connexion avec ces lois.

L'exemple le plus connu pour le premier cas de la deuxième loi, c'est la pseudo-dentition d'Odontopteryx, oiseau fossile Eocène. Au lieu des dents vraies perdues, Odontopteryx possède les bords du bec et de la mâchoire inférieure dentés en forme de scie.

L'exemple le plus frappant pour le deuxième cas de la seconde loi, c'est le bassin de Triceratops. Les ancêtres dinosauriens de Triceratops se sont adaptés à la vie bipède et comme tels ont possédé un ischium très long et très étroit et un pubis pourvu d'un postpubis également très long et très étroit (comp. Dollo, 10, p. 444). Dans son adaptation secondaire à la vie quadrupède, le Triceratops n'a pu regagner morphologiquement le bassin triradié de ses ancêtres lointains quadrupèdes. Car il a conservé les traces de la phase bipède dans le postpubis rudimentaire et dans l'ischium étroit et recourbé de son bassin, c'est à dire le postpubis, la partie nouvelle acquise dans la vie bipède, n'a pu disparaître totalement, et la nouvelle forme de son ischium n'a pu disparaître non plus (comp. Dollo, 10, p. 446).

L'exemple le plus important et le plus évident pour le troisième cas de la seconde loi, nous le trouvons aussi chez un Dinosaurien, étroitement lié au précédent, Stegosaurus. Les ancêtres immédiats de celui-ci ont été bipèdes comme ceux de Triceratops, et il s'est réadapté à la vie quadrupède comme celui-ci. Mais tandis que le bassin triradié des ancêtres lointains quadrupèdes a été réétabli physiologiquement par l'évolution régressive (par l'atrophie) du postpubis et de l'ischium chez Triceratops (Dollo, l.c. p. 446), chez Stegosaurus ces parties ont évolué dans une direction nouvelle. Chez Stegosaurus l'ischium se raccourcit et s'aplatit et le postpubis

fait de même et, en outre, s'applique intimement le long du bord ventral de l'ischium. Mais morphologiquement il n'y a pas ici non plus un retour à l'état du bassin triradié passé, car l'ischium a conservé quelque trace de sa forme acquise dans la phase bipède et la branche postérieure du bassin n'est plus constituée par l'ischium seul, mais par le complexe ischio-post-pubien. En évoluant dans la direction nouvelle, le postpubis a ainsi changé de fonction (Dollo, l.c. p. 447).

Nous trouvons un exemple pour le premier cas de la troisième loi dans l'évolution des bras chez les Octopodes. Les Octopodes, en s'adaptant à la vie benthique, ont perdu une paire de bras (les bras tentaculaires) de leurs ancêtres immédiats, les Décapodes hétéropodes. Ainsi ils sont redevenus isopodes (abstraction faite du cas exceptionnel de l'Argonaute et de l'hectocotylisation d'un de leurs bras) comme leurs ancêtres lointains, les Belemnotheutides (Décapodes isopodes), sans avoir pu regagner le même nombre de bras (comp. Dollo, 17, pp. 115–116).[1]

L'exemple le plus important pour le deuxième cas de la troisième loi, c'est le pied de Dendrolagus, un kangurou arboricole. La structure du pied des Macropodidæ sauteurs—la prédominance du quatrième orteil, la syndactylie du deuxième et du troisième, la réduction du cinquième et la disparition totale du gros orteil—nous montre que leurs ancêtres immédiats ont été arboricoles. Chez le Dendrolagus, un Macropodide qui est redevenu arboricole, le gros orteil opposable, totalement atrophié chez ses ancêtres immédiats, les kangurous terrestres, n'avait pu réapparaître. Mais les parties non réduites ont subi une évolution ascendante dans une nouvelle direction. Tandis que les métatarsiens et les phalanges ont diminué de longueur, les phalangines, les phalangettes et les griffes se sont allongées et les griffes recourbés en même temps. Ainsi le pied de Dendrolagus n'a pu retourner

[1] Un exemple encore plus convainquant serait la stégocéphalie secondaire des Chéloniens. Cette stégocéphalie se distingue de la stégocéphalie primaire des amphibiens stégocéphales ancestraux en ce que le postorbital, le supratemporal et l'épiotique, une fois perdus, ne réapparaissent plus dans la voûte cranienne (comp. Dollo, 19, p. 59). Mais la stégocéphalie secondaire des Chéloniens, quoique très probable (comp. surtout la Note récente de G. A. Boulenger, "Sur la place des Chéloniens dans la classification," dans *Comptes rendus*, t. 167, 1918, p. 514), n'est pas encore hors de doute. Comp. D. M. S. Watson, *Eunotosaurus africanus*, dans *Proceedings of the Zool. Soc. of London*, 1917, p. 1011 s.

à la structure du pied de ses ancêtres arboricoles lointains qui possédaient un gros orteil opposable, la syndactylie du deuxième et du troisième orteils, le quatrième orteil prédominant, et chez lesquels les griffes étaient réduites (comp. Dollo, 6, pp. 194 et 199).

Enfin, pour le troisième cas de la troisième loi, nous avons comme exemple la carapace et le plastron secondaires de la tortue Dermochelys. Les ancêtres lointains de cette tortue ont été, comme elle-même, des tortues marines ; son plastron primaire réduit (un anneau composé de quatre pièces) et sa carapace primaire encore plus réduite (représentée par la seule plaque nuchale) en témoignent. En s'adaptant à la vie littorale, les ancêtres immédiats de Dermochelys ont regagné une carapace et un plastron, mais cette carapace et ce plastron sont des parties tout-à-fait nouvelles d'origine dermale superficielle. En se réadaptant à la vie marine, Dermochelys a conservé cette carapace et ce plastron athèques de ses ancêtres immédiats, quoique tous deux déjà considérablement réduits (comp. Dollo, 7, pp. 9–14).

L'importance de la loi de l'évolution irréversible est multiple. Cette loi possède d'abord une application phylogénique, c'est à dire elle nous met en état de reconstituer avec le matériel paléontologique souvent si insuffisant dont nous disposons, si non de véritables séries phylogéniques, au moins des séries représentant des stades évolutifs incontestables. Son application éthologique est encore plus considérable : elle est souvent le seul moyen de déterminer les conditions d'existence et le mode d'adaptation à la vie des organismes fossiles. Mais quelquefois cette loi possède aussi une importance morphologique, parce que en nous en servant nous pouvons distinguer les homologies de pures analogies. Enfin, Dollo a montré qu'elle peut servir aussi comme guide dans la classification, qu'elle possède donc aussi une application systématique.

L'application phylogénique la plus importante de la loi a été faite par Dollo dans la difficile question de la phylogénie des Dipneustes, et le mémoire extrêmement ingénieux de Dollo sur cette question (comp. Dollo, 5) doit être regardé comme un modèle de l'esprit philosophique dans la paléontologie nouvelle. Avant Dollo cette question se trouvait dans un état vraiment chaotique, l'un des paléontologistes les plus éminents ayant déclaré le Dipterus, le type le plus ancien et le

plus primitif, être le type le plus specialisé.[1] Nulle part la notion de l'irréversibilité de l'évolution n'a donné de résultats si brillants qu'ici. Puisque cette notion exprime qu'on ne retourne jamais complètement à une structure ancestrale, on peut s'en servir pour reconnaître si une disposition est primaire ou secondaire et, par conséquent, quand on a une série entre les termes extrêmes de laquelle on possède des termes intermédiaires suffisants, pour déterminer le sens de l'évolution (comp. Dollo, 5, p. 97 [2]). Or, une pareille série nous l'avons dans la série paléontologique des Dipneustes : Dipterus Valenciennesi, Dipterus macropterus, Scaumenacia, Phaneropleuron, Uronemus, Ctenodus, Ceratodus, Protopterus, Lepidosiren (l.c. p. 88). Dollo nous montre que la structure de la queue aussi bien que celle du sommet de la tête, la squammation, les plaques jugulaires, l'appareil operculaire, la ganoïne et l'ossification de la mandible, le ruban sous-orbitaire, que tous cela témoigne, que l'évolution s'est faite dans cette série dans le sens Dipterus-Ceratodus et non inversement (l.c. pp. 86–7). C'est surtout par la structure de la queue que la notion de l'irréversibilité devient évidente. Dans une discussion longue et approfondie Dollo nous montre (l.c. pp. 89–97) que la queue diphycerque des Dipneustes (et des autres poissons modernes et anciens connus) est une queue diphycerque secondaire, dont la valeur morphologique chez les Dipneustes (la seconde nageoire dorsale + la seconde nageoire anale) n'est point equivalente à la valeur morphologique de la queue diphycerque primitive (nageoire caudale). On ne revient donc pas dans cette diphycercie secondaire à la structure primitive (l.c. p. 96).

Les autres cas de phylogénie les plus importants discutés

[1] Comp. A. S. Woodward, *Catalogue of the Fossil Fishes in the British Museum*, Pt. II. 1891, p. xx. Mais après la publication du mémoire important de Dollo, Woodward s'est rangé aux conclusions de Dollo. Comp. son Presidential Address to the Section of Geology, dans *Nature*, v. 81, 1909, p. 292 (et Dollo, 16, rem. (2), p. 387).

[2] En traitant la question de la phylogénie des Holocéphales (comp. Dollo, 13, pp. 107–8), Dollo dit: "La notion de l'Irréversibilité de l'Evolution—qui m'a conduit aux conclusions que je viens de justifier—a, une fois de plus, montré son utilité. Puisque—sans elle—on serait amené à soutenir que des Organismes spécialisés peuvent redevenir primitifs—pour se spécialiser à nouveau—dans la même direction, ou dans une direction différente. Postulat, qui—à moins de disposer de séries paléontologiques absolument complètes—ce dont nous sommes loin—détruirait toute possibilité d'arriver à la Phylogénie—but suprême, pourtant, de la Morphologie."

par Dollo sont : la phylogénie des Siréniens (Dollo, 3, p. 119), la phylogénie de la tortue Luth (Dollo, 7, p. 9), et la phylogénie des Holocéphales (Dollo, 13, p. 107).

Un des cas les plus importants quant à l'application éthologique de la loi de l'irréversibilité se trouve encore dans le mémoire sur la phylogénie des Dipneustes. Si l'on suppose que Dipterus provient de Ceratodus, comme celui-ci est une adaptation à la vie en eau corrompue, alors faudrait-il supposer, ou bien que Dipterus représente une adaptation à la vie dans la vase (eau corrompue aggravée), ou bien qu'il représente un retour à la vie en eau claire. Le premier cas étant celui de Lepidosiren, seul le deuxième reste à discuter (Dollo, 5, p. 99). Mais, les raisons paléontologiques et purement éthologiques laissant de côté (l.c. p. 100), la loi de l'irréversibilité de l'évolution s'y oppose nettement. Car " la ganoïne perdue se retrouverait-elle ? Le bouclier céphalique se résoudrait-il en ses éléments ancestraux ? Le ruban fibreux sous-orbitaire, avec ses osselets en nombre variable, formerait-il encore une arcade continue ? L'appareil operculaire reprendrait-il ses dimensions premières ? Les plaques jugulaires, évanouies, reapparaîtraient-elles ? " Tout cela étant réduit chez Ceratodus (l.c. p. 100), Dipterus ne peut donc représenter qu'une adaptation primaire à la vie en eau claire, c'est à dire que c'est un poisson pur (" le plus pisciforme de Dipneustes," l.c. p. 101).

Un autre cas important de l'application éthologique c'est la vie bipède chez les ancêtres immédiats de Stegosaurus et de Triceratops. Car " si l'évolution était réversible, ces deux Dinosauriens auraient repris exactement leur forme quadrupède antérieure, et on n'aurait pu distingeur leur vie quadrupède secondaire de leur vie quadrupède primaire " (Dollo, 10, p. 448).

Les autres cas importants de l'application éthologique sont : adaptation secondaire à la vie nectique des Pycnodontes (comp. Dollo, 17, pp. 108–9), adaptation secondaire à la vie nectique chez les Trilobites Deiphon et Æglina (Dollo, 16, pp. 410 et 412), etc.

Parmi les cas de l'application morphologique celui des nageoires ventrales abdominales secondaires chez les Téléostéens possède une importance spéciale. Comme on le sait, les ventrales des Téléostéens peuvent être ou abdominales, ou thoraciques ou jugulaires. Or, parmi les ventrales abdominales

nous avons deux categories : celles qui n'ont absolument aucune connexion avec la ceinture scapulaire et celles reliées à la symphyse claviculaire par un ligament. Comme il n'y a aucune raison pour l'existence de ce ligament en situ, nous devons supposer qu'il est le résidu dégéneré d'une connexion antérieure directe avec la ceinture scapulaire. Conformément à l'irréversibilité de l'évolution les ventrales sont redevenues abdominales, mais elles ont gardé leur connexion de thoraciques ou de jugulaires passés avec la symphyse claviculaire (comp. Dollo 14, p. 139).

Les autres cas importants de l'application morphologique sont : (1) les choanes très antérieures des Chéloniens marins (Dollo, 8, pp. 817–20), (2) la longirostrie et brevirostrie des Crocodiliens (Dollo, 12, p. 85), etc.

Enfin, il faut mentionner aussi le seul cas où Dollo a fait une application systématique de sa loi, celui des Ptyctodontes. Avant Dollo on avait classifié ces poissons fossiles, connus jusqu'alors seulement par des plaques dentaires, parmi les Holocéphales. Dans son mémoire important sur ce sujet (comp. Dollo 13), Dollo a montré, en s'appuyant sur la loi de l'évolution irréversible, que les Ptyctodontes ne peuvent pas être considérés comme Holocéphales, et qu'on doit les considérer comme des Arthrodères. Depuis, la conclusion de Dollo a été entièrement confirmée.

Quoique l'évidence empirique pour la validité de sa loi a été nombreuse et variée, Dollo ne s'est pas contenté d'une pareille démonstration purement empirique. Il a essayé d'en donner aussi une démonstration déductive. Il dit à cet égard :

" L'Irréversibilité de l'Evolution n'est pas simplement une loi empirique, reposant uniquement sur des faits d'observation, comme plusieurs l'ont cru. Mais elle a des Causes profondes, qui la ramènent, en dernière analyse, à une question de Probabilités, comme les autres lois naturelles. En effet, l'Evolution, étant une sommation de variations individuelles parfaitement déterminées, dans un ordre parfaitement déterminé—pour qu'elle fut Réversible—il faudrait admettre l'intervention de causes exactement inverses de celles qui ont donné naissance aux variations individuelles, sources de la première transformation, et à leur fixation, et dans un ordre exactement inverse aussi,—circonstances trop complexes pour qu'on puisse supposer qu'elles se réalisent jamais " (Dollo, 19, p. 59 rem. ; comp. aussi Dollo, 3, p. 127).

Et, en parlant de l'impossibilité d'une descendance de Dipterus de Ceratodus (Dollo, 5, p. 100, le passage cité plus haut) il dit :

" Et remarquons qu'il ne s'agit pas, ici, d'un caractère isolé—mais de toute une série des caractères—ce qui est bien plus grave en matière d'irréversibilité. . . . Or, c'est surtout dans son action sur des éléments aussi multiples qu'on peut affirmer, avec certitude, que l'évolution n'est pas réversible" (l.c. rem. 72, p. 122).

L'irréversibilité de l'évolution devient donc d'après Dollo d'autant plus probable qu'il s'agit d'un nombre plus grand d'éléments, et elle est pratiquement une nécessité quand il s'agit d'un nombre considérable d'éléments.

Ayant exposé la loi de l'évolution irréversible, les différents cas qu'elle explique, ses applications et sa base logique, nous voulons maintenant faire quelques observations critiques sur ces différents côtés de la loi.

D'abord sa base logique. La démonstration déductive de sa loi, tentée par Dollo, est très douteuse. Quant au nombre des éléments sur lesquels agit l'évolution, ce ne sont pas les cellules dont il s'agit, mais les organes et les parties des organes (parce que ce sont ces derniers seuls qui possèdent des dispositions déterminées dans le germen), et le nombre des ceux-ci n'est pas relativement considérable même dans l'organisme le plus complexe. Or, si nous tenons compte du nombre beaucoup plus considérable des individus, chez lesquels les organes et les parties des organes présentent des variations individuelles, la probabilité de leurs variations dans des directions différentes, et par conséquent aussi dans des directions inverses, devient possible. Ce n'est que si l'on affirme que les 'variations individuelles sont relativement peu nombreuses et prédéterminées, que ce raisonnement, fondé sur la probabilité pure, devient caduc. Mais alors la loi de l'évolution irréversible n'est pas le résultat de probabilité du nombre, mais le résultat de causes internes inconnues de l'évo lution organique.

Il n'y a donc aucune nécessité logique dans la loi de l'évolution irréversible, et cette loi reste une règle purement empirique. Voyons maintenant, combien les trois lois de cette évolution sont confirmées par l'expérience et dans quelle mesure on pourrait s'attendre à des exceptions possibles.

Quant à la première loi, cette loi semble être sans exceptions en tant qu'elle se rapporte aux organes et aux parties perdues. En effet, la perte d'un organe ou d'une partie étant devenue définitive par la perte de disposition correspondante dans le germen, il est presque impossible de supposer une réapparition de cette disposition, ayant en vue la difficulté de production des dispositions nouvelles dans le germen par l'influence des causes externes d'une part, et le degré de corrélation nécessaire de ces dispositions d'autre part.[1] Quand il s'agit d'un organe ou d'une partie réduite, alors deux cas sont à distinguer. Si la réduction a avancé autant que la disposition correspondante dans le germen tend vers sa disparition complète, l'organe réduit ou la partie réduite se trouveront pratiquement dans le même état que s'ils étaient déjà perdus. Mais si leur réduction n'a pas atteint un pareil degré, leur évolution en sens inverse ne sera pas impossible.

Pour la deuxième loi, il faut distinguer le cas d'une seule partie du cas d'un organe complexe. L'évolution régressive d'une seule partie, si pendant cette évolution et celle de l'évolution progressive précédente aucun changement de forme n'avait eu lieu, pourrait évidemment rétablir le point de départ de l'évolution progressive. Et l'évolution régressive d'une seule partie, si la disposition correspondante dans le germen n'est pas par trop affaiblie, pourrait évidemment aussi être suivie d'une nouvelle évolution progressive. Mais si un changement de forme s'est effectué pendant la première évolution progressive, et si ce changement de forme a été si considérable, qu'un changement dans la disposition correspondante du germen a été nécessaire, alors ni l'évolution régressive succédant à l'évolution progressive première ne pourra rétablir le point de départ de celle-ci, ni une nouvelle évolution progressive ne sera en état de le faire, puisque cela exigerait le retour à une disposition disparue. Si par exemple une dent s'était d'abord accrue et après diminuée sans changement de forme, cette dent pourra reprendre par diminution les dimensions qu'elle possédait au commencement de son accroissement, et un accroissement nouveau de la même forme ne sera point impossible (si la réduction n'a pas avancé par trop). Mais si l'accroissement a été accompagné d'un

[1] Comp. les réflexions similaires d'A. Handlirsch, 24, s 1328 (citées par Dollo, 15, rem. (2), p. 429).

changement radical de forme, si par exemple une dent conique est devenue comprimée latéralement, alors un retour à la forme conique ne sera pas possible ni pendant sa dimunition,[1] ni pendant un nouvel accroissement.

Quand il s'agit d'un organe complexe, Dollo affirme que son retour à l'état antérieur par l'évolution régressive n'est pas possible à cause de "l'indestructibilité du passé." Mais s'il s'agit d'une seule partie réduite d'un organe comme étant la raison prétendue de l'irréversibilité, on peut affirmer presque avec certitude que l'indestructibilité du passé n'existe pas en ce cas, puisqu'elle contredit à la loi si bien établie de l'évolution régressive nécessaire des parties et des organes non-fonctionnels. La réversibilité de l'évolution ascendante d'un organe complexe, quand elle dépend d'une partie réduite, n'est donc pas impossible (on peut supposer, par exemple, que les ventrales secondaires de Téléostéens reviendront dans l'avenir à l'état primaire par la disparition complète de ligament correspondant).

S'agit-il d'une partie non-réduite, mais dont la forme a changé pendant l'évolution ascendante d'un organe, alors l'indestructibilité du passé n'existe en sens strict non plus, la partie non-réduite pouvant changer de nouveau sa forme par une évolution progressive nouvelle, quoique l'état primaire de cette partie et par conséquent l'état primaire de l'organe en question ne puissent pas être rétablis. Ce n'est donc pas l'indestructibilité du passé, qui est la raison pour l'irréversibilité de l'évolution en ce cas. Prenons l'exemple du bassin de Triceratops. Le postpubis de ce bassin se trouve dans un état très rudimentaire, et comme des parties rudimentaires tendent à disparaître, si Triceratops aurait vécu plus longtemps, son postpubis aurait disparu certainement. Ce n'est donc que son ischium recourbé, très différent par sa forme de l'ischium de ses ancêtres tétrapodes lointains, qui a pu empêcher le retour de son bassin à l'état primaire.

Enfin, s'il y a une évolution ascendante des parties non-

[1] Cette impossibilité est justement ce que suppose W. D. Matthew comme ayant eu lieu pendant l'évolution des Felidæ, en supposant que les Félins proviennent de Dinictis, un chat machærodonte primitif (comp. W. D. Matthew, "The Phylogeny of the Felidæ," *Bulletin of the American Museum of Natural History*, v. xxviii. 1910, p. 290 s). Que cette phylogénie contredit à la loi de l'évolution irréversible, Scott l'a entrevu clairement (comp. W. B. Scott, 28, p. 540 s).

réduites (cas du bassin de Stegosaurus), alors c'est le changement de fonction qui sauve ces parties d'une évolution régressive; l'indestructibilité du passé n'existe donc ici non plus. Et il est clair que les mêmes raisonnements s'appliquent aussi d'après la troisième loi à l'évolution d'un organe complexe.

En resumé, l'irréversibilité de l'évolution d'un organe complexe dépend complètement de l'irréversibilité de l'évolution des parties particulières réduites ou non-réduites, qui le composent, et les deuxième et troisième lois ne sont pas non plus sans exceptions à cet égard: comme on l'a vu, c'est toujours la base germinale de la première loi, qui en est la raison dernière.

Comme je l'ai remarqué au début, la plupart des naturalistes ne connaissent que la première loi de Dollo, qui n'est qu'une partie de sa loi générale, quoique la partie la plus importante et la plus certaine.[1] Cette loi générale possède, malgré les exceptions possibles, une importance extraordinaire pour la philosophie biologique et la philosophie évolutioniste en général, et Dollo sera toujours considéré, comme Cuvier avant lui, comme le fondateur d'une grande loi du monde organique.[2]

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[1] A côté de la loi de l'évolution irréversible, Dollo a formulé (comp. Dollo, 4, p. 165) encore deux lois, celle de l'évolution discontinue (avant H. de Vries) et celle de l'évolution limitée. Dans ses écrits postérieurs, Dollo n'est revenu que rarement sur ces deux autres lois (sur la loi de l'évolution limitée, comp. Dollo, 7, p. 9; Dollo, 8, p. 813 et p. 820; Dollo, 9, p. 131; sur la loi de l'évolution discontinue, comp. Dollo, 5, rem. (66), p. 120; Dollo, 7, rem. (11), p. 9; et Dollo, 17, pp. 139–40).

[2] A la liste des travaux de Dollo sur la loi de l'évolution irréversible, j'ajoute les titres des quelques autres ouvrages (la plupart cités déjà par Dollo), dont les auteurs ont discuté ou appliqué cette loi. Parmi ces ouvrages, le livre important d'O. Abel, *Grundzüge der Palæobiologie der Wirbeltiere*, 1912, mérite une mention spéciale, parce qu'il contient presque tous les exemples cités par Dollo et en outre beaucoup d'autres. La loi de Dollo s'y trouve discutée pp. 616–18.

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THE GEOLOGICAL ASPECTS OF THE CORAL-REEF PROBLEM

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CONTENTS

Dana's Confirmation of Darwin's Theory.—The old coral-reef problem gained a new attraction for me some twenty years ago, when the embayed shorelines of the reef-encircled volcanic islands in the Pacific were recognised as giving independent support to Darwin's theory of upgrowth during intermittent subsidence; but it was only about five years ago that I learned how clearly Dana had in 1849 made explicit announcement of this physiographic evidence for subsidence and how generally the evidence had been overlooked by others, including Darwin himself. True, Penck had in 1896 perceived that the embayed shoreline of Queensland, as represented on large-scale charts, proved a submergence of the coast in association with the formation of the Great Barrier Reef in front of it; and Andrews,

Hedley, and Taylor of Sydney, New South Wales, had confirmed this inference by observations early in the 1900's on the coast itself; Marshall of Dunedin, New Zealand, similarly interpreted the embayments of certain barrier-reef islands that he visited a few years later in the central Pacific; and Vaughan has in recent years called attention to the embayments of reef-bordered coasts in the West Indies.

At an earlier date Crosby had noted the association of "half-drowned valleys" with the coral reefs of Cuba (1884); and Bonney had briefly called attention to the support given by embayments to the theory of subsidence, in the supplementary pages which he appended to the third edition of Darwin's *Coral Reefs* (1889). Nevertheless, the pertinence of Dana's evidence for Darwin's theory had gained no general recognition; I therefore prepared an article emphasising the value of "Dana's principle," and published it in the *American Journal of Science*, with which he had long been associated as editor, on the centenary of his birth.[1]

Exclusion of Still-stand Theories.—A year later the unexpected opportunity came to me of making a voyage across the Pacific, for which generous subventions were provided by the Shaler Memorial Fund of Harvard University and by the British Association for the Advancement of Science, whose meeting in Australia I attended; thus some thirty reef-encircled islands, as well as a long stretch of the Queensland coast inside the Great Barrier Reef of Australia, were briefly examined. In every case, the embayed shorelines of the mountainous islands within the barrier reefs proclaimed submergence. Furthermore, an examination of many barrier-reef charts shows that the embayment of the central islands is a universal characteristic. Hence all still-stand theories, which postulate a fixed relation between reef foundation and ocean-level, are disproved for barrier reefs; and as atolls resemble barrier reefs in all respects except in not having any central islands, the still-stand theories are made, to say the least, extremely improbable for atolls also.

Reef upgrowth thus appears to be intimately associated with submergence whenever the matter can be tested; and hence, if submergence could be caused only by subsidence, Darwin's theory of reef upgrowth during the intermittent sub-

[1] "Dana's Confirmation of Darwin's Theory of Coral Reefs," *Amer. Journ. Sci.* xxxv. 1913, 173–188.

sidence of the reef foundations would be established for barrier reefs, and made more probable than any other theory for atolls also. But, as if to add new interest to this old problem, two other causes of submergence, independent of reef-foundation subsidence, have been suggested: one is a general rise of ocean level, due to uplift of some part of the ocean floor outside of the coral-reef regions, the consequences of which have been set forth chiefly by Vaughan[1]; the other is the rise of ocean level due to the melting of Pleistocene ice sheets, as outlined some years ago by Belt, Upham, and Penck, and as recently analysed in detail by Daly.[2]

Distinction between Subsidence and Submergence.—The solution of the coral-reef problem therefore depends at present upon the discovery of some means of discriminating among the three causes of submergence indicated above. No means of absolute discrimination are known, because so many elements of the problem are beyond the reach of observation; but various considerations lead me to regard changes of ocean level as of secondary importance and to attribute the submergence which is demanded by reef-encircled islands, and which is therefore made probable for atolls as well, to local subsidence, essentially as Darwin and Dana supposed. The reasons for this opinion may be briefly stated.

As to a general rise of ocean level caused by an uplift of some part of the ocean floor, this cause of submergence is discarded, not only because it demands extravagant measures of ocean-floor uplift in order to produce the necessary changes of ocean level, but also because the submergence that it would produce must be everywhere of the same date, rate, and amount; whereas the features of various coral reefs demand submergences varying in place, date, and amount, if not in rate also. This aspect of the problem has been considered in another article,[3] and need not be further pursued here.

As to the sub-recent rise of ocean level caused bythe melting of the Pleistocene ice sheets as a means of accounting for the

[1] T. W. Vaughan, "The Origin of Barrier Coral Reefs," *Amer. Journ. Sci.* xli. 1916, 131–135.

[2] R. A. Daly, "The Glacial-control Theory of Coral Reefs," *Proc. Amer Acad. Arts and Sci.* li. 1915, 157–251.

[3] "A Shaler Memorial Study of Coral Reefs," *Amer. Journ. Sci.* xl. 1915, 223–271; see p. 259.

submergence of stationary reef foundations and the upgrowth of their reefs, this " Glacial-control " theory, like various other theories, gives a possible means of accounting for the few observable features of atolls, provided the various assumptions that go with the theory are accepted ; but when the consequences of the theory are confronted with the more exacting requirements of barrier reefs and their encircled islands, the theory proves unsatisfactory, because the emergence preceding the submergence is of too short a duration, and because the submergence that it provides is small and uniform in amount, and everywhere of the same date ; also because the postulate of still-standing islands and reef foundations is extremely improbable, and for other reasons as well. This aspect of the problem also I have endeavoured to set forth in an article already published.[1]

Unconformable Contact of Reef Limestones and their Foundations.—Attention will be directed in the present article chiefly to one highly significant structural feature of coral reefs ; a feature that, in spite of its strong testimony for subsidence, has been even more generally neglected than the embayed shorelines of reef-encircled islands: namely, the unconformable contact of reef and lagoon limestones with their foundations, as illustrated in section M of sector L, Fig. 1. This is a matter which is, one may say, tacitly involved in all theories that recognise the association of submergence with reef upgrowth, and the nature and the extent of the unconformity are of critical importance in estimating the amount of submergence and the duration of the preceding emergence of the reef foundations ; but, in spite of the century-long establishment of the elementary principle here involved, its importance has been almost universally overlooked, because the origin of coral reefs has been too generally treated as a zoological instead of as a geological problem.

It is true that a few small and discontinuous fringing reefs which border certain young volcanic islands, and a few other small and discontinuous reefs which occur in association with certain delta deposits may be regarded as lying conformably on a non-eroded under-structure ; but with these unimportant exceptions the limestones of all fringing and barrier reefs,

[1] "Problems Associated with the Study of Coral Reefs," *Sci. Monthly*, ii. 1916, 313–333, 479–501, 557–572 ; see p. 563.

whether now at sea-level in the process of formation or elevated above sea-level since their formation, appear to lie, as far as known, with striking unconformity on strongly eroded foundations. Hence the foundations, mostly volcanic islands, must have suffered erosion during a considerable period before they were submerged, in preparation for the unconformable deposition of the reef limestone upon them. Surely no geologist can hesitate to adopt this conclusion, if the facts of structure are as stated.

Furthermore, inasmuch as most fringing reefs which front the open sea in a narrow belt around their island lie, like the

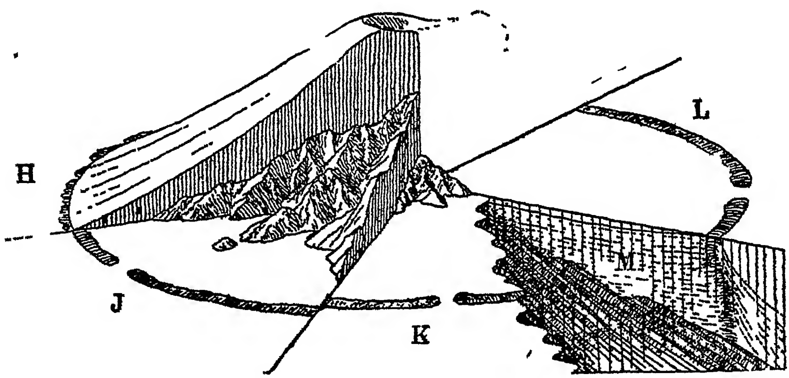

FIG. 1.—Sector diagram, illustrating successive stages in the formation of coral reefs around a subsiding volcanic island

As a result of subsidence, the dissected volcano must have an embayed shoreline, as in sectors J and K, and the lagoon deposits of barrier reefs and atolls must lie unconformably on the eroded surface of the submerged slope, as in section M The central islands within barrier reefs usually exhibit the features here drawn, but their bays are more or less filled with deltas, and their points are bordered by fringing reefs A number of uplifted reefs are known to rest unconformably on a slanting and eroded foundation, as in section M.

fringing reefs that commonly occur around a central island within a barrier reef, unconformably upon a strongly eroded volcanic surface, it must be concluded that fringing reefs do not mark stationary or rising islands as generally as Darwin supposed; but it is well to remember that Darwin had two views regarding reefs of this kind. The view to which he gave the most emphasis, and which is commonly associated with his theory of intermittent subsidence, was, as just implied, that fringing reefs are formed on stationary or rising islands. But he also announced another view, as follows: "If during the prolonged subsidence of a shore, coral-reefs grew for the first

time on it, or if an old barrier-reef were destroyed and submerged, and new reefs became attached to the land, these would necessarily at first belong to the fringing class."[1] Such is, I believe, the origin of the fringing reefs around Tutuila in the Samoan group, Fauro in the Solomon group, and elsewhere; for they border strongly eroded volcanic islands.

In the case of most sea-level reefs, the amount of erosion that took place during the period of emergence antecedent to submergence and reef-growth is, to the best of my judgment, greater than could have been accomplished during the Glacial epochs of lowered sea-level. It is not simply the erosion of valleys, now embayed, that is to be accounted for, but the general erosion of a volcanic island or of a continental island or coast, whereby the form of the reef foundation was profoundly altered before the reef limestones were deposited upon it. Fauro in the Solomon group is a case in point, for it is described by Guppy as a volcanic wreck, greatly reduced from its original dimensions. Furthermore, the depth to which the pre-submergence erosion proceeded, as inferred not from the present depth of embayments, which has been lessened from the original measure by post-submergence deposits, but from the size and form of the embayments as well as from the form of the spurs between them, appears in certain cases to have been two or three times greater than the maximum amount of lowering that the ocean could have temporarily suffered in consequence of the withdrawal of its waters to form the continental ice sheets of the Glacial period. Hence subsidence of the reef foundations, and not merely a change or an oscillation of ocean level, is demanded.

It is not only sea-level reefs that give this geological evidence for subsidence. Elevated reefs exhibit unconformable contacts with their foundations even more clearly than non-elevated reefs: indeed, in the case of certain elevated reefs that I have seen, the depth to which antecedent erosion progressed beneath the level to which the foundations were submerged during the formation of reef and lagoon limestones, measures 600 or 800 ft. The first example of the kind that came under my observation is on Vanua Mbalavu of the Fiji group: the vertical measure of the unconformity there seen is

[1] *Coral Reefs*, 1842, p. 124.

about 600 ft., but this is not its full measure, for it is continued to an unknown depth below sea level. This example has been confirmed a year after my visit of 1914 by Foye, who reports an unconformable elevated reef on Lakemba also, in the same group, with a minimum vertical measure of 320 ft.[1] On Efate, in the New Hebrides group, I saw terraces of unconformable elevated reefs up to 800 ft.

A few observers have called attention to the unconformable contacts of elevated reefs on their foundations. Nearly thirty years ago Walther recorded the occurrence of unconformable elevated reefs on the Sinai peninsula.[2] Some years afterwards Becker,[3] and, later, Smith,[4] announced the unconformable contact of a series of elevated reefs on Cebú in the Philippine Islands, where the highest reefs stand at an altitude of 2,362 ft. Wanner states that quaternary coral limestone lies unconformably on inclined conglomerates at an altitude of 400 metres in the east arm of Celebes.[5] Other geological studies in the East Indies give similar information. But in many accounts of elevated reefs the important question of unconformity is not mentioned: thus Levesey reports that "on top of a hill [in British New Guinea], judged to be over 3000 ft. high, . . . a large coral reef . . . stood out so clean cut, and so hard and solid, it appeared as if it had only emerged from the sea a short time previously"[6]; but the relation of the reef limestones to their foundation is not announced.

It seems as impossible to explain unconformable reefs without deep erosion before their formation and strong subsidence during their formation, as to explain their present altitude without uplift after their formation. True, Suess believed that the present altitude of elevated atolls in the Pacific was not due to their uplift, but to a lowering of the ocean surface; for he not only thought that all high-standing atolls had essentially

[1] W. G. Foye, "The Geology of the Lau Islands (Fiji)," *Amer. Journ. Sci.* xliii. 1917, 344–350.

[2] J. Walther, "Die Korallenriffe der Sinai-Halbinsel," *Abh. k. Sächs. Ges. Wiss., Math. Phys.* Cl. xiv. 1888, 439–506.

[3] G. F. Becker, "Geology of the Philippines," *21 Ann. Rep. U.S. Geol. Surv.*

[4] W. D. Smith, "Contributions to the Geology of the Philippine Islands: I. Cebú Island," *Phil. Journ. Sci.* i. 1906, 1043–1057.

[5] J. Wanner, "Beiträge zur Geologie des Ostarms der Insel Celebes," *N. Jahrb. f. Min.* 29 Beilageband, 1910, 739–778; see p. 770.

[6] *Geogr. Journ.* xii. 1899, 436.

the same altitude, but he overlooked the very variable amounts of erosion that they have suffered, although these variations indicate great differences in the dates of emergence ; and he had an unwarranted confidence in the evidence afforded by certain terraces for the almost universal emergence of continental shores by the same measure as the altitude of the high-standing atolls.

It is impossible to explain the facts now known regarding elevated reefs by regarding them as having stood still while the ocean surface sank, for the reefs stand at unequal heights and have suffered very unlike amounts of erosion.[1] Moreover, the unconformable sea-level and elevated reefs of certain archipelagoes, such as the Fiji Islands,[2] demand a rather complicated succession of unequal and non-synchronous submergences and emergences for their production, and are not to be accounted for by any universal and uniform rising and sinking of ocean level ; they can be explained only by local and diverse uplifts and subsidences of the islands themselves, the epochs of reef-formation being the epochs of subsidence. I believe the same statement will be shown true for the reefs of the Philippine and Solomon Islands, when they are more closely studied.

Darwin's theory alone gives a rational interpretation for such reefs. Inasmuch as atolls occur in association with archipelagoes where unconformable fringing and barrier reefs are found both at sea-level and elevated above sea-level, the same interpretation may be reasonably applied to the reefs of all three kinds thus associated. As to archipelagoes of atolls alone, no demonstration of origin can be given to-day ; but the presumption is manifest that they can be best explained in the same way as the atolls of barrier-reef archipelagoes ; and this presumption is confirmed by the Funafuti boring.

Origin of Elevated Reefs.—In this connection let it be noted that the not infrequent explanation of elevated reefs as having been formed during pauses in the elevation and emergence of their foundation, and as therefore contradicting Darwin's theory, is seriously defective, in that it does not consider whether the reefs lie conformably or unconformably on their

[1] "The Structure of High-standing Atolls," *Proc. Nat. Acad. Sci.* iii. 1917, 473–479.

[2] "Extinguished and Resurgent Coral Reefs," *Proc. Nat. Acad. Sci.* ii 1916, 466–471.

foundation. If conformable, their formation during pauses in elevation is indicated; but very few reefs of this kind are known. If unconformable, as is usually the case, a period of exposure to erosion, followed first by submergence, and later by emergence, must be recognised; and three alternative explanations for the reefs are then open. First, the reefs may have been formed during pauses in the final elevation following a rapid submergence without reef growth; second, the reefs may have been formed during pauses in the submergence preceding a rapid and final elevation without reef growth; third, some reefs may have been formed during pauses in submergence, and some during pauses in elevation. The only way to make safe choice among these alternatives is to examine the detailed structure of the reefs concerned. Observations of this kind are so difficult that they have seldom been made. Certain terraced reefs on Efate in the New Hebrides group, the structure of which is fairly well exposed in a ravine, seemed to me to accord better with the second or third explanation than with the first.

Inattention to Embayments and Unconformities.—It is not my object to insist that all coral reefs have been formed in one way; still less that they have all been formed by upgrowth during subsidence, although my personal belief is that that method of formation best accounts for the great majority of reef structures. The object of this article is to lead those readers who are still interested in the old problem to consider attentively both the qualitative and quantitative value of the two lines of geological evidence favouring subsidence that are provided by embayed shorelines and by unconformable contacts, for sea-level as well as for elevated reefs; also to examine not only Darwin's theory and the Glacial-control theory, but also the various still-stand theories of coral reefs, as set forth by Rein, Semper, Murray, Wharton, and others, in which no account whatever is taken of the two lines of geological evidence here pointed out; and then to select the theory that seems best able to account for the facts.

The result of such consideration and examination on my own part is the conclusion that, famous as are the contributions to the coral-reef problem by the above-named inventors of various still-stand theories, their inventions are to-day of little more than historic value, for their arguments are invalidated

by the neglect of essential factors. Let it be here remembered that the true interpretation of embayed shorelines was published by Dana in 1849, in an often-quoted report ; while the meaning of unconformable contacts was recognised half a century earlier. Surely the neglect of principles so well established in geological science and so pertinent in discussions of coral reefs, vitiates the conclusions that the discussions reach.

The chief reason for this neglect seems to be that the origin of coral reefs has been, as above noted, too often regarded as a zoological problem, whereas it is in reality for the most part a geological problem that can be solved only by geological methods. A living reef is, to be sure, a superb aquarium, where even a physiographer must marvel at the abundance, the variety, and the beauty of the organic forms that flourish upon it ; but the great body of the reef, from its inorganic foundation up to the sea surface, is a huge rock mass, the structural relations and the origin of which must be investigated by the methods ordinarily employed by geologists in making out the history of rock masses in general. A brief review of the contributions made to the coral-reef problem by some of its most noted investigators will show how far they have failed to meet this requirement.

Semper on the Reefs of the Pelew Islands.—Semper appears to have had no experience as a geologist. His famous studies of the Pelew Islands, which led him to the conclusion that the reefs there seen were constructed during the elevation of their foundations, must be rated as of little value when it is understood that he assumed the islands to have been formed by eruption below sea-level and to have emerged as a result of uplift ; that he took no account of embayed shorelines or of conformable or unconformable reef contacts ; and that he rejected so manifest a possibility as a gentle tilting, by which the southern part of the group, where elevated reefs occur, would be raised, while the northern part, where atolls occur, might be depressed.[1]

From all that Semper and other observers of the Pelew

[1] Semper's chief writings on this subject are: "Die Entstehung der Corallenriffe," *Verhandl. Phys.-med. Ges. Wurzburg*, i. 1869, vi–viii ; "Die Palau-Inseln im Stillen Ozean," Leipzig, 1873 ; "Die naturlichen Existenzbedingungen der Thiere," Leipzig, 1880 ; translated as "Animal Life as Affected by Natural Conditions," 1881 ; see Chapters VII., VIII.

group have written, it is eminently possible that those islands were built up above sea-level by volcanic eruptions; that they were strongly eroded during a higher stand than that of to-day; that subsidence then took place, during which coral reefs and lagoon limestones were unconformably deposited on the eroded and submerged volcanic slopes; that a slight tilting afterwards caused the emergence of the reefs now elevated above sea-level in the southern part of the group, while permitting the continued upgrowth of several atolls in the northern part; and that a moderate subsidence has taken place since the tilting, whereby a new barrier reef, now at sea-level, has been built up around the embayed shoreline of the southern elevated reefs. It is singular to note that these manifest possibilities have been about as little considered by the many geological authors who have quoted Semper against Darwin as they were by Semper himself.

Rein on the Origin of Atolls.—Rein first suggested the possibility that atolls may have been formed as coral crowns upon submarine banks that were built up to small depth by the accumulation of organic deposits upon comparatively deep, still-standing foundations[1]; but the suggestion still remains merely a possibility without any direct evidence in its favour, as far as the stability of the submarine foundations is concerned. Rein's inexperience in geological argument may be measured by his statement that the elevation of marine deposits on the two sides of the Atlantic, in Africa and North America, gave reason for thinking that Bermuda had not subsided, as well as by his failure to inquire into the structural relations of reefs elsewhere than in Bermuda, and by his belief that the occurrence of certain elevated reefs argued against their formation during subsidence.

Murray on Atolls and Barrier Reefs.—Rein's suggestion as to the origin of atolls was independently announced by Murray in 1880,[2] whereupon it gained an undeserved popularity; for although Murray had had the enviable opportunity of serving on the staff of the *Challenger*, and of thus seeing a number of coral reefs during his voyage, his preparation for geological

[1] J. J. Rein, "Beiträge zur physikalischen Geographie der Bermuda-Inseln," *Ber. Senckenb. naturf Ges.* 1870, 140–158; "Die Bermuda-Inseln und ihre Korallenriffe" . . . *Verh. 1sten Deut Geogr'tag*, 1881, 129–146.

[2] J. Murray, "On the Structure and Origin of Coral Reefs," *Proc. Roy. Soc. Edinb.* x. 1880, 505–518; "Structure, Origin, and Distribution of Coral Reefs," *Proc. Roy. Inst.* xii. 1888, 251–262.

investigation seems to have been little, if any, greater than that of his German predecessors. He argued against the theory of subsidence on the ground that volcanic islands ought to occupy areas of elevation, and that subsidence should be expected only in the intermediate oceanic areas; he thought that barrier reefs were formed by outgrowth around stationary islands; he failed to recognise that, if barrier reefs were thus formed, the fringing reefs and the lagoon limestones inside of the barrier reefs should rest conformably, as in section M, Fig. 2, on the non-eroded submarine extension of the island slopes; that the islands should not have embayed shorelines; and that extensive deltas

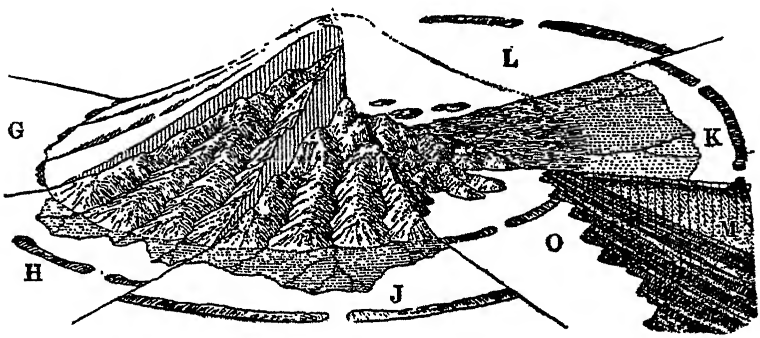

FIG. 2.—Sector diagram, illustrating successive stages of atoll development by the outgrowth of a barrier reef around a still-standing volcanic island, which is gradually worn away.

Note the increasing width of the alluvial plain fronting the non-embayed mountain base in sectors H and J; the reduction of the island to a lowland in sector K, and the removal of the lowland in sector L. The outgrowing reef rests on its advancing talus, and the talus rests conformably upon the non-eroded submarine slope of the volcano, as in section M. No reef-encircled islands are known which present the features shown in sectors H, J, K, or L. Islands within barrier reefs usually have tapering spurs advancing between drowned-valley embayments, as in sector O.

should stretch forward from simple shorelines into wide lagoons, as in sectors J and H; and he failed also to recognise that the facts contradict these essential consequences of his theory. He believed also that lagoons were excavated by solution, although lagoon floors give no indication of such origin, for they are covered with accumulating calcareous deposits; and he even suggested that outgrowing barrier reefs might be converted into atolls by the gradual wearing down of their stationary central islands, although no example of a transitional stage, in which the central island is worn down to low relief and surrounded by delta plains, as shown in sector K, Fig. 2, is known.

Wharton on the Truncation of Atoll Foundations.—Wharton's hypothesis that the flat floors of atoll lagoons represent volcanic islands truncated by marine abrasion[1] is, like various other theories, easily conceivable, but it involves contradictory processes, and it has no independent support. It does not explain why reef-forming corals should fail to form reefs while the truncation is in progress, and yet succeed in forming reefs after the truncation is completed. If atoll foundations have been truncated, then uplifted and dissected atolls, such as occur

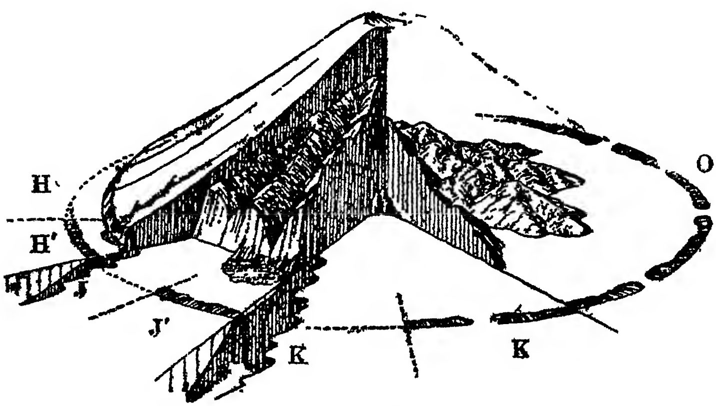

FIG. 3.—Sector diagram, illustrating successive stages in the truncation of a still-standing volcanic island by ocean waves, and the subsequent growth of a reef on the resulting submarine platform.

Before the reef is formed, the cliffs at the back of the platform will be free from deltas and talus, as in sectors H, J, and K: after the reef is formed, deltas and talus will advance from the cliff base into the enclosed lagoon, as in sectors H' and J'. The reefs must be of small thickness and must rest unconformably on a flat rock floor. Tahiti is one of the very few reef-encircled islands that has such features, it resembles sector J', except that its delta plain extends inland into the valleys, thus showing that submergence has taken place since its cliffs were cut. Most reef-encircled islands resemble sector O.

in Fiji in moderate number, should show a flat platform of volcanic rocks beneath their limestones; but no such platforms are known. If volcanic islands were truncated before atoll reefs grew up around the margin of the truncated surface, the central volcanic islands of barrier reefs should be at least cut back in cliffs, as shown in Fig. 3, but that is very rarely the case. As the central islands of barrier and of fringing reefs give good evidence by a slanting unconformity that strong subsidence occurred before or during reef formation, it is

[1] W. J. L. Wharton, [Address], *Report Br. Assoc. Adv. Sci.* 1894, 699–710; "Foundations of Coral Atolls," *Nature*, lv. 1897, 390–393.

difficult to understand why the islands on which atolls were to be formed should stand still for a long time before atoll formation began; but, like nearly all other students of the coral-reef problem, Wharton took no account of these aspects of the problem.

However, it is, I believe, possible that a still-standing volcanic island should, after its eruptive construction, be cut away by ocean waves, because no reefs can be formed on the beach of loose detritus that would surround such an island while abrasion is in progress [1]; but it would require an enormously long period of undisturbed abrasion to produce a platform for a large atoll and especially to reduce its central part to a depth of thirty or more fathoms; and so long a period of stability seems unreasonable, in view of the testimony of barrier-reef islands, as above indicated. Moreover, unless an abraded platform were disturbed after it was formed it would probably remain without reefs indefinitely. The Glacial-control theory, as expounded by Daly, gives a more plausible explanation of the smoothness and depth of atoll lagoon floors than is provided by Wharton's hypothesis; but, as above noted, there are serious difficulties in the way of accepting any theory of still-standing and truncated atoll foundations.

Inadequate Treatment of the Coral-Reef Problem.—It is not only the several investigators above-named who have overlooked the geological evidence for the subsidence theory of coral reefs that is provided by embayed shorelines and unconformable contacts. Nearly all other students of the problem have been equally inattentive to its geological aspects, although embayed shorelines and unconformable contacts are almost immediately discoverable by observation as facts of occurrence and are very readily detected by deduction as essential consequences of the theory of subsidence. I have searched through many articles by observers abroad and by students at home, and it is very rarely the case that they mention these significant structural features of fringing and barrier reefs, whether they accept or reject Darwin's theory.

Accounts of the Pelew Islands by Kubary ('73), Wichman ('75), Friederichsen ('01), and Wiszwianski ('09), are silent on this aspect of the problem; so are the accounts of the Seychelles by Coppinger of the *Alert* ('83), Keller ('98), Chun of the *Valdivia*

[1] "Clift Islands in the Coral Seas," *Proc. Nat. Acad. Sci.* ii. 1916, 284–288.

('00), and Gardiner ('06). The same is true of various articles on reefs by Studer of the *Gazelle* ('77), and of the zoological report of that vessel's exploration ('89); also of the descriptions of Rodriguez by Balfour and Maskelyne ('79); of observations by H. O. Forbes on the East Indian archipelago ('79), and of his discussion of the Great Barrier reef of Australia ('93); of articles by Lehnert on Borneo ('82), and by Hoffmann on the Society Islands ('82), of descriptions of various reefs in the *Challenger* report ('85), and of the Royal Society report on Funafuti ('04); of articles by Hagar on the Marshall Islands ('86), Bourne on Diego Garcia ('88), Heilprin on Bermuda ('89), Hickson on North Celebes ('89), Lister of the *Egeria* on Tonga ('91), Jukes-Brown and Harrison on Barbadoes ('91), Saville Kent on the Great Barrier reef ('93), Bernard on New Caledonia ('95), Semon on East Indian reefs ('96), Dahl on the New Guinea region ('99), and Sollas on Funafuti ('99).

The same statement is true of articles by Ortmann ('92), Baumann ('99), Crossland ('02), Voeltzkow ('03), Bornhardt ('00), and Werth ('01), on reefs of the East African coast and islands, except that the last two observers recognise embayments as indicating subsidence; of Guppy's numerous reports on the reefs of the Solomon Islands, Fiji and other islands; of Gardiner's many studies in Fiji ('98), the Maldives ('02), and elsewhere ('86); of Agassiz' extended researches in Hawaii ('89), the Bahamas ('94), Australia ('96), Fiji ('98), and many other islands of the Pacific ('03). It is true, however, that the three last-named investigators mentioned the evidence for subsidence given by embayed shorelines in order to express their disbelief in its validity. The same statement holds regarding the report on Torres Straits by Haddon, Sollas, and Cole and regarding accounts of Yap in the Carolines by Volkens ('01), of Rota in the Mariana group by Fritz ('02), of Jaluit by Schnee ('04), of the New Hebrides by Mawson ('05), of the New Guinea region by Sapper ('10), Richarz ('10), and Wernicke ('12), of atolls in the Indian Ocean by Fryer ('10), of a general discussion of atolls by Wood-Jones, based on observations of Keeling atoll ('10), of Wallis Island by Viala ('11), of reefs in the East Indies by Niermeyer ('11) and Wichmann ('12), of various Pacific atolls by Elschner ('13), and of Nauru by Hambruch ('14).

General discussions of the coral-reef problem by Allman

('73), Jordan ('82), A. Geikie ('83), Seler ('84), Vetter ('84), Lapparent ('85), Perrier ('87), Caullery ('00), May ('02), Giraud ('02), and Seurat ('06) make no mention of the structural features here under consideration ; similar discussions by Langenbeck ('90) and Lendenfeld ('02) take some account of embayments, but are silent as to unconformable contacts.

As above stated, Walther's description of the reefs of Sinai peninsula is exceptional in making explicit mention of the unconformable reef contacts with tilted and eroded strata ('88) ; equally exceptional are Vaughan's statements in several recent articles, to the effect that unconformities exist between reef limestones and their foundations in Florida and the West Indies, and Foye's description of unconformable reefs in Fiji ('17). However, in the nearly thirty years between Walther's early report and these later studies, a number of geologists conducting structural investigations in the East Indies, without special regard to coral reefs, have recognised the unconformable contact between elevated reefs and their foundations, as has already been briefly noted.

In view of the prevailing silence as to embayments and unconformities among observers of coral reefs, it is perhaps not unnatural that the authors of text-books on geology and physical geography should say nothing as to the occurrence of these important structural features of fringing, barrier, and elevated reefs ; though it does seem strange that they should not call attention to the theoretical value of such features in the discussion of competing theories, inasmuch as both embayments and unconformities are easily shown by deduction to be essential consequences of Darwin's theory, and to be impossible consequences of all coral-reef theories which postulate a constant relation of land and sea level.

Yet, as a matter of fact, with the three exceptions noted below, neither the observational occurrence nor the theoretical expectation of these features is mentioned in the chapters or paragraphs devoted to coral reefs in Lyell's *Principles of Geology* (1872), Peschel-Leipoldt's *Physische Erdkunde* (1879), Hann, Hochstetter, and Pokorny's *Allgemeine Erdkunde* (1881), Green's *Physical Geology* (1882), Hahn's *Inselstudien* (1883), Günther's *Lehrbuch der Geophysik* (1885), Phillip's *Manual of Geology* (1885), Neumayr's *Erdgeschichte* (1886), Prestwich's *Geology* (1886), Richtofen's *Führer für Forschungsreisende* (1886), Suess'

Antlitz der Erde (vol. ii. 1889), Jukes-Brown's *Handbook of Physical Geology* (1892), Bonney's *Story of our Planet* (1893), Geikie's *Text-book of Geology* (1893), Kayser's *Lehrbuch der allgemeinen Geologie* (1893), Walther's *Allgemeine Meereskunde* (1893) and *Einleitung in die Geologie* (1893), Penck's *Morphologie der Erdoberfläche* (1894; embayments are briefly considered), Leconte's *Elements of Geology* (1895), Supan's *Grundzüge der physischen Erdkunde* (1896), Credner's *Elemente der Geologie* (1897), Scott's *Introduction to Geology* (1897), Toulet's *Ocean* (1904), de Lapparent's *Traité de Géologie* (1905), Haug's *Traité de Géologie* (1907), Wagner's *Lehrbuch der Geographie* (1908), de Martonne's *Traité de Géographie Physique* (1909), Tarr and Martin's *College Physiography* (1914), Pirsson and Schuchert's *Text-book of Geology* (1915; embayments are very briefly mentioned), or Lake's *Physical Geography* (1915; embayments are considered).

Instability of Scientific Opinion.—It is interesting to review certain articles cited above, which appeared shortly after Murray had announced his still-stand theory of reef formation, whereby Rein's previously announced theory of atolls was brought into prominence. In spite of the incomplete analysis of the problem then presented, the mere possibility of the formation of atolls by upgrowth from stationary submarine banks proved singularly disconcerting to many persons who had before accepted Darwin's theory of subsidence, as if it were the only possible theory. It was as if they said:—"Another method of making atolls has now been proposed; therefore the one we have hitherto accepted is wrong." Such persons would have been less disposed to abandon their former belief if they had given due consideration to the evidence, which Darwin pointed out for the subsidence theory, that is found in its capacity to bring various kinds of reefs—fringing, barrier, and atoll—into reasonable correlation, for the Rein-Murray theory is deficient in this respect: still less would they have been disposed to abandon Darwin's theory if they had perceived that it accounts for the structural features associated with fringing and barrier reefs—namely, the embayments of the shorelines that they border, and the unconformable contact of their limestones on eroded foundations—while the Rein-Murray theory accounts only for atolls.

Furthermore, the unchanging relation of land and sea-level

postulated in the Rein-Murray theory, makes embayed shorelines and unconformable reef contacts impossible. Singularly enough, few, if any, careful attempts were made to deduce the essential consequences of the rival theories, with a view of impartially selecting that one as the most probably correct which yielded peculiar consequences corresponding to previously unnoticed or unconsidered facts. Indeed, the searching deduction of contrasted consequences from the various theories of coral reefs as a means of testing their validity has been most unwarrantably neglected.

Scientific opinion regarding the origin of coral reefs thus seems to have been more largely guided in the later years of the nineteenth century by subjective preference than by objective logic. The occurrence of subsidence was very generally denied, and Darwin's theory was repeatedly rejected by geologists who had previously accepted it, because it had been suggested that atolls might possibly be formed without subsidence. If fringing, barrier, and elevated reefs had also been duly considered, defection from the theory of intermittent subsidence would surely have been less general.

It is not for a moment intended that other theories of coral reefs than Darwin's should not deserve cordial consideration; for an investigator has no more manifest duty than to hold his mind open for the invention and reception of new solutions of old problems, however convincingly an already adopted solution may appeal to him. But the mere announcement of a new suggestion is not enough; the suggestion should be subjected to rigorous examination and verification before an explanation, previously accepted on good grounds, is given up and the new one is accepted. The fact that between 1880 and 1900 the Rein-Murray theory of atoll formation was very generally adopted without any adequate verification shows not only that scientific opinion was too readily turned toward a new idea, but also that the previous belief in the theory of subsidence must have been held without the warrant of independent verification. When the history of science in the nineteenth century is written, it is to be feared that the chapter on the origin of coral reefs will not be flattering.

Verification of Coral-reef Theories.—Geology was a young science and its methods were imperfectly developed in 1838 when Darwin first brought forward his theory; the theory seems

to have gained acceptance more because of its simplicity and apparent sufficiency than because of the verification which its capacity to correlate various kinds of reefs provided for it. Geology was a more exacting science in 1880, and yet in the following twenty years many a geologist seems to have been indifferent, as far as the origin of coral reefs is concerned, to the need of the kind of verification for his beliefs that comes from the success of the unforeseen consequences of a theory in matching previously unobserved or uncorrelated facts ; for it is otherwise impossible to explain the inattention to the verification provided by the shoreline embayments of reef-encircled islands and reef-bordered coasts ; and to the further and larger verification provided by the unconformable contacts of reef limestones on their foundations.

Principles so well established and so pertinent as those involved in the embayments of shorelines and in the unconformable contact of marine limestones upon foundations that have suffered subaerial erosion, should no longer be overlooked in the indoor discussion of coral reefs, and much less in their outdoor observation. Reports and text-books which omit the consideration of these principles from the pages in which coral reefs are treated must be regarded as still representing the nineteenth-century phase of the discussion, and as having failed to reach the twentieth-century phase.

The Reefs of the Seychelles.—The reefs of the Seychelles Islands in the southern Indian Ocean may be briefly considered as a concrete illustration of the importance of embayed shorelines and unconformable contacts in the coral-reef problem. The central members of this remarkable group are small granitic islands which rise in mountain-like forms from a vast submarine bank, 200 by 80 miles across, with a maximum depth of forty fathoms near its centre. The margin of the bank is somewhat shallower than the centre, especially on its northern side, where several atolls reach the sea surface. The largest of the granitic islands is Mahé, 17 miles in length and 2,993 ft. in height ; its shoreline is indented with well-marked embayments, which are separated by tapering points fringed with narrow reefs. Keller [1] and Chun [2] have reported the occurrence of an elevated reef 80 ft. above sea-level. The same authors, and Gardiner also,

[1] C. Keller, *Die Ostafrikanischen Inseln*, Berlin, 1898 ; see p. 158.
[2] C. Chun, *Aus den Tiefen des Weltmeeres*, Jena, 1900 ; see p. 426.

give excellent views of the tapering points and re-entrant embayments of the coast. It may be safely assumed that both the sea-level and the elevated reefs are unconformable on the granitic spurs that slope down between the embayments ; for granite is not a surface-made rock ; and even if it were, its present spur and valley form could not be its original form ; but no description of the island that I have found makes explicit mention of these significant inferences.

When the deduced consequences of various coral-reef theories are confronted with the facts regarding the Seychelles reefs, it becomes plain that no theory can be accepted that does not involve submergence preceding or during reef formation, as well as long-continued erosion before submergence. Whether the sea-level reef has been formed directly after the uplift by which the elevated reef gained its present altitude, or whether the uplift has been followed by a new subsidence cannot be determined from the scanty records now available ; but in any case strong submergence, explicable only by subsidence, must have preceded the formation of the elevated reef. Hence all so-called still-stand theories are excluded from application here.

Furthermore, the features of the Seychelles include three conditioning factors : the extraordinary area of the vast submarine bank, the great volume and long duration of erosion implied by the unconformity of the reefs on their sloping granitic foundations, and the special changes of level involved in the formation of the vast bank and the two fringing reefs. The changes of level thus demanded are manifestly unlike the changes recorded on other reef-encircled islands in the Indian and Pacific Oceans ; hence these factors cannot be satisfied by any theory of coral reefs which postulates stationary reef foundations in an ocean that rises and falls everywhere by the same amount. The vast area of the submarine bank, and the great volume of erosion before the sea-level and the elevated fringing reefs were formed, both demand a longer period of emergence than can be provided by the Glacial-control theory ; moreover, the granitic islands of the Seychelles group are not cliff-rimmed, as they should be if the submarine bank were the work of abrasion by the lowered and chilled ocean, as the Glacial-control theory assumes.

Again, the narrow sea-level fringing reef around Mahé is so unlike the three-mile reef plain adjoining Rodriguez island

in the same ocean that, although both must have been formed after the submergence by which the valleys of the two islands were embayed, it seems impossible that both should have been formed in the same period of time ; Rodriguez must have been submerged before Mahé ; hence the submergence preceding the formation of the Rodriguez reef and the submergence preceding the formation of the Mahé reefs do not appear to have been caused by a single, uniform, and universal rise of ocean level. Conversely, the 80-ft. emergence of the elevated reef on Mahé, the 1,200-ft. emergence of Christmas Island in the eastern Indian Ocean, and the absence of all signs of emergence on many reef-encircled volcanic islands of the Pacific, cannot be explained by a fall of ocean level everywhere of the same amount. Likewise, the elevated but undissected Loyalty atolls, at altitudes of 200 or 300 ft., and the elevated reefs in Fiji at various altitudes up to 1,030 ft., partly or largely dissected, cannot be explained by a fall of ocean level, everywhere of the same amount and date.

All the difficulties that thus embarrass the theories which regard reef-encircled islands and reef-bordered continental coasts as stationary, while the ocean rises or falls around them, vanish if the islands and coasts which serve as foundations for reef growth are supposed to be subject to such movements as are postulated in Darwin's theory ; that is, to slow or intermittent movements of elevation and of subsidence, which vary in place, amount, rate, and date, and which are frequently or generally greater in measure and more rapid in rate than fluctuations of ocean level ; not that no such fluctuations occur, but that the many indications of submergences and emergences, varying in place, date, and amount, demand local movements of the submerged or emerged land or island, and not universal and equable changes of ocean level for their explanation. The chief value of such universal changes of level, especially of those caused by the climatic variations of the Glacial period, lies in their modification of the amount of submergence or emergence due to local movements of subsidence or elevation. This aspect of the problem has been elsewhere discussed.[1]

Darwin's Theory of Intermittent Subsidence.—If reef foundations are regarded as unstable, lateral reef growth will take

[1] "Problems Associated with the Study of Coral Reefs," *Scient. Monthly*, ii. 1916; see p. 565.

place during stationary periods, as Darwin clearly stated, and upward reef growth will be favoured during and for some time after movements of subsidence, as he stated more clearly still; movements of elevation will interrupt reef growth, as he also recognised. Movements of subsidence have naturally given name to Darwin's theory, for only through their aid can the transformation of fringing reefs into unconformable barrier reefs, and of barrier reefs into atolls, be satisfactorily accounted for; but his theory explicitly recognised the occurrence of stationary periods and of movements of elevation as well as movements of subsidence: indeed, the emphasis that he gave to the alternation of long stationary periods with brief movements of subsidence suggests that his scheme should be called the theory of intermittent subsidence.

Only under the diverse conditions of this elastic theory can the diversity of the structural features of various reef-encircled islands and reef-bordered coasts be adequately explained. The main postulate of the theory, namely, the instability of reef foundations, is well grounded; for every island group in the Pacific, the geological history of which has been well worked out, is shown to have suffered various changes of level which clearly demand the occurrence of local movements, differing in date and amount from the local movements of other groups. The opinion reached by Schuchert in a careful study of the geological changes of land and sea in Oceanica deserves quotation in this connection: "The entire western half of the Pacific bottom, and especially the Australian region, appears to be as mobile as any of the continents of the northern hemisphere, with the difference that the sum of the continental movements is upwards, while that of the ocean bottoms is downwards."[1] The distribution of animals on oceanic islands also calls for changes, and particularly for subsidence, in the ocean floor; land snails are especially significant in this respect, as shown by the studies of Pilsbry[2] and Crampton.[3] In view of all this, it would seem as if the burden of proof

[1] C. Schuchert, "The Problem of Continental Fracturing and Diastrophism in Oceanica," *Amer. Journ. Sci.* xlii. 1916, 91–105; see p. 105.

[2] H. A. Pilsbry, "Mid-Pacific Land Snail Faunas," *Proc. Nat. Acad. Sci.* ii. 1916, 429–433. This is a concise statement; the subject is elaborately treated in his *Manual of Conchology*.

[3] H. E. Crampton, *Studies in the Variation, Distribution, and Evolution of the Genus Partula*, Carnegie Inst., Washington, 1916; see p. 12.

lay upon those who assume that reef foundations have not subsided.

The Origin of Atolls.—The problem of sea-level atolls is, as above noted, different from that of fringing, barrier, and elevated reefs, inasmuch as the geological history of atolls cannot be made out by surface observation. True, the foundations of the atolls in the northern part of the Seychelles group must in all probability have suffered movements very similar to those suffered by Mahé and the other granitic islands. Similarly, the atolls of the Fiji group can hardly have had stationary foundations while the neighbouring volcanic islands suffered the varied and diverse changes of level that are testified to by their many kinds of reefs; for the reefs of this most interesting group include, besides a number of typical atolls, many narrow and unconformable sea-level fringes, unconformable sea-level barriers at various distances from their embayed islands, and little dissected and much dissected elevated reefs at different altitudes, most of which are strongly unconformable on their foundations.

Again, the atoll foundations in the Society group must have shared in the movements of subsidence by which the neighbouring volcanic islands have been deeply embayed. But the central Pacific archipelagoes, consisting of atolls only, must long remain objects of surmise, except in so far as the evidence for subsidence given by the Funafuti boring[1] is held to apply to other atolls; for, as far as the visible features of those atolls are concerned, they can be explained by any one of several theories, provided the postulates and the processes of the theories are accepted.

The Glacial-control Theory.—For example, if it be assumed that the ocean bottom in the region of atoll archipelagoes has long remained fixed, then the changes of ocean level and temperature, assumed to have been caused by the climatic changes of the Glacial period, together with the alternation of reef growth and extinction that has been assumed to accompany these oceanic changes, will suffice to account for the superficial features of atolls as they are now observed; but the absence of clift shores on the central islands of fringing and barrier reefs in other parts of the Pacific compels me, as I have else-

[1] The best summary of this evidence is given by E. W. Skeats, *Amer. Journ. Sci.* xlv. 1918, 81–90.

where shown, to reject the assumption that the lowered temperature of the Glacial ocean sufficed to extinguish reef growth and permit the abrasion of Pre-glacial islands; and the mobility that is found to characterise the ocean floor beneath all archipelagoes of decipherable history leads me to question very seriously the long-continued stability of the ocean floor in the regions of atolls.

The most that can, to my belief, be said for the Glacial-control theory is that the fluctuations of ocean level, which it properly postulates, have combined with the elevations and subsidences of reef foundations in such a way as sometimes to favour reef submergence, sometimes reef upgrowth, sometimes reef outgrowth, sometimes reef emergence; and more particularly that the Glacial rise of ocean level has co-operated with widely prevailing movements of subsidence in such a way as to cause an almost universal submergence of reef foundations in recent geological time, as a result of which nearly all present-day reefs are of narrow or immature form, and very few are so far broadened as to deserve the name of mature reef plains. It is possible, however, that the theory has fuller application along the border of the coral zone.

The upshot of all this is that, wherever opportunity is given to make a critical test of the conditions under which coral reefs have been formed, a local subsidence of their foundation, as testified by the embayment of the adjoining coasts and the unconformity of the reef limestones on the underlying rocks, appears to have been a determining condition of their construction. It is for this reason that I have come to regard Darwin's theory of coral reefs as much more competent than any other.

Molengraaff's Theory of Subsiding Volcanic Islands.—Darwin was led by his theory of the subsidence of reef foundations to assume that subsidence has recently prevailed over large areas of the Pacific and Indian Oceans. It has been urged that, if such wide-spread recent subsidence had prevailed, the continental coasts should show features of emergence, whereas they frequently show features of submergence. A way out of this difficulty has lately been opened by Molengraaff, who suggests that the subsidence, in response to which reef upgrowth has taken place, need not be a wide-spread movement of the ocean floor, but a local sinking of volcanic islands by reason of their

excessive weight.[1] Whether this cause of the sinking of volcanic cones is true or not, it is noteworthy that if the subsidence of reef foundations is thus locally restricted, it may be accompanied by a small rise of ocean level, as compared with the time before the volcanic islands were built up.[2] Molengraaff's idea may therefore be regarded as a valuable supplement to Darwin's theory of coral reefs. It is, however, not intended to exclude deformation of the ocean bottom as a cause of change of levels; for such deformation is highly probable near the Tonga islands, and is abundantly proved for the Australasian archipelagoes.

[1] G. A. F. Molengraaff, "The Coral Reef Problem and Isostasy," *Proc. Akad. Wet. Amsterdam*, xix. 1917, 610-627.

[2] "The Isostatic Subsidence of Volcanic Islands," *Proc. Nat. Acad. Sci.* iii. 1917, 649-654.

SCIENCE AND THE INTERNATIONAL LANGUAGE

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One of the most important problems of reconstruction will be the selection of the " third language " or " lingua franca." In a recent letter to *Nature*[1] on " International Latin," Dr. Caspari has proved an able advocate of Latin; whereas in a review by the same journal of Gilbert H. Richardson's *Declaration concerning the need for Standardising an Auxiliary International Language* it is assumed that this urgent need can only be met by Esperanto or Ido, or some similar language drawn up for the purpose. This divergence of opinion is so extreme that an impartial comparison of proposed international languages would seem desirable. The present time seems opportune also because a correspondence initiated by Burdon Sanderson has recently taken place (*Times Lit. Suppl.* May 23—July 15, 1918) in which the claims of Latin as a universal language for all purposes were strongly urged, but did not meet with unanimous acceptance even among Latin scholars. It seems probable, however, that the use of Latin for scientific purposes only, stands a greater chance of being accepted as a practical solution of what is perhaps a greater need. In the first place the scientific public has, at the lowest estimate, a standard of education higher than that of the wider public for whom a universal language would be provided. This education usually includes a grounding in Latin sufficient for future acquirements. Then again, the need for a medium of communication between scientists of different nationalities is obvious; no less so, unfortunately, the difficulties in the way of making a suitable choice. It seems probable that national rivalries, accentuated as between the belligerent nations, will prevent the adoption of any living language by

[1] *Nature*, xcvii. 81 (1918).

common consent. And the rival claims of the new synthetic languages are no less, but even more, keen and irreconcilable than those of national languages. A suitable historic language could be accepted by all parties without any appearance of yielding the pride of place.

It therefore seems desirable to take up the matter where it was left by Dr. Caspari, to state his and other arguments with somewhat more detail and to give such definite examples as may form a basis for discussion and criticism.

The advantages offered by the synthetic languages Volapuk, Esperanto and Ido should of course be considered most carefully. The greatest undoubtedly is the ease and certainty with which they can be learnt, due to their logical and consistent grammars. In the case of Ido the roots are derived in suitable proportion from all important linguistic stocks, so that it shall not appear utterly unfamiliar even to the smaller nations. On the other hand, these languages appear most colourless to those who like, and are even assisted in the expression of their thought by, the historical associations of a real language which grew up to meet the needs of humanity.

Many such language lovers are to be found among professional scientists. Latin is peculiarly rich in such associations. The preliminary difficulties of Latin are largely overcome by its actual and probable retention as an important part of school curricula on other grounds. It is not too much to expect that, with improved methods, they should be much more largely overcome than at present. Even now it is claimed by at least one school in England that Latin has been used for many years in this school as a medium of ordinary intercourse at stated periods (*Times Lit. Suppl.* June 6, 1918). This familiarity with Latin, arising from its widely diffused use as a school subject, is probably far more intimate than that which would be brought about by a judicious selection of roots, as in Ido.

In the case of the Romance languages in Europe and America the element of familiarity is of course already present without any formal education. The adoption of Latin would naturally meet with a particularly enthusiastic reception from Italy, and it should seem peculiarly fitting, both to present admirers of Italy and to historians, that her ancient language and that of the founders of Western civilisation should regain its long-lost position, at any rate in the realms of pure and

applied science, destined as these are to include an ever-increasing proportion of national activities. Italy has shown herself second to no nation, one might even say pre-eminent, in the recognition and encouragement of science. For example, at the Jubilee of Lord Kelvin's appointment to the professorship of natural philosophy at Glasgow University, an occasion spontaneously recognised as one of international importance, and attended by delegates from the Universities, scientific societies and Academies of practically all civilised nations, it was remarked that the only nation represented by a State official was Italy. " At the Kelvin Jubilee official recognition was studiously withheld. The King of Italy could send an ambassador. But Italy is one of those nations where science is honoured for its own sake" (*Saturday Review*). For the sake of brevity nothing has been said as to the claims of Greek, for it will be admitted probably by a large majority that the balance of practical convenience is most clearly on the side of Latin.

The suitability of Latin as a medium for expressing the ideas of most sciences in their early stages may be abundantly illustrated, *e.g.* from the works of Copernicus, Newton, Leibnitz, the two Bacons, Linnaeus, Harvey, Gilbert, and Napier of Merchiston. As was remarked by Prof. F. Granger in the *Times* correspondence mentioned above, " Science in the pages of Descartes and Newton moved easily in a Latin dress, the materials of which were derived from a still spoken but un-Ciceronian tongue." It seems probable that a style modelled upon one or other of these writers will suffice to express even the more complex and specialised modes of thought which have developed in modern science. A difficulty strongly felt by some in connection with Latin is that due to the formal, and to our ideas unnatural, arrangement of words in Ciceronian Latin. This point, however, has already been dealt with by Prof. Granger, who shows how much nearer to the modern idiom is the language of the Vulgate, which arose from the spoken language. The periodic style would no doubt have its uses at international gatherings of a formal nature, and modern precedents for these occasions are of course available, for example in the Public Orations of Sir J. E. Sandys.

The number of inflexions and the prevalence of exceptions are indeed the features of Latin which are likely to prove most

annoying to such scientists as are disinclined to give more than a minimum of thought to the medium of communication. It is obvious of course that no new exceptional forms would be introduced, although it seems likely that the old would have to be retained in the case of words in constant use, such as the verb "sum," the noun "vis," or the pronoun "hic." This seems necessary in order to preserve the continuity of Latin and its similarity to the language of the medieval scientists. But all rarer exceptions might either retain these forms or be inflected according to a standard type, at the option of the writer. In the case of new nouns, the number of declensions might easily be reduced to three, as in the scheme described below; and the conjugations might also be reduced to two, as in the case of new French verbs.

Some of the new words already coined could no doubt be made more self-explanatory by a free use of the facilities of Latin. For example "atomplexitas" instead of "atomicitas." A new word is wanted for "molecule" which shall not be too long to be introduced into compound words.

The allocation of Latin words to express such ideas as "potential" in its various senses would require of course the most careful consideration by experts familiar both with the scientific ideas involved and with Latin philology. The forms and idioms of pharmaceutical chemistry would furnish many useful precedents for chemistry. The question of the nomenclature of salts suggests itself—should the basic and the acidic part be related as noun and adjective, as bismuthum nitratum, or as bismuthi nitratum "the nitrate"? To oxides the termination "a" might uniformly be added; thus not only "strontia, thoria," but also "calcia." The word "calx" itself might have the more general meaning which is already familiar in historical chemistry. The termination would, how ever, have to be retained with a different meaning in "ammonia" unless this were changed to "amine." On looking through the names of the elements it seems that a thorough latinisation might lead to a greater consistency. The termination "um" would of course be retained for the "metallic" elements natrium, stibium, as well as hydrogenium. The elements of intermediate character, boron, silicon, would remain as at present, and, with arsenicon, selenion, would be third-declension nouns on the model of carbo-onis. It is not

unsuitable that the elements of the zero group should find place here with the alteration, good on philological grounds, of "helium" to "helion." More alteration would be required in the more electronegative elements, for which the termination "us" might be suitable. Thus "phosphorus" would be associated with "nitrogenus" (the closest analogy in Latin being such words as Trojugena). To these would be added "oxygenus," and the "halogenus" group "fluorus, chlorus," etc., which would form adjectives "chloridus, chloratus," etc., or nouns "chloridum," etc., according to the convention chosen. The classical word "sulfur-is" might have to remain as an exception.

The following specimens, almost literally translated from plain descriptive chemistry, are presented, not as models of style, but as showing how freely such familiar Latin could be read by chemists who retain a few shreds of school Latin and have the occasional aid of a dictionary.

Properties of fused Silica.	*Silicæ propria vitrosæ.*
It does not crack on subjection to the most violent and sudden changes of temperature.	Non rimatur etiam per maximam aut subitam temperaturæ mutationem.
It is unattacked by the volatile acids with the exception of hydrofluoric acid.	Nullo aceto volatili corroditur excepto hydrofluorico.
It has a melting-point approximately equal to that of platinum.	Punctum liquefactionis pæne idem est ac platini.
It is harder than ordinary glass.	Durior est quam vitrum commune.
Above 1000° C. it is permeable to hydrogen and certain other gases.	Supra 1000° C. permeabilis est hydrogenio et quibusdam aliis æribus.
Its coefficient of expansion is $0{\cdot}0_{6}59$ per degree centigrade (about $\frac{1}{17}$ of that of platinum).	Expansio valet $0{\cdot}0_{6}59t°$ C. (id est, circa septimam decimam partem expansionis platini).
Its expansion up to 1000° C. is regular; above 1200° C. it contracts.	Usque ad 1000° C. æqualiter expanditur, supra 1200° C. contrahitur.
As far as is at present known, it shows no tendency to devitrification.	Quoad adhuc compertum est nulla devitrificatio apparet.
Its density is 2·2 (approximately).	Densitas valet fere 2·2.
This hydrocarbon is apparently identical or isomeric with that obtained by Liebermann . . . by reduction of the δ-pimaric acid of Bordeaux colophony with hydriodic acid. It was suggested by Liebermann, who assigned to it the formula $C_{20}H_{32}$ or $C_{20}H_{34}$, that it was probably identical with the colophene (or colophene dihydride) obtained by Deville by the action of concentrated sulphuric acid upon turpentine.	Hoc hydrocarbon, ut videtur, aut idem est ac illud quod Liebermannus confecit per hydrogenationem aceti δ-pimarici ex colophonio (Gallico) cum aceto hydriodico, aut cum illo isomericum. Putabat Liebermannus, qui ei formulam $C_{20}H_{32}$ vel $C_{20}H_{34}$ assignaverit, fieri posse ut idem fuerit ac colophen illud, vel colophen dihydridum, quod Devillius confecit per interactionem aceti sulfurici fortis et terebinthi.

CORRESPONDENCE

To the Editor of "Science Progress"

PROFESSOR ELLIOT SMITH'S VIEWS ON FLINT IMPLEMENTS

From J. Reid Moir

Dear Sir,—In Science Progress for October (pp. 335–336) appears a short review of Prof. Elliot Smith's lately-published book *Primitive Man* (London: Oxford University Press). This Review, while giving a very excellent, general account of the work under discussion, does not, however, make any reference to the Professor's views on flint implements. But these views appear to me to be of such an unusual order, that I feel it to be necessary to comment upon them.

Prof. Elliot Smith states on p. 28 of his book that the first man to produce an edge on a stone by deliberate chipping probably found that he had a great difficulty in convincing his associates to adopt such a procedure, and that, in all likelihood, his experience "was similar to Galileo's, Watt's, and Lister's," and while not disputing the soundness of this conclusion, it occurs to me, since reading the Professor's views on flint flaking and flint implements, that many of us modern flakers of flint have similar difficulties to overcome.

It is clear, in the first place, that Prof. Elliot Smith regards the earliest Chellian palæoliths as owing their existence to "special creation" as it were, and not to any slow evolutionary progress in the methods of flint flaking. And such a view appears in strong contrast to the many emphatic protests which the Professor registers against those who, for instance, argue that races of people living upon two sides of the Pacific evolved independently "a winged dragon with deer's horns." Prof. Elliot Smith states (p. 34) that if this is so we must postulate "a highly specialised human instinct to dream dragons," and I agree. But on the other hand I am unaware of any reasons

for believing that human beings in the remote past possessed a highly specialised instinct to dream Chellian palæoliths.

On p. 27 the Professor states that the *first man* (the italics are mine) to deliberately give an edge to a flint by chipping in "a particular way" was perhaps reproved by the "palæolithic argument that his predecessors found unchipped stones good enough for them, and it was therefore extremely foolish to attempt to supersede methods which experience had shown to be so thoroughly efficient" (there would appear to be some slight confusion of thought here, as if the man's associates and predecessors had only used unchipped stones, which are not generally classed as palæoliths, the "arguments" put forward could not well be of a palæolithic, but rather of a proto-eolithic order). On p. 30 Prof. Elliot Smith has advanced a step further, and now states that "there cannot be any doubt that the Chellian inventor was probably regarded by his fellow men as a crazy visionary," and that he met with much opposition "before he was able to convince them that his method of chipping flint was a real advance upon their eolithic crudities." Now, as one who, after much experience in flaking flints, has succeeded in making implements of the Chellian type, I regard it as in the last degree improbable that any primitive man, who, on Prof. Elliot Smith's own showing, had been in the habit of using only unchipped stones (one rather fails to see to what sort of real use such "edgeless" specimens could be put) should suddenly, without any prior knowledge of flint flaking, have a remarkable inspiration, and proceed to make a Chellian palæolith. Such a view runs counter to all experience, and I would as soon believe that a man could make a bicycle without any prior experience in its manufacture. But if Prof. Elliot Smith is himself unfamiliar with the flaking of flint, and thus, in this particular matter, on a par with the primitive person who had been using only unchipped stones, let him take a flint nodule and a hammer-stone and see if he can make a palæolithic implement. Even though the Professor would have this advantage, that he had seen a Chellian artefact while the primitive person had not, I am nevertheless quite certain that such an experience would modify profoundly his opinions regarding the origin of the earliest pointed and ovate palæoliths. Further it is common knowledge that there is in existence very good evidence to show that the Chellian specimens

were almost certainly the outcome of an orderly progress in flint-implement making. This evidence takes the form of definite flint implements which can be seen and handled, and which by no stretch of imagination can be described as "unchipped stones," or "crudities" of any sort or kind.

On p. 27 of his book the Professor states that "there are scores of ways of chipping a stone implement," by which, as one does not chip an "implement," I suppose he means that there are scores of ways in which a stone may be chipped in producing an implement from it. I had no idea that there were so many ways of "chipping" a stone, and Prof. Elliot Smith would be doing a service to science in describing each method in detail. Then again in the footnote to p. 27 I notice it is stated that "it is at least as simple, if not definitely easier, to shape an implement by rubbing and polishing" as by flaking. I regret to say, however, that my experience with flints is dead against any such conclusion, but perhaps the Professor has by experiment proved the truth of his contention.

On p. 18 we are told that "the great cultural break in Western Europe itself (and even in its flint work) did not fall between the so-called Palæolithic and Neolithic ages, but between the Lower and Upper Palæolithic periods," and again that "there is a much closer kinship between the flint-work of the so-called Upper Palæolithic and the Neolithic ages than there is between the former and that of the Lower Palæolithic period." Here once more I find myself in opposition to Prof. Elliot Smith. If there was any "break" in the manner of flaking flints in the past it certainly occurred at the end of Acheulean times, as the technique of this period is fundamentally different from that of the succeeding Mousterian phase. The angle at which Mousterian man delivered his blows upon the flint he was shaping was quite different from that which the Acheuleans favoured, as is clear when a series of the two types of implements is examined. And there are other well-marked differences between the technique of the Acheulean and Mousterian craftsmen.

Again, I think that there can be no doubt the forms of the implements of Upper Mousterian times merge gradually into those of the Lower Aurignacian.

These are some of the principal points upon which I disagree with Prof. Elliot Smith, and I am of opinion that had he made

himself really familiar with the subject of flint fracture (as all who write authoritatively on flint implements ought to do), he would not have written as he has done. It cannot be too often emphasised that the study of flint implements is as complex and difficult as any other scientific subject, and that it is regrettable when men with deservedly great reputations fail to realise this fact.

Yours faithfully,
J. Reid Moir.

THE DISCOVERY OF NEW PARTS IN ARCHÆOPTERYX

From Dr. B. Petronievics

Dear Sir,—In his review on the recent advances of Palæontology in Science Progress, January 1918, Mr. W. P. Pycraft seems to ascribe the discovery of the new parts in the British Museum specimen of Archæopteryx to Dr. Smith Woodward. Although I am indebted to Dr. Woodward for the permission of the new preparations, the discovery itself is wholly mine, as is evident from the following " Introductory " to the article reviewed by Mr. Pycraft (*Proc. Zool. Soc.* April 1917), which was approved by Dr. Woodward himself.

" Introductory

" Some further preparation of the British Museum specimen of Archæopteryx recently done by Mr. F. O. Barlow, under the direction of myself and Dr. Woodward, has resulted in the complete uncovering of the right coracoid bone and the discovery of the pubes. The work was undertaken at my request after I had convinced myself, by a prolonged study of this specimen in connection with Dames' two memoirs on the Berlin Archæopteryx, that the bones in question must exist buried in the limestone matrix. On the results I have prepared an exhaustive paper, which I hope to publish later; but the following preliminary notice, written by Dr. Woodward, who has made use of our joint observations, gives a general account of the important additions to our knowledge of the pectoral and pelvic arches of this primitive Jurassic bird which are now made possible. Some differences of opinion between us are indicated in footnotes.

"I have to thank Dr. Woodward and also Dr. C. W. Andrews for the valuable help and advice they have given to me.—B. P."

Being no palæontologist by profession (which does not mean that I am ignorant in palæontological matters) and not having been able to write English at that time (1916), I invited Dr. Woodward to make a joint-communication on the discovery, and was glad that the eminent palæontologist accepted it.

Yours faithfully,

B. PETRONIEVICS.

NOTES

DEATH IS EVERYWHERE

DEATH, death is everywhere. Where'er we tread,
We stir the dust of myriads long since dead.
Our food and drink, of life the staff and stay,
Are but the finished products of decay.
Our marble halls, with all their quarried stones—
What are they but the bleached and whitened bones
Of countless generations, piled on high,
Hecatomb upon hecatomb to the sky,
Barrows the dead have built themselves ?—The Earth
Is one o'erflowing cemetery,—its girth
One seamless zone of graves. There's not a rod
Upon its surface that cries not to God
For some life sacrificed. We mortals are
But mites and midges on a moss-grown star,
Frail ephemerides that breed and crawl
Among the middens of this festering ball.

CLOUDESLEY BRERETON.

The Organisation of the Women's Vote (Lady W. Cowan)

For the first time in the history of our country duly qualified women are in a position to vote and to influence, perhaps decisively, the result of a General Election.

Since women have realised their new political responsibilities there has been a widespread demand for special education, and many organisations have been catering for this appetite for political information, and new combinations for the preparation of women as citizens on non-party lines have been formed all over the country, with special speakers delivering lectures on selected subjects.

Careful observers have remarked that, generally speaking, the political sympathies of these new voters favour a Coalition rather than party Government; a Government prepared to

see that the Peace terms are framed in the same spirit as the victory was won and the armistice signed.

Women realise that in the reconstruction period which must follow the war the country requires the Ship of State to be steered by men and women representing and co-ordinating the best, irrespective of the present lines of political cleavage, and maintain that the problems of Peace demand a union of ideas, imagination, and courage just as much as those of War.

Women are not showing any alacrity to swarm into the party hives, nor are they sipping the party honey with the avidity which some of the wire-pullers might desire. Rather do they show a disposition to hear the great questions of the hour discussed on wide lines of patriotism, and demand that those who have sacrificed so much should be secured against the perpetuation of the evils and injustices of pre-war days, and that the best conditions of work and of living should be realised for all those who help to produce the wealth of the State. Questions affecting the family as a unit, questions of housing, health, and wages, should be dealt with, not on party lines, nor in the interests of wire-pullers and place-hunters, but by those who are humanitarian in principle and practical citizens of a world-wide Empire, and have at heart the greatest good of the greatest number from a national and imperial point of view.

Women desire to see representative Government purged of the poison of party prejudice, and more actively giving voice to the aspirations and hopes of a Democracy which realises its freedom in the laws passed by a representative House of Commons.

While appreciating the benefits of an informed, educated, and critical Opposition, women are beginning to see that an obstinate, pig-headed party blocking machine, calling itself an Opposition, is a travesty of representative Government, and is, so far as practical results are concerned, a mere dog in the manger. Women desire to see an Opposition drawn from all sections of the House of Commons; its members thinking, reasoning, and voting on the great questions of the day as intelligent representatives of a free people, and not as parts of the tied machine of a party caucus. It is in this spirit that the new women voters are shouldering their responsibilities, and it augurs well for the country that such earnestness of

purpose and loftiness of ideal should inspire their political action at the poll.

A New Monthly Botanical Journal (Marie C. Stopes, D.Sc.)

American botanists have decided to unite in the production of a journal to supplant the *Botanisches Centralblatt*, which has become difficult or impossible to obtain owing to the confusion of the war, and which, moreover, no longer meets with the unqualified support of the Allied countries.

The new journal sets out to be very comprehensive and international in character. The central editorial board includes the leading botanists of the United States, with representatives of the following branches:

Agronomy, Soil Technology, and Plant Production.
Bibliography, Biography, and History, J. H. Barnhart, New York Botanical Garden, New York City.
Bacteriology, H. J Conn, New York Agricultural Experiment Station, Geneva, N.Y.
Botanical Education, C. Stuart Gager, Brooklyn Botanic Garden, Brooklyn, N.Y.
Cytology, C. J. Chamberlain, The University of Chicago, Chicago, Ill.
Ecology and Plant Geography, H. C. Cowles, The University of Chicago, Chicago, Ill.
Forest Botany and Forestry, Raphael Zon, U.S. Forest Service, Washington, D.C.
Genetics, G. H. Shull, Princeton University, Princeton, N.J.
Horticulture, W. H. Chandler, Cornell University, Ithaca, N.Y.
Morphology, Anatomy, and Histology, E. W. Sinnott, Connecticut Agricultural College, Storrs, Conn.
Palæobotany and Evolutionary History, E. W. Berry, The Johns Hopkins University, Baltimore, Md.
Pathology, Donald Reddick, Cornell University, Ithaca, N.Y.
Pharmacognosy, Henry Kraemer, University of Michigan, Ann Arbor, Michigan.
Physiology, B. M. Duggar, Missouri Botanical Garden, St. Louis, Mo.
Taxonomy, J. M. Greenman, Missouri Botanical Garden, St. Louis, Mo., and J. G. Schramm, Cornell University, Ithaca, N.Y.

Foreign editors for each subject are also being arranged to cover the production of work in each of the branches.

The journal will be entitled *Botanical Abstracts*, and is designed to come out monthly, and to give as rapid publicity as possible to all papers which appear.

The journal begins with the year 1918, and the English representative is to be The Cambridge University Press.

Palæobotanical Papers

We are informed that all Palæobotanical papers published in the British Empire should be sent to Dr. Marie Stopes, University College, London, W.C., as soon as possible, to be entered and quoted in *Botanical Abstracts*, as she is the British editor for Palæobotany. Authors should send in references from page proof of forthcoming papers, so that publication of the abstracts can be really prompt.

British Scientific Instrument Research Association,

one of the earliest associations formed under the scheme of the Department of Scientific and Industrial Research, has secured premises at 26, Russell Square, W.C.1, where offices and research laboratories will be equipped. The first chairman of the Association was Mr. A. S. Esslemont, whose recent lamented death has been a severe loss to the Association. The Council have elected Mr. H. A. Colefax, K.C., as Chairman to fill the vacancy. The Vice-Chairman is Mr. Conrad Beck, C.B.E., to whose energy and personal influence is largely due the successful formation of the Association. Almost all the leading Optical and Scientific Instrument manufacturers are members. The Department of Scientific and Industrial Research is represented by Major C. J. Stewart, Captain F. O. Creagh-Osborne, R.N., C.B., Mr. S. W. Morrison, O.B.E., Colonel R. E. Home, D.S.O., R.A., and Mr. Percy Ashley. The Council have recently co-opted as members of their body the Hon. Sir Charles A. Parsons, F.R.S., and Prof. J. W. Nicholson, M.A., D.Sc., F.R.S. Sir Herbert Jackson, K.B.E., F.R.S., F.I.C., has been appointed Director of Research, and Mr. J. W. Williamson, B.Sc., Secretary of the Association.

British Science and Invention Exhibition

In view of the wide public interest taken in the British Scientific Products Exhibition, held at King's College, London, during the past summer, the British Science Guild has decided to organise another Exhibition next year. The main object of the Exhibition will be to stimulate national enterprise by a display of the year's progress in British science, invention, and industry. Further particulars of the Exhibition will be available in due course. A large part of the recent Exhibition has been transferred to Manchester, where it will be on view at the Municipal College of Technology.

Notes and News (D. O. W.)

The Nobel Prize in Physics for the year 1917 has been awarded to Prof. C. G. Barkla, Professor of Natural Philosophy in the University of Edinburgh. Prizes in Physics for 1918 and in Chemistry for 1917 and 1918 have been reserved.

The list of Royal Society Medallists for the year 1918 is as follows: The Copley Medal to Prof. H. A. Lorentz, For. Mem. R.S., for his distinguished researches in mathematical physics. The Rumford Medal to Prof. Charles Fabry and Dr. Alfred Pérot (jointly) for their contributions to optics. A Royal Medal to Prof. Alfred Fowler, F.R.S., for his researches in physical astronomy and spectroscopy. A Royal Medal to Prof. F. G. Hopkins, F.R.S., for his researches in chemical physiology. A Davy Medal to Prof. F. S. Kipping, F.R.S., for his studies in the camphor group and among organic derivatives of nitrogen and silicon. The Darwin Medal to Dr. H. F. Osborn for his researches on vertebrate morphology and palæontology. The Hughes Medal to Mr. Irving Langmuir for his researches in molecular physics.

Marshal Foch has been elected an honorary member of the French Academy of Sciences.

Dr. J. Horne has been elected President of the Royal Society of Edinburgh.

The Moxon Medal of the Royal College of Physicians has been awarded to Dr. F. W. Mott.

The David Livingstone Centenary Medal of the American Geographical Society has been awarded to Col. Candido Mariano da Silva Rondon in recognition of the merit of his explorations in South America.

We have noted with great regret the announcement during the past quarter of the death of the following distinguished men of science: Dr. H. Dyer, first Principal of the Imperial College of Engineering, Tokyo, Japan; Sir Edward Fry, G.C.B., F.R.S.; Prof. O. Henrici, F.R.S., Emeritus Professor of Mechanics and Mathematics at the City and Guilds Technical College; Dr. J. Harper Long, Professor of Chemistry at the North-Western University Medical School, Chicago, and one-time President of the American Chemical Society; Bishop Mitchinson, Master of Pembroke College, Oxford, a keen geologist; Rev. A. M. Norman, F.R.S.; Dr. R. Saundby, Emeritus Professor of Physic at Birmingham University; Sir Ratan Sata; Sir W. H. Thompson, K.B.E., scientific adviser to the Ministry of Food (drowned by the torpedoing of the *Leinster*); Prof. H. Shaler Williams, of Cornell University, geologist and palæontologist; Mon. C. J. E. Wolf, the distinguished French astronomer.

Prof. A. A. Michelson, Head of the Department of Physics in Chicago University, has received a commission as Lieut.-Commander in the U.S. Navy.

Prof. Maxwell-Lefroy has accepted a year's engagement with the Government of the Commonwealth of Australia for £3,000 plus £2,000 for experiments. He is to investigate the blowfly, the grain weevil, the woolly aphis, prickly pear, and St. John's wort.

At a meeting held at Huddersfield on August 21 the shareholders of British Dyes, Ltd., approved the scheme for amalgamation with Messrs. Levinstein, of Manchester.

According to *Science* (August 30) Yale University will receive, as residuary legatee of the late John W. Sturling, about $15,000,000, a sum which will nearly double the endowments of the university.

Nature states that the German Chemical Society has celebrated its jubilee by collecting a fund of 2,500,000 marks for the more extensive publication of chemical works of reference, such as *Beilstein*.

At the time of writing the constitution of the proposed Institute of Physics had not been finally settled by the Societies concerned in its formation (*i.e.* the Physical, Faraday, and Röntgen Societies). A guarantee fund of £1,000 is required before it can be definitely launched.

The Japanese Government is erecting, near Tokyo, a permanent laboratory for experimental work on nitrogen fixation. There are already two companies in that country engaged in the manufacture of calcium cyanamide, and it is reported that Dr. Takamine has secured for Japan the rights of the American modification of the Haber process.

By a unanimous vote at a general meeting of the Institute of Hygiene it was resolved to erase the names of all German and Austrian members from the register of the Institute. To commemorate the successful termination of the war, it is proposed to build a "Temple of Hygiene" in the centre of London, which would afford better facilities to the Institution in the prosecution of its work.

In *Science* (September 6, 1918) Prof. C. C. Nutting reports, in outline, the results of the Iowa University Expedition to Barbados and Antigua. The object in view was to secure collections in marine zoology, entomology, and geology from this, scientifically, little-known region, and also to study the living forms in and

around the islands visited. Lodging and laboratory facilities were provided by the Colonial authorities, while Mr. John B. Henderson, of Washington, provided a fully equipped 27-foot launch, which made it possible to dredge down to 150 fathoms. It is stated that practically all the species taken with this dredge will provide new locality records extending the known geographical range.

Apparently the question of power-alcohol is at last to be taken in hand by the Government. Mr. Walter Long has appointed a Committee, with Sir Boverton Redwood as chairman, to investigate the available sources of supply with particular reference to its manufacture from materials other than those which can be used for food purposes, the method and cost of such manufacture, and the manner in which alcohol should be used for power purposes. The Committee includes Mr. Arnold Philip (Admiralty Chemist), Prof. Charles Crowther (representing the Board of Agriculture), Sir H. Frank Heath, Prof. Harold B. Dixon, and Brig.-Gen. Sir Capel Holden. Mr. E. S. Shrapnell-Smith will act as Secretary to the Committee.

A general meeting of industrial and professional chemists was held at Manchester on October 26 last to consider the draft regulations of the British Association of Chemists and the report of the Nominations Committee of that body. It will be remembered that the movement which resulted in the formation of the Association originated at Manchester in 1917. Since then many local sections have been formed, notably in London, Liverpool, Leeds, Huddersfield, Newcastle, and Glasgow; the object of the Association being to safeguard the interests of professional chemists in the same manner that the British Medical Association looks after the interests of medical practitioners. It was stated at the meeting that, as a result of negotiations between the Association and the Institute of Chemistry, the latter body has considerably widened its scope and the area of admission to membership, and has agreed to become the sole registration authority for chemists. Further, Mr. H. W. Rowell, the General Secretary, announced that co-operative action with other scientific societies was contemplated, and that the Government departments had been approached with a view to representation on industrial councils and similar bodies. Finally, it was agreed to proceed with the registration of the Association as a limited company after a movement to register it as a trades union body had been defeated by a large majority.

The total amount which had been collected for the Ramsay Memorial Fund at the time these notes were written was £39,500, and efforts are being made to raise the remainder of the Fund as speedily as possible. H.R.H. the Prince of Wales has accepted the position of Patron, and has given the first donation of one thousand shillings to a Million Shilling Fund which has been started so as to give to all an opportunity of taking part in the Memorial. Considerable progress has been made in connection with the proposal for the foundation of Ramsay Memorial Fellowships by the Allied and Neutral Governments. Five of these—namely, Italy, Japan, Spain, Greece, and China—have already intimated their intention of founding Fellowships, and a number of other Governments are considering similar action. This plan should result in bringing a large number of chemists trained in foreign countries to continue research work in the United Kingdom.

The Salters' Company have decided to form an Institute of Industrial Chemistry to aid the application of scientific research to the chemical trades. They have appointed Dr. M. O. Forster, F.R.S., Chairman of the Technical Committee of British Dyes, Ltd., and Honorary Treasurer of the Chemical Society, to be the first Director, and state that he will take up his duties at an early date. These

will include the following: (*a*) To make arrangements with universities and institutions to enable students to obtain facilities for research and technical training; (*b*) To arrange between manufacturers, students, and universities for the investigation of any particular problems requiring research; (*c*) To give practical advice and information to those who are, or intend to become, industrial chemists, especially to men whose careers have been interrupted or affected by Naval, Military, or National Service; (*d*) To advise the Company generally as to the progress and possible extension of the work of the Institute. The Company will establish two types of Fellowships, for which post-graduate students of any recognised university will be eligible. These two classes are as follows: (*a*) Fellowships to enable post-graduate students to continue their studies at an approved university or other institution under the general supervision of the Director; and (*b*) Industrial Fellowships to enable suitably equipped chemists to carry on research for any particular manufacturer, under an agreement which will be entered into between the Institute, the manufacturer, and the Fellow. The monetary value of these Fellowships is not stated. No building is at present in contemplation, the address of the Institute being the Salters' Hall.

The Twelfth Annual Report of the Carnegie Foundation for the Advancement of Teaching, whose object is to provide life insurances and pensions for college and university teachers in the United States, Canada, and Newfoundland, states that the Trustees have concluded that they can no longer provide free benefits, and that they are therefore arranging for a contributory scheme on similar lines to the Federated Universities superannuation scheme in force here. There is to be one very important difference however. In the English scheme the contracts are carried out through the agencies of existing insurance corporations. The Carnegie Institute is founding a special teachers' insurance and annuity association which, not having to make shareholders' profits, confers appreciably greater benefits at lower rates than those which the British university teacher has to pay. Why university teachers should have been specially excluded from the Teachers' Superannuation scheme recently introduced in Parliament by Mr. Fisher is a mystery which no one concerned has apparently been able to fathom. On the surface it appears to be an act of wilful and deliberate injustice to the only body of teachers who have received no benefits from Mr. Fisher's administration.

A note in *Nature* (August 15) corrects what was probably a somewhat widespread impression concerning the supply of optical glass at the outbreak of war. It appears that with seventy years' experience of optical glass manufacture to help them, Messrs. Chance Bros. & Co. were able to supply nearly the whole of the optical glass required for instruments used by our forces during the war, and that without any assistance from the formulæ determined by the Glass Research Committee of the Institute of Chemistry. This Committee, however, rendered great assistance to the manufacturers of scientific and heat-resisting glassware.

Sir Robert Hadfield has issued a translation of a Report from the German Committee for Technical Education concerning the steps to be taken to counteract the serious diminution in the numbers of scientifically trained workers in Germany—*i.e.* engineers, chemists, mining engineers, metallurgists, and architects—which, it is stated, will "render the reconstruction of our economic life ominously difficult." Written on the supposition that the war would end favourably for them, it contains suggestions that would be of much value if utilised here; though it is difficult to conceive that an average English manufacturer should consider a "lack of scientifically trained technicists" likely to hinder his progress! The suggestions are directed towards two ends. First, to enable those whose training has for so

long been interrupted by the war to finish their training as quickly as possible; and secondly, to remedy the defects in the preliminary education of the students, this having fallen far below the usual standard on account of the leniency shown at the examinations. Remedial measures of three kinds are advised: (i) That during the transition period, estimated at two years, the college curricula should be altered by transmitting only the principles of technical knowledge and omitting comprehensive details which can easily be acquired in professional practice. Purely scientific education should, however, be diminished as little as possible, since that is much harder to obtain later on. (ii) To adopt a "far-reaching individualisation of instruction," since men coming back from the army will have arrived at very different stages of knowledge, each requiring special gaps to be filled in. For this purpose it is considered that lectures alone cannot suffice. (iii) Bursaries should be provided for the many whose finances have become embarassed owing to the war, and who would otherwise have to become wage earners at once, to the grave detriment of the public good. Finally, the Committee requests the Army authorities to demobilise the teaching staffs of the technical universities and mining schools at the earliest practicable moment, and the students themselves as soon afterwards as possible.

The Smithsonian Institute is issuing a series of monographs dealing, in non-technical language, with the mineral resources of the United States. The last published of these (*Bulletin* 102, Part 6) deals with petroleum. The United States at present produces about two-thirds of the world's supply of this product, so that the information contained in the report is of considerable importance to all users of petroleum and its derivatives. It appears that the liquid deposits in the States are being rapidly exhausted, so fast, indeed, that at the present rate of consumption they will all have been used up by 1930. That is, unless the wasteful method of mining now employed, which yields only about 10 per cent. of the total deposit, is modified. It is not, however, unreasonable to expect that further exploration and development will make available a reserve of oil in Mexico and Central America equal to the total now remaining in the United States. The dominant sources of supply at present are the Kansas-Oklahoma[1] and the Californian fields, from which about one-third of the total reserve has been used up. Outside the United States the most important reserves are those in Mexico and Russia, the others (in the Dutch East Indies, Roumania, India, and Galicia) being relatively unimportant. The process of exhaustion may be delayed by improvements in the methods of mining and of consumption. At present a field is "exhausted" when some 40 to 50 per cent. of its store has been withdrawn. By preventing the ingress of water by the improved methods of drilling and pumping now available much better figures can be obtained; further, the enormous waste which now occurs could easily be stopped. As to consumption, crude oils should be used directly in engines of the Diesel type, instead of using them as fuel for raising steam. This alone would double the power-generating capacity of the 7,000,000,000 barrels still underground. It is stated that "the use of oil-fuel has grown so extensively during the past year that an overburden now rests upon it which will bring an inevitable train of industrial disasters in the coming months, as the supply is wholly inadequate to sustain even the current demand. Unfortunately, the swing away from coal in favour of fuel-oil is still continuing." However, even when the liquid deposits are exhausted the "petrol era" will not

[1] The Cushing Pool in Oklahoma gave about 300,000 barrels per day in 1915: more than one-third of the daily consumption of petroleum in the whole of the U.S.A.

come to an end, for the deposits of oil shales will yield enormous supplies—sufficient to last hundreds of years. South-West Indiana alone is underlaid by shales sufficient to produce 100,000,000,000 barrels of oil. These shales, too, may yield valuable by-products. For example, a ton of Colorado shale gives about 50 gallons of oil, 3,000 cubic feet of gas, and 17 lb. of ammonium sulphate for use in agriculture. Finally, of course, alcohol can be manufactured from organic sources which are in practically limitless supply[1]; there is, notably, the possibility of its production from the rank vegetation of the tropics.

Bulletin No. 7 of the Advisory Council of Science and Industry of Australia contains an account of the proceedings at a conference of agricultural scientists which was convened by the Council at Melbourne in November 1917. At the end of the conference a number of resolutions were passed for the purpose of advising the Council as to the best methods of benefiting the agricultural industry in Australia. It was decided to advise the formation of a Seed Improvement Committee, a Plant Introduction Bureau, and the appointment of a permanent agricultural representative to the United States for the purpose of keeping Australia in touch with the improved methods which are continually being introduced in that country. It was further resolved that steps be taken to resuscitate the tobacco-growing industry, possibly by creating a Government monopoly, and, finally, that more experiments be made with a view to the production of power-alcohol.

An article by Mr. D. Brownlie in *Engineering* (August 23, 1918) contains an account of the steps which have been taken to economise fuel in the U.S.A. It appears that, owing to war conditions, this question is as pressing there as in the British Isles, the shortage being estimated at 80,000,000 tons per annum. To meet it a Fuel Engineering Division has been formed in the U S. Fuel Administration in Washington. It comprises two departments, one dealing with the railroads and another with all stationary power plants. The department has made certain recommendations for the conservation of fuel, and has appointed inspectors who will make personal inspection of every plant. It has power to curtail or cut off altogether the supply of fuel to any plant found to be needlessly wasteful. To assist users to carry out their recommendations a 50-minute film has been prepared, showing good and bad methods of operation, methods of testing boilers, etc. No figures showing the efficiency of boiler plants in the U.S.A. are available, but Mr. Brownlie is of the opinion that it is not greater than over here, where investigations of 250 typical plants show an average efficiency of 60 per cent., as compared with the 75 per cent. easily obtainable. Indeed, the steam plants running the anthracite mines in Pennsylvania consume 10 per cent of the coal they raise, while in Great Britain the average figure for all classes of mines is 7 per cent. Owing to the great demand for fuel in the winter of 1917, and the consequent lack of competition, the amount of impurity in the coal had greatly increased. To remedy this the Government took strong action, especially in Pennsylvania, appointing inspectors to condemn all anthracite yielding more than 20 per cent. ash. In May last only one-half per cent. of coal was thus condemned. Had the same standard been in operation in January and February over 50 per cent. would have been condemned! Mr. Brownlie's paper contains valuable suggestions as to the methods that are available to control the output of boiler-houses.

[1] It is remarkable that the consumption of alcoholic beverages and of petrol in the U.S.A. in 1916 were approximately equal; each about 2,000,000,000 gallons, or 20 gallons per head of the population.

Memorandum No. 21, from the Health of Munition Workers Committee, contains an account of the investigations made by Dr. H. M. Vernon on the Causation of Industrial Accidents. Data were collected from four factories for periods varying from 9 to 25½ months, and the analysis which he gives of the 50,000 accidents which came under notice leads to interesting and somewhat remarkable results. In the first place, as might be expected, speed of production plays a considerable part; the diurnal variation of accidents generally corresponding with output variations. Fatigue apparently had little effect on the accidents to male workers; but with women the number of accidents when working a 12-hour day (75 hours per week) was 2½ times greater than when the period was reduced to 10 hours. With the longer shift women were treated for faintness 9 times more frequently than men; with the shorter only 3 times more frequently. A striking difference was shown between the night and day shifts, the *former* giving a distinctly larger output and 16 per cent. fewer accidents. Dr. Vernon attributes this to psychical influences, the workers being much less excited and distracted from their work during the night than in the daytime. This conclusion is supported by the fact that the accidents were at a maximum at the beginning of the shift, and fell gradually the whole night to about half the initial value. Indirect evidence of the ill-effects of alcohol was obtained—especially in respect of alcohol consumed during the dinner hour by day workers and just before coming to work by those engaged at night. Although the total number of accidents at night was less than by day, eye accidents were from 7 to 27 per cent. greater, the excess being greatest in the worst-lit factory. Finally it was noted that temperature has a most important effect. The most favourable temperature is 65° to 69° F.; the number of accidents increasing rapidly at higher temperatures (by 30 per cent. at temperatures above 75° F.) and more slowly at lower ones Moreover, as the outdoor temperature grew colder accidents increased rapidly; at one factory women's accidents were nearly 2½ times more numerous when the temperature was at or below freezing-point than when it was above 47° F., whilst the men's accidents were twice as numerous. To reduce the number of accidents to its inevitable minimum it is desirable to induce in all the workers throughout their hours of labour the same mental outlook as is present in the night-shift workers in the early hours of the morning, when their attention is best concentrated on their work. However, to produce this state of mental calm in the daytime would require such monotonous conditions of labour that the worker would certainly pronounce the cure worse than the disease.

The Report of the Committee of the Privy Council for Scientific and Industrial Research for the year 1917–18 conveys the general impression that that body is very well pleased with itself. And indeed much progress has been made. The fuel-research station is in course of erection at East Greenwich, and should be finished by the end of the year; its cost being estimated at £120,000. The National Physical Laboratory has been taken over from the Royal Society, and the salaries of its staff raised to the scale recommended by the Executive Committee of the Laboratory. Further, the pensions scheme in operation for university teachers has been adopted in order to facilitate transfer of staff between the universities and the Laboratory. The Board of Trade are arranging, subject to the approval of the Treasury, for the transfer of their Electrical Standards Laboratory to Teddington, thus avoiding the uneconomical maintenance of dual standards. The cost of the Laboratory to the Council for the (then) current financial year was £89,750, while, in addition, it rendered services to the several war departments which will be met out of the Vote of Credit at an estimated cost of £74,100.

Research Boards for Food and for Tin and Tungsten production in Cornwall have been established. Several new industrial research associations have been formed, notably the British Scientific Instrument Research Association, which has received a grant of £36,000 to be spread over the next five years, and the British Photographic Association, which will receive £1,500 a year for the same period. The Iron Manufacturers' Research Association has been founded by the British Iron Puddlers, who have subscribed all the necessary funds, and have decided not only that all the results of the researches undertaken shall be freely available to each firm (and 97 per cent. of the whole industry is represented), but that "all existing knowledge, trade secrets, and procedures shall be pooled for the common good." The internal-combustion engine makers failed to form an association in the absence of sufficient common ground for common action, but each section is moving towards the formation of a separate association with power for co-operation in attacking any problems of common interest which may arise. Similarly, the makers and users of refractories have not so far been brought into agreement. Nevertheless, the Council has made a grant of £1,500 towards the cost of a new research building for refractories at Stoke. A subscription of £500 for five years is to be paid to the International Commission for the Publication of Constants and Numerical Data, provided that the French Government, who have been the principal supporters of the Commission since its foundation in 1909, should continue their subscription of 3,000 francs. Fifty-eight students and research workers have received personal grants: but so far the Advisory Council to the Committee have not felt themselves able to draw up stereotyped regulations for their awards, and are, at present, basing them on their personal knowledge of the research worker or of the professor or other person recommending him. While this is being done it would be more satisfactory if the Council gave a list of the institutions at which those who have received grants are working. It has been decided that, save in exceptional circumstances, not more than two annual grants should be made to one student to enable him to undergo training in methods of research, or more than five annual grants in the nature of a Fellowship.

The second *Bulletin* issued by the Advisory Council is a Memorandum on Cutting Lubricants, Cooling Liquids, and on Skin Diseases produced by Lubricants. It forms a valuable summary of our information on this subject, but is hardly suited for abstract here.

The Council of the Bread and Food Reform League, which is engaged in a campaign for the increased use of finely ground whole wheat meal, ask us to state that if specimens of bread containing large fragments of bran are sent to their office at 37 Essex Street, W.C.2, together with particulars as to its origin, they will take steps to remedy the defective milling to which it is due. The League also desires to draw attention to a Report published by the Public Health Service of the United States which emphasises the fact that white flour deprived of the essential phosphates and vitamines will, when it forms the principal food, lead to "deficiency disease" conditions most prejudicial to the general health. On the other hand, for easy digestibility whole wheat meal must be ground to sufficient fineness to pass through a 16-mesh sieve.

ESSAYS

SCIENCE IN EDUCATION (Prof. E. H. Starling, C.M.G., F.R.S.)

Educational Shortcomings

FROM a broad standpoint the present war is a struggle for existence between two types of organised society. One, the autocratic, is based on the dominance of a military caste; in the other, the democratic, the chief power is represented by public opinion. Comparing only Germany and England, some might say that the struggle is really between two kinds of oligarchies, the one military and the other aristocrat-capitalist: but it cannot be denied that while the success of the former type would lead to a tightening of the bonds which hold the rest of the population in check, a complete victory of England would preserve and enhance its power of self-evolution, and would ensure the continuance of the process by which the ultimate political power is transferred to the maximum number of the people. In the one case we should see a strengthening of aristocratic rule, in the other case a progressive democratisation of the system and form of government.

In the struggle for existence, whether by peaceful commerce or by the methods of war, an ever-increasing perfection of organisation is a necessary condition for the survival of states as of animal types. There are some who believe that there is an inherent disability in a democracy to secure the perfection of organisation, the competence of the chiefs of public departments, the discipline and fitness of all its members which are necessary to secure the efficiency of any state either in peace or war. Germany's success during the first four years of the war might be taken as a proof of superiority in her form of government, especially when her material disadvantages and the relative weakness of her population are taken into account. Even if she were able to secure a peace negotiated on equal terms with her enemies, she would have gained much by the war. By her very existence as a military state she would continue to impose a military form of government on the democracies by which she is surrounded, and the main efforts of these democracies for years to come would be directed to securing national safety by continuing the competition in armaments and in military preparations which their people found so heavy a burden before the war.

But even if Germany had attained the peace which she desired—a peace which would be victory for her and defeat for the Allies—the abstract question as to the most efficient form of government would not be decided. The vast and ruthless experiment which Fate has been enacting before our eyes is not what scientific men would call a "clean experiment," since the course of the struggle and the varying fortunes of the two sides have not been determined by the nature of the governments involved. The Allies, including autocratic Russia, democratic England, and republican France, had, as nations, neglected science and misconceived or despised education; while against them they had a state which, since the time of Napoleon, has been imbued with the idea that education is the most effective means of enhancing its power and prosperity. As a result we find that,

throughout Germany, science was recognised and appreciated as that knowledge of actualities which must be at the disposal of men of affairs if they are to control and deal with the material conditions of their environment. Highly efficient industries based on science were instantly transferable to the provision of munitions of war, while the widespread university education provided an intellectual proletariat fit to staff these as well as all the new Government departments which spring up during a great war to deal with its new-found scientific activities. The fundamental lesson of science had indeed sunk deep into the heart of the German nation, the lesson that success in life depends on the employment of exact knowledge to foresee, months or years ahead, the likely trend of events, and on making the necessary preparations to meet them and to weld them to their national purpose.

Thus it came about that the slow-moving, sluggish-brained German showed himself at first more prompt in attack, quicker in reaction, and more adaptable to the ever-changing conditions of warfare than the quick-witted Frenchman himself, and surpassed us just in those qualities of initiative and resourcefulness which we have always considered as peculiarly English endowments. Against the resource and discipline of this nation, actuated by one purpose and one idea, we opposed a kindly, gentlemanly stupidity, which, though preserved from actual catastrophe by the self-sacrifice and bravery of our youngsters, was indirectly responsible for more human suffering and misery than the studied and ruthless brutalities of the German. Our leading was inefficient, not through self-seeking on the part of the leaders themselves, but through ignorance. The great rally of the nation occurred in spite of an education which taught the ruling classes that their first duty was to their clan, their party, or their service, while it left to the Trades Unions the task of teaching the artisan how to "play the game." Our countrymen, sound at heart, were able, under the stress of the great national danger, to merge the lesser in the greater, the party, class, or union in the nation. But a still greater handicap was the intellectual poverty of the education—an education which launched men on life without any knowledge of the nature of the world around them or of the chain of natural causation responsible for the happenings which made up their existence. Indeed, the nation's training tended to inculcate a contempt for any knowledge which did not promise some immediate practical advantages to its votary.

Necessity for Educational Reform

In view of the ignorance of elementary truths shown so often by their leaders—some of them the finest products of our present system of education—it is hardly to be wondered at that the nation as a whole has begun to ask whether something is not wrong with the system, and to demand reform, especially in the direction of the introduction of a greater proportion of natural science and of modern learning into our schools and universities. But every movement for reform evokes a counter-movement on the part of those, conservative by nature or by age, who are firmly impressed with the advantages of the system in which they have been brought up and of the danger to the state of revolutionary change. They would keep the impious hands of the reformer off the ark of the Covenant, the traditional classical education, which in their opinion is responsible for inculcating the fine spirit of duty and devotion in our younger officers. It might be asked what percentage of our younger officers have had a classical education, or what connection there is to be found between military honours gained for gallant deeds and the type of education of the officers and men

involved? But what is to be dreaded is that the necessity for reform in our educational methods may be lost sight of in some sterile dispute as to science versus the humanities, compulsory Greek versus Bolshevism, or some equally meaningless formula. To debate the claims of one subject against another at the present time is beside the mark; an education based on chemistry would be as incomplete and disastrous in its results as one based on the Greek syntax. Specialists may be used by the state, and the state should appreciate their worth and know how to employ them for its greater ends. It is not by making all men specialists, however, that we shall achieve wisdom; professors may be as stupid as members of Parliament so soon as they are taken out of the corner of knowledge over which their practical experience extends. It is because of the existence of this idea of an antagonism between science and, say, the classics that I have ventured to set down what seem to me to be the first principles of every education.

It is now universally recognised that the education of the community is the duty of the state. All stages of education are controlled to a greater or lesser degree by the state. This control has developed of late years with the increased amount of public funds allotted to education, and there is no doubt that the responsibility of the state will become rapidly greater with the growing recognition of the supreme importance of education for the welfare of the nation.

Objects of Education

It is therefore in its relation to the state—and by that I mean the common will and resultant action of the whole community (which in a democracy should be synonymous with the state)—that I propose to deal with the objects and methods of education. Each individual must be fitted by his education to play his part in the community in adding to the common weal, *i.e.* to be a healthy, good, and useful member of the community. Let us see what is implied in these qualities. I propose to say nothing in reference to health, since all are agreed as to the significance of the word "health," and as to the chief means by which it may be ensured. Fresh air, good food, good housing, bodily discipline, all these are contained implicitly in every scheme for the improvement of the younger members of the race. It will be useful, however, to inquire into the real significance of the qualities which we speak of as "good" and "useful." It is evident that "good" cannot have an identical significance in different races. A "good" Japanese or a "good" Brahmin will have different ideals of conduct and will behave otherwise than a "good" Englishman. Underlying these differences there is a common meaning: wherever he may be, a "good" citizen is one who works in harmony with other members of the community, who observes the rule of the tribe and is able to subordinate, both in desire and action, his individual welfare to that of the body of which he is a member. This implies that the individual during his plastic years of growth has been constrained to act in certain ways, and has thus received from his environment habits, bodily and mental, which have moulded his behaviour and even his emotions to the advantage of the community. The making of a good citizen has always been regarded by every nation as the first and most significant part of education. The establishment and observation of a social rule has been of such prime importance in maintaining the integrity of a race and in enabling it to resist aggression from without, that the office has been in many cases entrusted to a special class of the community—a priesthood. Under the authority of the priesthood the tribal law has been imposed on the community by the appointment of dire penalties for its slightest infringement, and

all the resources of literature and art, everything which can act on the mind or senses of man, have been called in to give an ideational basis and to strengthen the emotional impulse to "goodness." It is hardly surprising, then, that to this priesthood has been entrusted, in the majority of cases, the training of the young. Their first and often their only care was to make a good citizen; the further office of education, that of making a useful citizen, could generally, until the advent of science, be left to the manual training received by apprenticeship in the arts of war and in the crafts of peace.

Training of Character

Stated in biological terms, we may say that the main object sought in this part of education is to establish automatic reactions in the plastic brain of the young individual, reactions which, though in many cases at first conscious, become more and more relegated to the class of instinctive reactions or behaviour. It is a higher phase of the process by which the infant learns to walk, or by which the soldier springs sharply to attention at the word of command and maintains his dressing and equal step on the march. In the infant we may use the methods of reward and punishment for the production of these reactions, employing the world-old discipline of pain to inhibit such reactions as are undesirable. From early life, at any rate in the higher communities, this method becomes insignificant in comparison with the influence of mimicry and the desire of self-esteem and the esteem of one's comrades. With the growth of intelligence the reactions so learned acquire an ideational tone and an intellectual justification from the consciously or unconsciously biased teaching of history, literature, or religion. The special part played by religion in this connection is shown by the great social changes and transformations in rules of conduct which follow any general change in religious conviction or belief. Thus a rule which is at first enforced by the common will of the community or by the pressure of external circumstances comes finally in the old community to be expressed in the desires and impulses of the individual himself, so that morality or proper social behaviour (goodness) becomes in the educated individual instinctive. A number of terms are employed to denote the different aspects of the quality of the individual thereby produced: we speak of morality, training of character, inculcation of self-control and righteousness, the evocation of altruism and patriotism; in common language, we try to teach the individual to "play the game," and we endeavour so to build up his mentality that he cannot act otherwise without doing violence to his instincts. This part of the individual's training depends to a very slight degree on the intellectual content of the education. At an English school it is the tone of the school and the constant contact with the rules of its little society as experienced in all the relations of life, and especially in combined games, which play the greatest part in the fixation of rules of conduct. To teach a boy to play the game is the main object and the principal achievement of the training of the Boy Scouts, as it is of the English Classical Public School. With the same school curriculum the most diverse characters can be formed and the most divergent rules of conduct inculcated. The German gymnasiast, who furnishes the greater part of the higher servants of state in Germany, and whose ideals of conduct we have learnt to abhor, has received a classical education probably superior to that of nine-tenths of the boys who leave our public schools. Our officers in the present war have received the most diverse of educations—in most cases singularly imperfect in range and thoroughness. But they practically all have shown their assimilation of the rules of conduct accepted as becoming to members of the British commonwealth of states,

It is thus incorrect to imagine that a reform of the school curriculum, whether in the direction of a better teaching of science or classics, the abolition of Greek, or the introduction of more science or more scientific studies, will have any large influence in determining the character of the man produced by the system of education. On the other hand, it would be a mistake to undervalue the influence of the intellectual content of the education on the character and behaviour. Everything here, however, depends on the emotional colour which is given to the studies. All school studies can be biased or turned to the glory of God or of the Fatherland, or be made to afford lessons in the desirability of certain abstract principles which may guide conduct, such as the ideas of freedom, sacrifice, patriotism, or national vanity. A certain community of opinion on purely intellectual questions is of value in promoting the solidarity of a nation. The value of literature must not be underrated in bringing a boy in relation with his fellows, in making him responsive to their changing thoughts, and in enabling him to enter into their hopes and fears, their joys and sorrows. But according to our ideas this side of education has a still more important part to play. A too rigid social rule may result in producing a people so "moral" that all change becomes "immoral" and social progress practically ceases; such a condition seems to have existed in Japan during the 200 years under the Shoguns,[1] and must have occurred again and again in the history of past empires and nations.

It is to the intellectual side of the curriculum that we must look for the implantation of suggestions and desires for initiation and invention, so that the social rule may be regarded not as something hard and fast, carved in granite for all time, but as comparable to a tree in full growth which is constantly putting forth new branches and fruits.

We may conclude, then, that, although we regard the building up of character as the first aim in our school system, we may nevertheless give the educational reformer a free hand in the modification of the school curriculum without necessarily interfering with this its supreme purpose.

Training in Human Experience

A race, even though composed of individuals who are all "good," must go down in the struggle for existence if it is unable to fight for its own hand and to utilise its past experience and the foresight gained by this experience in procuring from the natural resources of the world all those products which are necessary for the maintenance of its members. This efficiency of the state can be attained only if their education has succeeded in making the citizens useful as well as good. It is this function of education which we in England have most neglected, and in which the need for reform is the most pressing. Our failure to correlate education to the needs of the nation, to make it useful in fact, rendered us unready for the present war, and has prolonged the war at the expense of thousands of lives, and leaves still in the balance our fate in the times that are to follow.

The position of any species in the animal scale is determined by the range of its powers of reaction and adaptation; in the higher types it depends on the ability of the animal to profit by experience, so that in all its acts it may eschew the evil and choose the good. In man the young animal comes into the world with a brain representing almost a clean slate, a plastic mass of nervous matter, in which the paths are to be laid down and the behaviour determined by experience, *i.e.* by education. The helplessness of the human infant as compared with the

[1] Chamberlain tells us that the Japanese word used for an immoral action meant literally "an other-than-to-be-expected action."

young of other animals is an index of the degree to which his future behaviour and capabilities are determined by his education. From the moment of birth onwards the child is trying and testing the properties of his environment and of his powers in relation to the environment, and the experiences so gained, appraised according to the varying emotions either of pleasure or of pain which accompany them, are the building stones of the mind and character of the adult man. The acquisition and storing of sense experience in man is enormously enhanced by the power of speech, *i.e.* by the ability to use words as symbols for sense experiences, so that words can be dealt with like counters in a game, without changing them at every step into the currency of the complicated series of phenomena with which they were originally connected. Moreover, the use of written and spoken language has made it possible to utilise the whole of racial and human experience in guiding the reactions of any individual. In a community all members have the advantage of the experience, and can profit by the wisdom of the most highly gifted of their race—a fact which greatly enhances the rate of progress in the acquisition of knowledge and power by man throughout the civilised world.

The use of symbols for facilitating and quickening thought has been extended far beyond the mere employment of words. In mathematics symbols have been invented in order to deal with quantities, and have been carried to such an extent that a few letters arranged in the form of an equation may represent the result of years of labour and millions of experiences by men under all manner of conditions. Such shorthand expressions of past sequences of experience are designated as natural laws, meaning by this that, since certain phenomena have followed in orderly and known sequence in all past experience of man, the same sequence will be observed in the future. Science is then nothing more than the orderly presentment of human experiences. Its value lies in the fact that a man, when he knows the normal sequence of sense experience, can foretell what will happen as a result of a given set of conditions and can regulate his actions thereby. He thus acquires foresight and wisdom by the experience of others imparted to him in his education, and has not himself to undergo the hard and painful process of gaining wisdom by the method of trial and error, a method to which, under the designation of "muddling through," this country has been too often reduced by the ignorance of its leaders.

The object of the intellectual side of education is to place at the disposal of the individual such parts of human experience as will be of value to him in raising his efficiency and in guiding his behaviour within the limits of freedom allowed to him by the social rule of his community. It is naturally impossible to endow any individual with the whole of human experience; but it is possible to give to every one such a survey of human knowledge and such an attitude towards all knowledge that he may appreciate its importance in all affairs of life, and may know that, behind the little area with which he is acquainted, there is a great body of experience called science which can be drawn upon for guidance in any new or unexpected situation which may arise.

In this country it has been customary to draw a sharp line of distinction between the man of affairs and the expert, rather to the detriment of the latter. People have failed to recognise that the man of affairs is generally an expert in some very minute fraction of human activities, and that it is by specialising in this one direction that he has succeeded in making money and acquiring a position in society. The limitations of his experience and the paucity of his general knowledge often render him the worst of guides in matters falling outside his own little speciality. The striking advances made by German industry during

the last twenty years, which gained for them the monopoly of production of many articles essential for the business of peace or war, was due not so much to the wide knowledge possessed by each individual as to the fact that throughout Germany there was a respect for knowledge and an appreciation of its value to the nation. Thus it was worth while for men to become expert in all the varied branches of science, so that in every undertaking the whole of the past experience of mankind dealing with the subject could be brought to bear on any problem in question.

Importance of Instruction in Language

From a broad standpoint it is impossible to draw any marked line of distinction between literary and scientific studies. Since words are the counters we use in the complicated processes of thought, we cannot hope to attain accuracy of thought and reasoning without accuracy of expression. A well-turned phrase is one which signifies exactly what we intend to say. An essay possessing literary form must be one in which the ideas are well expressed and the reasoning logically carried out. The first principle in education, whether we call it literary or scientific, is a training in the use of language, and we shall achieve little in the way of improvement of education until every lesson, whatever its subject matter, is at the same time a lesson in expression, *i.e.* in the use of appropriate language. From the very beginning of education every word learnt should be realised as expressing some definite experience, and it is especially important to avoid the use of expressions which have no real significance. Among the Japanese every piece of writing, whatever its content, was an object of respect and acquired a sacred and even magic import. What we have to guard against is allowing a child to attach to words, or to a collection of words, a similar importance apart from their real meaning. The devotion of the inarticulate Englishman to catchwords and his hypnotisation by phrases is largely due to his defective training in language. A real reform in this respect might go far to break down the senseless adhesion to party formulæ which is such an impediment to social reform.

The first essential, therefore, in the intellectual side of education is training in language. Our forefathers recognised this in putting Latin and Greek in the forefront of secondary education. At that time these two languages were the gateways to all human and scientific knowledge then existent, while Latin was the only means of communication among scholars and therefore of acquiring new knowledge or new ideas from other workers and thinkers. But now the situation has changed: it is our own language which is the portal to knowledge and the instrument of thought, and training in its use should be the beginning of education and occupy a prominent place throughout the whole of school life. Every lesson, whether in other languages, ancient or modern, in science or history, should at the same time be an exercise in the use of the mother tongue. There seems no reason why this vast and noble heritage of ours should not be an object of study as detailed and devoted as that applied by the French to their own language. And indeed the principal value of the study of other languages for most boys and girls is that they gain therefrom a fuller acquaintance with their own. By this means they learn the roots of which their words are built up, the real value of each word, and the meaning of exactness of expression. This is not to deny utility to the teaching of other languages than English. A knowledge of Latin, for instance, not only elucidates the meaning of words in English and teaches a just appreciation of the methods of grammatical construction, but it also facilitates

the acquisition of any of the Romance languages. As to the utility of modern languages, such as French, German, Italian, or Spanish, no arguments are necessary. Which of these are chosen for a detailed study in school must vary from time to time and from school to school, according to the average requirements and ambitions of the scholars and the habit of intercourse, commercial and intellectual, of our nation with others. It is difficult, however, to see any valid grounds for retaining Greek as a school subject. It is true that we trace back the origins of our civilisation, of our general ideas, and of our politic, to Greek civilisation; but it is not necessary to study Greek language or Greek grammar in order to learn Greek history or to make some acquaintance with Greek thought or art. Most boys who learn Greek at school never get farther than the rudiments of the language, whereas, through the medium of English, all boys could be taught something of the part played by Greece in the evolution of modern ideas. As to the employment of Greek roots in scientific language, half the men who employ them do not know their real meaning, and they could and should be learnt as part of the lesson in English when dealing with the meaning and construction of words.

Natural Science and the Humanities

After the training in language, the instrument of thought, comes the imparting of human experience—*i.e.* knowledge. The different forms of experience can be roughly classed as—

(1) The nature and behaviour of things—*i.e.* the material world, and the relation and importance of these things to the individual.

(2) The relations of men among themselves and to the individual.

These two groups correspond roughly to what are generally spoken of as natural science and the humanities. Judging by the normal order in which the curiosity of the child awakens to his environment, at any rate in the case of the boy, the knowledge of *things* should precede the more difficult and complex study of the relations of *men*; the latter, which must be regarded as the more directly important to the average man, being more fully developed in later school life with the growing mental powers of the child.

It will be evident that if we accept the principles already enunciated a very wide latitude is possible in planning out a school curriculum. Granted the acceptance of these principles, I should be inclined to leave their working out in practice to those whose life has been spent in the education of the young. I venture, however, to give a scheme of elementary and secondary education arranged in three stages, more by way of illustration than with a desire to lay stress on the particular order of the stages therein contained. Throughout it must be premised that the training is divided into moral and intellectual, and the latter again into the training in the instrument—*i.e.* language or symbols—and the training in human experience.

1. *Elementary Training.*

(*a*) *Behaviour.* Moral training.—A child learns the rule of the school in his lessons and in his play. An ideational aspect is given to these lessons in Scripture, in tales from History, etc. At this early stage the chief value of these studies is the strengthening of the educational effects of the social law.

(*b*) *Words.* Their meaning and their use. Reading, writing, and speaking.—I would lay special stress on speaking, and make a child give an account in words of something he has seen or knows. Moreover, there is no reason why difference

in class should be marked throughout life by difference in pronunciation. Men can never be equal, but the inequality should depend on character and intellectual capacity, not on manner.

(*c*) *Quantities*. Arithmetic.—Here the child begins to learn a new set of symbols, to use them as counters and to correlate them with conceptions of quantity.

(*d*) *Things and their qualities*.—This would represent the beginning of education in natural science and would correspond to what is often called Nature Study. It is at this initial stage that a child could be helped to satisfy his natural curiosity about the things around him, and to learn that a greater satisfaction and a wider knowledge can be acquired later when he comes to study the various sciences strictly so-called. The great object of the training at this epoch is to stimulate curiosity, not to damp it. This and all later education in science will fail unless it is joined with training in expression, so that a child may learn the accurate use of terms, and know words, not as mere jangle, but as denoting definite things, happenings, or qualities. Every lesson in science should at the same time be a lesson in English, and in clearness of thought and language.

2nd Stage.

(*a*) *Behaviour*. Games, general discipline of the school or family.—Certain aspects in the instruction of history and literature.

(*b*) *Language*. English grammar and composition and literature.—Some modern language.

(*c*) *Quantities*. Mathematics.

(*d*) *Relations of things*. Science, description in speech and writing.—At this period the child should learn the elements of the properties of matter and energy, *i.e.* chemistry and physics. Some instruction in the phenomena of life (physiology) should be added and correlated with the chemical and physical training.

(*e*) *Human relations*. History, geography, literature.—The child should learn something of the relation of the individual to the community in which he is governed; a subject which, though almost entirely neglected at the present day, is essential if we are to turn out good citizens ready to take their part in a self-governing community.

3rd Stage.

(*a*) *Behaviour*. Cadet Corps, or O.T.C., games, religious instruction.—A great part is played by the subtle influences acquired from the various subjects which are being studied in the school or by way of recreation, or are under discussion by his fellows.

(*b*) *Language*. English.—One or more modern languages. Latin.

(*c*) *Science*.—At this advanced stage an endeavour should be made to give the student some idea of the history of science and of the great laws or generalisations, such as the laws of motion with their astronomical illustrations, the atomic theory, conservation of energy and of matter, evolution, heredity, the interdependence of animals and of plants, etc. These philosophic questions will offer ample opportunity for training the scholar in writing and in facility of expression.

(*d*) *Human relations*, including English and foreign literatures, ancient and modern history.

Education and Personal Freedom

I would specially emphasise that in our education we need a sense of direction, *i.e.* we must consciously strive to teach the child such things as will

make him a good citizen and a useful citizen. Every study that is retained or that is introduced into the curriculum must be able to establish its claims on these grounds. We shall not accomplish reform simply by resolving to give so many hours more to natural science, or so many hours less to Greek. To make the whole of education scientific we must give it order and direction from beginning to end. When we say that the object of education is to train a child for its future work in life, this does not mean the stunting of education by limiting it, *e.g.* to bookkeeping and shorthand, on the plea that the boy is probably going to undertake a commercial career. The essential part of every man's work in the world is that of a member of a self-governing community, and the training for his political life is as important to the State as the training for his future vocation or handicraft. Any education drawn up with regard to the future career of a child dare not be narrow either on the literary or scientific side or lacking in the formation of character, without enslaving the individual by compelling him to adhere closely to the occupational rut in which he is first placed. It is essential that a certain liberty of choice of occupation be rendered possible, so that any man may be able to change the direction of his activities if, in the course of development, he finds new interests and talents other than those adapted to the occupation for which he was first designed. But the constant question of the parent, "What is the use of this to my boy?" is justifiable, and the schoolmaster should be able to give a satisfactory answer on this point with regard to every hour that the boy spends under his charge.

In urging the introduction of science and of scientific method into education I would insist once again that science is nothing more than the practical experience of mankind throughout all ages, ordered and classified. Science represents, therefore, the eyes of mankind, by using which men may acquire wisdom and gain foresight. A nation without science, like a blind man, must stumble at every obstacle which it meets, thereby falling behind in the race for existence. A reform of our school curriculum by the introduction of natural science as a compulsory study into our schools will not give us this power of foresight, unless we can change at the same time our national attitude towards knowledge and the spirit in which knowledge is imparted in our schools. England has always possessed men distinguished in science, but science as a whole she has starved and neglected. She has preferred to run in the blinkers of the practical man, and has made no effort to establish the necessary connection between the eyes and the brain, between those who have knowledge and those who are entrusted with the direction of her affairs. But the young men of to-day are the leaders of to-morrow; and, if only we can change the spirit of our education, we may look to them for a new era of national life and for a vindication of the claim of democracy to survive as a permanent and dominant type of society.

PHYSICAL RELATIVITY HYPOTHESES OLD AND NEW (G. W. de Tunzelman)

The Relativity Hypothesis, also known as the Relativity Principle, which has recently been developed into a far-reaching theory, has been a subject of controversy and ever-increasing interest amongst physicists for about a quarter of a century. It originated in the failure of the celebrated Michelson-Morley experiment, made in the year 1887, to obtain evidence of the motion of the earth through the ether of space.

The general principles of the universally accepted Faraday-Maxwell repre-

sentation of the transmission of electromagnetic action by means of a medium, as opposed to the theory of action at a distance, showed that any visible optical effects due to the relative motion of ether and matter must, if existing at all, be exceedingly small. For no effect proportional to v/c, the ratio of the velocity of translation of the matter relatively to the ether, to the velocity of light in free ether, about 300,000 kilometres a second, was to be expected, but only effects proportional to the square and to higher powers of this ratio.

It was suggested by Maxwell that it might be possible to detect a difference in the time of propagation of a ray of light between two points at a fixed distance from each other, when the straight line joining them was placed, first in the direction of the earth's orbital motion, and then in a direction at right angles to it. The highest value that could be attained by v would be that due to the motion of the earth through an ether at rest, and the principal component of this, amounting to about 30 kilometres a second, will be that due to the orbital motion. The component due to the earth's rotation is not quite half a kilometre a second, and may be neglected in comparison with the former. There will also probably be a component of unknown amount due to the motion of the whole solar system through space. But this will increase or diminish the component due to the orbital motion according to the position of the earth in its orbit, so that for at least half the year v will have a value of at least that due to the orbital motion.

The experiment was first made by Michelson in 1881, no effect being observed. It showed that, as predicted by theory, there was no first order effect, *i.e.* no effect proportional to v/c, but the method was not considered sufficiently sensitive to prove beyond doubt the absence of any second order effect, *i.e.* one proportional to the square of v/c. The experiment was therefore repeated with much greater refinements of detail by Michelson and Morley in 1887. The essentials of the apparatus employed consisted of a metallic framework with two arms, *OA* and *OB*, at right angles, and as nearly as possible of equal length, provided at their free ends with metallic mirrors perpendicular to them, and with a mirror of unsilvered glass at *O*, with its plane bisecting the angle between them. A convenient source of light was placed on *AO* produced through *O*, and a telescope was fixed with its axis in the line *BO* produced through *O*. The whole arrangement was bolted to a stone block floating in mercury, so that the arms lay in a horizontal plane, and could be rotated slowly and steadily about a vertical axis. A beam of light from the source would then be split up at *O*, part being transmitted to *A*, reflected back to *O*, and part of this reflected along the axis of the telescope to the observer's eye. The other portion of the split beam would be reflected to *B*, reflected back to *O*, and a part transmitted along the axis of the telescope. The smallest difference in the lengths *OA* and *OB* would give rise to coloured interference fringes. The apparatus was adjusted so that the fringes were as sharply defined as possible when *OA* represented the direction of the earth's orbital motion. The ray would then travel from *O* to *A* with the velocity $c+v$, and from *A* to *O* with the velocity $c-v$, while the other part would travel from *O* to *B* and back again with the velocity v. Simple geometry will then show that the time taken by the former will be greater than that taken by the latter by one part in a hundred millions, the ratio v^2/c^2. If the apparatus were then turned through a right angle, making *OA* instead of *OB* perpendicular to the component of orbital motion, calculation shows that for a velocity of 30 kilometres a second the fringes should be displaced by an amount equal to about two-fifths of the distance between two successive fringes, provided the arms *OA* and *OB* were 11 metres in length. The actual arms were much shorter, but their effective length was increased to 11 metres

by an arrangement of additional mirrors reflecting the rays backwards and forwards. The apparatus would have indicated a displacement of about a twentieth of the amount expected, but the result was entirely negative. The expected retardation in time was rather less than the thousand-million-millionth of a second.

If, however, there were no drift through the ether, it would follow that the earth carried a layer of ether along with it, and if this were the case, so also would the other planets. But this would necessarily give rise to eddies in the ether, and other optical results showed that its motion must be entirely free from eddies, or, in other words, free from any trace of spin.

It was suggested, almost simultaneously, by Fitzgerald and by Lorentz, that the null result would be accounted for if the whole apparatus, in common with all solid bodies moving through the ether, underwent an extremely minute contraction in the direction of motion, of an amount which, for speeds comparable to that of the earth's orbital motion, would be proportional to the square of the speed, and for this speed would amount to about one part in two hundred millions, or about 6½ centimetres in the earth's diameter. This seemed a startling proposition, but reason was shown for anticipating some contraction, and a little later, some new observations led Lorentz and Larmor by two very different paths, neither of which had any apparent relation to the Michelson-Morley experiment, to the conclusion that such contraction must occur, and that its amount would be exactly that required to explain the null effect. But other attempts, made in very diverse ways, to obtain evidence of the motion of material bodies through the ether were all defeated by some corresponding compensation, just as though the forces of nature were in a conspiracy to prevent our attainment of the evidence sought for.

It was therefore suggested by some physicists that, under the title of *the principle of relativity*, it should be assumed as an axiom that it is inherently impossible to obtain experimental evidence of the motion of material bodies relatively to the ether. In order to form a starting-point for physical argument, this hypothesis was embodied in a statement to the effect that to any observer, whatever his own motion might be, light will appear to travel through space, that is to say, through ether free from matter, always in straight lines and always at one and the same speed. Some out-and-out relativists even proposed to discard the ether, as a mere framework existing only in our own minds, mainly on the ground that it would simplify the fundamental differential equations of electrodynamics. But this simplification would be obtained at the expense of reducing their content, in that it would leave out of account the existence of so firmly established a phenomenon as the inertia of free-travelling radiation, exhibited in the pressure exerted by a ray of light upon any material body upon which it impinges. And the existence of such a pressure is a direct consequence of the universally accepted Faraday-Maxwell theory, which led to its being sought for and experimentally verified by Lebedeff. Moreover, the discarding of the ether, by making anything in the shape of a mechanical interpretation of electrical action impossible, would render the interpretation of these simplified differential equations in terms of definite physical concepts almost, if not altogether, impossible, and differential equations, although most valuable guides in the interpretation of nature, can hardly be regarded as in themselves constituting such interpretation. The elimination of the ether from our representations of physical phenomena would have the further effect of reopening the age-long controversy between Newton's relativist critics and the supporters of his view that absolute motion in space, and hence also absolute direction in space, are legitimate physical concepts.

According to the older relativity theory, the Copernican system of astronomy

which takes the sun as the centre of our solar system, consisting of sun, planets, satellites, asteroids, comets belonging permanently to it, and, finally, its meteorites and cosmic dust, is not a more correct representation than the Ptolemaic system, which took the earth as centre, but is merely a simpler one, enabling the phenomena to be correlated by a simpler system of mechanics than would be required if the latter were adopted.

Consider further such problems as are presented by the shape assumed by the surface of a liquid in a bucket rotating about a vertical axis through the centre; the figure of the earth, approximately an oblate spheroid; the resistance of a spinning body, such as a top, and of the earth itself, to any displacement of its axis of spin except in either of the two opposite directions in which that axis might be prolonged; the resistance of a swinging pendulum to the displacement of its plane of swing. A freely suspended pendulum of considerable length and mass was, indeed, employed by Foucault as a means of demonstrating the actual, or absolute, rotation of the earth, its movements being such that they could be explained most simply, and, on the basis of the accepted principles of mechanics, could be explained only on the assumption that the pendulum tended to conserve the absolute aspect in space of its plane of swing. Sir Isaac Newton regarded phenomena of this kind as definitely indicating absolute rotations, and therefore absolute directions in space, and has expressed that opinion in unequivocal terms. One of the ablest of his recent critics, H. Poincaré, in *La Science et l'Hypothèse*, objects to Newton's position, that he attempts to explain relations between phenomena by means of the notions of absolute direction and absolute rotation, involving the notion of absolute space, which Poincaré, in common with all the thoroughgoing advocates of the older relativity, ancient and modern, maintain to be physically inconceivable to the relatively constituted human mind. I have dealt elsewhere[1] with this question in much greater detail than is possible within the limits of space here available, and I must confine myself to very briefly summarising the conclusions there aimed at, to serve as a basis for understanding the nature of the new relativity theory, which, unlike the older one, is not built upon a philosophical foundation, but must be regarded rather as a legitimate hypothetical point of view which appears at present to offer considerable promise of leading us, it may be directly or it may be indirectly, to the discovery of new physical truths.

Space, for the philosophic relativist, is the geometrical abstraction of pure extension, and in this sense, absolute space is an expression which does not appear capable of representation in the form of a definite mental concept; but we must guard ourselves against drawing the conclusion that there cannot be physical phenomena indicative of absolute motion and direction in space, for this would be to assume that our minds are to be accepted as measuring the possibilities of the physical universe. Newton's writings show clearly that, although he seldom referred explicitly to a medium filling all space, and never discussed its nature, he firmly believed in its necessity, if only for transmitting the action of gravitation. His attitude on the subject is expressed in the remark that: "Although practically, and at present, nothing is to be accomplished with this conception, we might still hope to learn more in the future concerning this hypothetical medium; and from the point of view of science it would be in every respect a more valuable acquisition than the forlorn idea of absolute space." I therefore consider that we are justified in the conclusion that Newton regarded space as occupied by a

[1] *A Treatise on Electrical Theory and the Problem of the Universe* (1910), Chapter V: The Ether as a Framework for Absolute Motions.

physical reality, the ether, which formed the requisite framework of reference, although, perhaps, too dimly outlined in his mind to enable him to crystallise the concept into a statement which would have carried conviction to the minds of his contemporaries.

Poincare argued, in the work previously referred to, that the inhabitants of a planet, so veiled in clouds that no external bodies would be perceptible, could not infer with certainty that the planet was rotating, but only that such an assumption would provide the simplest basis possible for a system of mechanics. But he so far qualified his contention in a later work, *La Valeur de la Science*, as to admit that the assumption would make it possible to establish so much wider a correlation amongst physical phenomena that it might be considered as of the same order of probability as the reality of the external world. That is to say, he admitted that practical, though not theoretical, certainty of the rotation would be attainable by the inhabitants. And theoretical certainty must certainly be regarded as unattainable in any conclusions derived from experience.

Bertrand Russell has shown, in his *Principles of Mathematics*, that strictly demonstrable *mathematical* concepts of absolute time, and of absolute position and direction in space, are attainable, and he upholds Newton's actual verbal expressions from this point of view. But in his references to absolute motion, Newton was not dealing with the purely mathematical concepts of rational dynamics, but with the physical concepts of practical dynamics—quite another matter.

In order to investigate even so simple a case of motion in space as the path of a raindrop relatively to a fixed point O on the earth's surface, we must choose some framework of reference, and as simple a one as any would be to imagine three straight lines of lengths sufficient to extend to the limits of the field of motion, one east and west—the X axis, say, one north and south—the Y axis, and one vertically up and down—the Z axis. Then the position of any point P, relatively to O, can be specified by the lengths of three straight lines, x, y, z, representing the distances from the planes YZ, ZX, XY, to P, and therefore parallel, respectively, to OX, OY, OZ, reckoned positive if drawn eastward, northward, or upward, and negative if drawn westward, southward, or downward. Then x, y, z are called the co-ordinates of the moving drop, or, strictly speaking, of the centre of the drop, at the time t, reckoned from a moment selected as the starting-point in time. Since we know with great accuracy the motions of the earth relatively to the centre of the sun, we could then determine the motion of the raindrop relatively to the sun's centre. We should simply imagine a similar set of axes with its origin at the sun's centre and parallel to X, Y, Z, respectively at the time $t = o$, the starting-point in time. We could then determine at any time t the co-ordinates of the point O relatively to the sun's centre and the angles between the corresponding axes, and hence the co-ordinates of the point P relative to the sun's centre. Similarly, if we knew the earth's motion through the ether we could find the co-ordinates of P relatively to a set of axes fixed in the ether, the terrestrial axes and the etherial ones being coincident at the time $t = o$. But without knowing the earth's motion through the ether, we can picture it without difficulty. For the ether of present-day physics must be considered as a substance of great density filling all space and allowing material bodies to pass through it very much as a piece of wire gauze can be drawn through mercury, but with the difference that the ether offers *no* resistance to such motion, and that it remains fixed in position like a solid, its smallest portions being capable only of making minute vibrations about their permanent positions, instead of wandering freely from place to place, like the particles of a liquid.

By adopting Riemann's most fruitful suggestion of regarding space as a

manifold, we can form, not quite a physical concept, but what may be called a mathematical concept, of absolute position in space, without the aid of the ether. Consider a straight line of length a as traced out by a moving point, giving a one-dimensional point-manifold. Let the line trace out a square by moving through a distance a at right angles to its direction and in one plane, giving a two-dimensional point-manifold. Let the square trace out a cube by moving through a distance a at right angles to its plane, giving a three-dimensional point-manifold. So far all the points may be considered as existing simultaneously, for no point has been required to move into a position already occupied. And this simultaneous existence is characteristic of the conception of space. The division between two successive positions at every stage in the motion is a division between *here* and *there*. But in forming the line as a manifold of points, the plane as a manifold of parallel straight lines, or the cube as a manifold of parallel planes, we may alternatively regard the division as between *before* and *after*, each thus giving a different representation of the one-dimensional time-flux. Now mathematical analysis, which is nothing but a highly developed system of formal logic, shows beyond the possibility of question that the obstacle to the indefinite continuation of the former process is due entirely to the limitation of our power of visualisation of the relations, and not to anything inherent in the relations themselves. It would therefore be quite legitimate to represent what we call the present state of the universe, not as a division between a past which has ceased to exist and a future not yet existing, but as a division between two continuously existing systems. The three-dimensional space p in which our minds picture the external world, by the correlation of the impressions derived from our senses, a process which begins in earliest infancy, would then form, at any given instant, a division between two portions of four-dimensional space. To an intelligence capable of such a visualisation, they might be pictured as existing simultaneously. If two observers at differents points on the earth's surface desired to compare the results of the study of a moving point, say an agreed point on the moon's surface, their space frameworks would differ, for the axes would in general differ in direction, and be drawn from different origins. Therefore calculations would have to be made, and would be made by Euclid's geometry, which we know by experience to be reliable so far as ordinary astronomical observations are concerned. But whether this would be the case for calculations of the minute accuracy requisite in such a case as the Michelson-Morley experiment we cannot be sure. For the Euclidian geometry involves axioms and implicit assumptions founded on ordinary observation, and these may be only approximately true. Such is Euclid's axiom of parallel straight lines, which leads to the conclusion that the sum of the three interior angles of a plane triangle is equal to two right angles, and Lobachefsky has shown that two different and perfectly self-consistent systems of geometry are derivable from the alternative assumptions that the sum of these three angles is, respectively, greater or less than two right angles. If the difference were small enough, either of the resulting spaces would be indistinguishable from Euclidian space, but in neither of them could a straight line be moved from one position to another without change in size or shape, as Euclid tacitly assumes in the fourth proposition of his first book. Indeed, a straight line, as we conceive it, could not exist, and would be more properly described as a straightest line. The time would be the same for the two observers, but this would not be the case for observers on different planets, and the correlation of the two time systems would depend on the speed of transmission of a light signal through the ether, from one to the other—that is to say, on the very question which it is sought to determine.

Minkowski was the first who succeeded in finding a space-time framework which would be the same for every observer, wherever and whenever he might be. He showed that this would be the case for a system of four axes at right angles to one another in a four-dimensional manifold representing space and time together, but without distinguishing between them. He called this space-time, and showed that, while it remained invariable, its resolution into space and time separately gave rise to components which depended on the motion of the observer. It will be obvious from what has gone before that space-time looked at from another point of view would be identical with a four-dimensional space-manifold, so that the geometrical treatment developed for dealing with the latter will be immediately applicable to the former.

The possibility now appears that this demonstrated variability of the space and time framework of ordinary physics may be of such a nature as to nullify, by an exact compensation, any and every evidence which we might otherwise hope to obtain by experience of the contraction of material bodies in consequence of their motion through the ether, just as no conceivable experiment could make a man aware of an increase or decrease, however great, in his own size, if the whole scale of nature simultaneously changed in the same proportion. The assumption, as a working hypothesis, that this possibility may prove to be true, provides a perfectly legitimate basis for a physical theory, and now forms the starting-point of what physicists mean by the expression *theory of relativity.*

The ultimate aim of all physical theory in the present stage of our knowledge is the expression of phenomena in terms of strains set up in the ether and of the stresses which give rise to them. The relativist's aim is, therefore, to show that a more complete correlation of physical phenomena is obtainable in terms of stresses and strains of the ether of the Minkowski time-space than in terms of the older theory. Even, however, should the relativity theory reach a development realising this expectation, we should, so far as we can see at present, have to translate our results into terms of the older theory, or something corresponding to it, before we could form physical conceptions of them—that is to say, before we could even think intelligently about them except in so far as we could satisfy ourselves with mere mathematical concepts. We should, however, expect to be able, by means of the wider theory, to resolve some of the difficulties at present confronting us, and then to revise the existing theory more or less completely, without transcending the concepts of space and time to which our powers of visualisation are restricted.

No physical theory that human intelligence is capable of constructing can ever be more than a very imperfect model of natural phenomena. Should it prove possible, and it appears probable that it may, mathematically to extend our modelling beyond our powers of direct physical conception, we shall have to apply a similar process to such models, modelling them in their turn, in order to bring them within our limits of physical conception.

Newton's mechanical world-models, and those of his immediate successors, were imagined as built up of material particles of size apprehensible to our senses, and any such system possesses the property of a mechanism—that the relations between its constituent parts at any instant are determinable, provided our mathematical methods are sufficiently powerful, from a knowledge of the relations at an instant immediately preceding or succeeding it. That is to say, from a complete determination of the system at any one instant we can deduce its state at any past or future time. Newton's own mathematical methods, great as was their advance on anything previously known, were adequate only to the solution of comparatively simple problems. Lagrange extended them so far as to give a

complete solution of any mechanical system which can be completely specified in terms of co-ordinates. And these co-ordinates need not be of the simple character previously referred to, but may be any measurable quantities of any nature, provided their number is sufficient to determine the system, *e.g.* in the case of a system of material particles, three times the number of particles requiring distinct consideration. But in many modern problems relating to molecular and electrical phenomena the number of co-ordinates involved is practically infinite, *e.g.* in the case of the molecules in a gas. If, however, such systems remain, during the period considered, in a steady state, *i.e.* free from changes in constitution, closely approximate solutions may be obtained in terms of a limited number of co-ordinates obtained by a process of averaging. That is to say, the system must be one which can be treated as capable of being brought back to any previous state by purely mechanical means, such as reversing the sensible velocities. The Lagrangian equations can be most easily obtained by Sir William Rowan Hamilton's *principle of least action*, which is applicable to all such systems. The potential energy, W, of such a system can be expressed in terms of the co-ordinates only, and the kinetic energy, T, in terms of the co-ordinates and their first derivatives with respect to the time. The action determining the motion is taken as the difference, $T - W$; and the principle asserts that when the system passes from its state at one instant to its state at any other, the path followed—*i.e.* the sequence of changes through which the system passes—will be such that the average value of this difference, during the interval of time occupied by the passage, will be smaller for the actual path than for any other possible closely adjacent path. The principle is expressed mathematically by the equation—

$$\delta\int_{t_0}^{t_1}(T - W)dt = o,$$

where t_0 and t_1 are the initial and final values of the time.

Hilbert succeeded, in 1915, in generalising Hamilton's principle into a form suitable for application to Minkowski's four-dimensional manifold, and in the same year Einstein obtained a set of equations representing the generalised law of gravitation as viewed from Minkowski's standpoint, and succeeded by their aid in arriving at a practically exact solution of a problem which had baffled astronomers for several generations—the considerable discrepancy between the observed motion of the perihelion of the planet Mercury, and the value predicted by Newton's law of gravitation.

These results appear to warrant Lorentz's conclusion that the relativity theory may now be said to have taken a definite form, and I propose in another article, to which the present one may be regarded as preliminary, to deal with what I think I am justified in calling the general theory of relativity.

THE ELECTRIFICATION OF SEEDS (Charles Mercier, M.D., F.R.C.P.)

THIS process has now reached a stage of practical success that requires some notice in SCIENCE PROGRESS. Begun with experiments in a few pots, it has advanced year by year to larger and larger plots of ground, until at the present time electrified seed is being grown on more than 2,000 acres. These 2,000 acres are now ripe for harvest, and in many cases are being harvested, so that a decisive judgment on the merits of the process can now be formed, and it may be said at once that the judgment of competent experts who have visited the farms and

examined the crops is all one way, and is extremely favourable. These experts include representatives of several foreign Governments, of several Colonial Governments, of the Indian Government, of our own Government, of important newspapers, as well as men who are distinguished in the world of agriculture, and whose names are widely known as experts on this subject.

The various Government representatives will of course report to their respective Governments, and it would not be becoming of me to forestall their reports, though in several cases the general trend, and even the specific recommendations, have been communicated to me without any injunction as to secrecy; but I am violating no confidence when I say that, so far as they are known to me—and only one gentleman carried official reticence to the point of giving no indication either way—they are uniformly favourable, and for the most even enthusiastic.

The differences between the crops grown from the treated and those grown from the untreated seed are manifold, and it is important to remember that in every case the treated and the untreated seed were taken from the same bulk, were sown on the same land, in the same field, at the same time, were subjected to the same cultivation, were examined at the same time, and, those that are reaped, reaped on the same day. It is important also to bear in mind that in all the cases to which reference is made here the crops were grown by practical agriculturists under ordinary farming conditions, for their own satisfaction, and with a view to the adoption or rejection of the electrified seed in future years. Such tests have not the rigorous scientific exactitude of tests made by scientific experts at experimental stations, but they are not less trustworthy as guides to the practical farmer. Experiments in the scientific manner have been carried out at five or six experimental stations, but the reports are not yet to hand, and therefore cannot be given here.

A practical agriculturist who has spent his life in farming can estimate within a small margin of error, by mere inspection of a growing crop of grain immediately before harvest, how many bushels per acre the crop will yield. The estimate seems to the non-expert a risky one, and one likely to be falsified by the threshing machine; but in practice it is not found to be so. The *flair* of the farmer in scenting the yield of his crop is analogous to the *tactus eruditus* of the physician, to the judgment of the cloth merchant as to the wearing quality of a cloth, to that of a wine merchant as to the value of a fine wine, or that of a tea-taster as to the proper blend of varieties of leaf. These things are not to be measured or expressed in accurate figures. They are personal judgments, made possible by long experience and attention based upon native capacity. Yet great trades are built up upon them, and serious errors are almost unknown. Such is the skill of the expert farmer in gauging the yield of his crops. The Government has such reliance on his skill that it publishes every year the probable yield of the harvest or ever a field has been reaped, and the estimate is never much astray.

The first and most important difference between the crops grown from electrified seed and those grown from unelectrified is the increase in the yield of the former. Estimated in the way above described, half a dozen or more experts have agreed that crops they have inspected have shown a difference of from eight to twelve bushels per acre. In the case of wheat, this means a difference of from 25 to 37 per cent. Estimates made of other crops by experts inspecting singly have been in some cases less, in other cases more than this, but probably, as far as can be judged from the reports that are as yet to hand, the figures mentioned represent about the average.

The second difference is that the crops from electrified seed show a greater weight per bushel, ranging from one to four pounds. This is very important, as every one who keeps horses knows. It means the difference between a poor sample and a mediocre sample. It means the difference between a mediocre sample and a first-rate sample. It means the difference between a sample that can be used only for milling and a sample that can be used for seed. It means better milling quality, more flour, and less offal.

The third difference is in the length of the straw. This is susceptible of actual measurement, and it is found by measurement that the straw growing from the electrified seed is from two to as much as eight inches longer than that growing from the untreated seed.

The fourth difference is in the stoutness and the strength of the straw. 75 culms of unelectrified oat straw tied up in a bundle measured $3\frac{3}{4}$ in. in circumference. The same number of culms from electrified seed measured $4\frac{3}{4}$ in.—an excess of $26\frac{2}{3}$ per cent.

From this follows the fifth difference, that the crop from electrified seed stands better than that from unelectrified seed. After the thunderstorms at the end of July, in field after field the electrified crop was standing upright, while the adjacent unelectrified seed was, in large patches, flat upon the ground.

The sixth difference is that the electrified seed tillers much more than the unelectrified—that is to say, it throws up many more culms, and therefore each plant occupies more ground and produces more ears in the case of wheat and barley, more panicles in the case of oats. Consequently, the same quantity of seed produces a heavier crop, or a smaller quantity of seed may be used to produce the same amount of crop. In one case—on the farm of Mr. Legg, near Corfe Castle—5 acres were sown with electrified oats at $3\frac{1}{2}$ bushels per acre, and 5 acres adjoining were sown with oats of the same sample, but unelectrified, at 4 bushels per acre. The produce of the electrified was a much thicker plant, stouter and longer straw, more numerous and heavier ears of grain.

These results are established. They are not uniform. Every crop does not exhibit the same degree of contrast as every other, but practically every crop shows a substantial contrast—much more than enough to repay the cost of the treatment. The process is now past the experimental stage, and is become established. It would be absurd to treat it as still uncertain when such results have been shown on 2,000 acres of land of the most various quality in widely different parts of the country.

The rationale of the process is unknown. Whether it acts by stimulating the energy locked up in the ungerminated seed; or by the addition by ionisation of ions that assist the growth of the seed; or by destroying certain bacteria, or the spores of certain fungi; or by stimulating the growth of other bacteria or other fungi; or in some other way, is not known. Here is a most fertile field for investigation, for at the present time we are feeling our way in the dark. It is found, for instance, that, to obtain equivalent results, barley must be treated for twice as long as wheat. Every kind of seed needs its own special treatment, which can be determined only by a long and careful course of experimentation, extending, it may be, over several seasons. For many seeds the correct mode of treatment has not yet been discovered, and up to the present no result has been obtained; but there is every reason to suppose that results will be obtained in time. It is evident that if the rationale of the process were known, this time might be very sensibly abbreviated. Here, then, is a field of investigation waiting and clamouring to be cultivated, and certain to achieve important results.

Postscript

Since the foregoing account was written, some of the measured results of the harvest of 1918 have come to hand. They show gains from the crops growing from electrified seed over the crops growing in the same fields from unelectrified seed of from 9 per cent. to more than 60 per cent., the average being more than 30 per cent. *Verbum sapientis.*

ISOSCELES TRIGONOMETRY: Suggested Additions to the Trigonometrical Ratios (Sir Ronald Ross)

1·0. AT present trigonometry is based entirely upon the right-angled triangle; but a useful addition can be founded upon the isosceles triangle. Thus, for the solution of triangles, any right-angled triangle may be divided into two isosceles triangles by drawing a straight line from the right angle so as to bisect the opposite side; and hence any triangle may be discussed in terms of four isosceles triangles. For this purpose it is useful to add two trigonometrical ratios to the list in use. If A is the angle between the two equal sides of an isosceles triangle, we may denote by $\text{bas}\,A$ the ratio of the base to one of the sides; and if sA is the supplement of the angle A, we may denote $\text{bas}\,(\pi - A)$ by $\text{bas}\,sA$. We then obtain the following easily verified relations:

1·1. $$\text{bas}A = 2\sin\frac{A}{2} = \sqrt{1+\sin A} - \sqrt{1-\sin A} = 2\sin A\left\{\sqrt{1+\sin A}+\sqrt{1-\sin A}\right\}^{-1}$$

$$= \sqrt{2(1-\cos A)} = 2\sqrt{1-\cos^2 A/2} = \sin A\,(\cos A/2) = \text{etc.}$$

$$\text{bas}sA = 2\cos\frac{A}{2} = \text{etc.}\quad \text{bas}A + \text{bas}sA = \sqrt{1+\sin A}\quad \text{bas}A - \text{bas}sA = 2\sqrt{1-\sin A}$$

$$2\sin A = \text{bas}A\,\text{bas}\,sA\quad 2\cos A = \text{bas}\left(\frac{\pi}{2}-A\right)\text{bas}\left(\frac{\pi}{2}+A\right) = (2-\text{bas}^2 A).$$

1·2. $$\text{bas}\,s^2A = \text{bas}\,A$$

$$\text{bas}^2A + \text{bas}^2 sA = 4$$

$$2\,\text{bas}(A \pm B) = \text{bas}\,A\ \text{bas}\,sB \pm \text{bas}\,sA\ \text{bas}\,B$$

$$2\,\text{bas}\,s(A \pm B) = \text{bas}\,sA\ \text{bas}\,sB \mp \text{bas}\,A\ \text{bas}\,B$$

$$\text{bas}\,2A = \text{bas}\,A\sqrt{4-\text{bas}^2A} = \text{bas}\,A\ \text{bas}\,sA = \text{bas}\,2sA$$

$$\text{bas}\,\tfrac{1}{2}A = \sqrt{2 \pm \text{bas}\,sA} = \sqrt{2-\sqrt{4-\text{bas}^2A}}$$

$$\text{bas}\,s\tfrac{1}{2}A = \sqrt{2+\text{bas}\,A}$$

$$2\,\text{bas}(A \pm B) = \frac{\text{bas}\,2B}{\text{bas}\,B} \pm \frac{\text{bas}\,2A}{\text{bas}\,A}$$

$$\text{bas}(A+B)\text{bas}(A-B) = 2\cos B - 2\cos A = \text{bas}^2A - \text{bas}^2B$$

$$\text{bas}\left(\frac{\pi}{2} \pm A\right).\sqrt{2} = \text{bas}\,sA \pm \text{bas}\,A$$

$$\text{bas}\left(A \pm \frac{\pi}{2}\right).\sqrt{2} = \text{bas}\,A \pm \text{bas}\,sA$$

$$\text{bas}\ (\pi \pm A) = \mp\,\text{bas}\,sA$$

$$\text{bas}\,s(\pi \pm A) = \mp\,\text{bas}\,A$$

$$\text{bas}\ (2\pi \pm A) = \mp\,\text{bas}\,A$$

$$\text{bas}\,s(2\pi \pm A) = \mp\,\text{bas}\,sA$$

$$\text{bas}^4\frac{A}{2} - 4\,\text{bas}^2\frac{A}{2} + \text{bas}^2A = 0$$

$$\text{bas}^3\frac{A}{3} - 3\,\text{bas}\frac{A}{3} + \text{bas}\,A = 0.$$

1·3. $\dfrac{d\,\text{bas}\,x}{dx} = \tfrac{1}{2}\,\text{bas}\,sx \qquad \dfrac{d\,\text{bas}\,sx}{dx} = -\tfrac{1}{2}\,\text{bas}\,x$

$\dfrac{d\,\text{bas}^{-1}x}{dx} = \dfrac{4}{\sqrt{4-x^2}}$

1·4. $\text{bas}\,A = \text{bas}\,s\dfrac{A}{2}\,.\,\text{bas}\,s\dfrac{A}{4}\,.\,\text{bas}\,s\dfrac{A}{8}\,\ldots\,\text{bas}\,s\dfrac{A}{2^n}\,.\,\text{bas}\dfrac{A}{2^n}$

$= \text{bas}\dfrac{A}{n}\,.\,\text{bas}\dfrac{2\pi+A}{n}\,.\,\text{bas}\dfrac{4\pi+A}{n}\,.\,\text{bas}\dfrac{6\pi+A}{n}\,\ldots\,\text{bas}\dfrac{(n-1)2\pi+A}{n}$

$\text{bas}\,A\,.\,\text{bas}\,2A\,.\,\text{bas}\,3A\,\ldots\,\text{bas}\,nA = \text{bas}(n+1)A.$

1·5. $\text{bas}\,\pi = 2 \qquad \text{bas}\,s\pi = 0$

$\text{bas}\dfrac{\pi}{2} = \sqrt{2} \qquad \text{bas}\dfrac{\pi}{4} = \sqrt{2-\sqrt{2}} \qquad \text{bas}\dfrac{\pi}{8} = \sqrt{2-\sqrt{2+\sqrt{2}}}$

$\text{bas}\dfrac{\pi}{3} = 1 \qquad \text{bas}\dfrac{\pi}{6} = \sqrt{2-\sqrt{3}} \qquad \text{bas}\dfrac{\pi}{12} = \sqrt{2-\sqrt{2+\sqrt{3}}}$

$\text{bas}\dfrac{\pi}{5} = \dfrac{\sqrt{5}-1}{2} \qquad \text{bas}\dfrac{\pi}{10} = \sqrt{2-\tfrac{1}{2}\sqrt{10+2\sqrt{5}}}.$

1·6. Prove that $\text{bas}\,\theta<\theta$ and $\text{bas}\,\theta>\tfrac{1}{2}\theta\,.\,\text{bas}\,s\theta$; or $\text{bas}^2\theta>\dfrac{4\theta^2}{4+\theta^2}$. So that

$\theta>\text{bas}\,\theta>\dfrac{2\theta}{\sqrt{4+\theta^2}}$. Put $\theta=\dfrac{\pi}{n}$. Then

$\dfrac{\pi}{n}>\text{bas}\dfrac{\pi}{n}>\dfrac{2\pi}{\sqrt{4n^2+\pi^2}}$. Difference is $\dfrac{\pi}{n}-\dfrac{2\pi}{\sqrt{4n^2+\pi^2}}=\pi\left(\dfrac{1}{n}-\dfrac{1}{n\sqrt{1+\left(\dfrac{\pi}{2n}\right)^2}}\right)$.

·7. $(\text{bas}\,s\theta+i\,\text{bas}\,\theta)^n=\text{bas}\,sn\theta+i\,\text{bas}\,n\theta$ by De Moivre's theorem.

·0. For the solution of triangles, if a, b, c are the sides and A, B, C the opposite angles, we have

$$\frac{a}{b}=\frac{\text{bas}\,2A}{\text{bas}\,2B}=\frac{\text{bas}\,A\,\text{bas}\,sA}{\text{bas}\,B\,\text{bas}\,sB}$$

$$a^2=b^2+c^2-bc(2-\text{bas}^2A)=(b-c)^2+bc\,\text{bas}^2A=b^2+c^2-bc\frac{\text{bas}\,4A}{\text{bas}\,2A}.$$

It is useful to write $\text{bas}(b, c, A)\equiv\sqrt{(b-c)^2+bc\,\text{bas}^2(1, 1, A)}$. Then

$$\text{bas}(b, c, A) = b\frac{\text{bas}\,2A}{\text{bas}\,2B} = c\frac{\text{bas}\,2A}{\text{bas}\,2C}, \text{ etc.}$$

Also $\text{bas}^2A = 4\dfrac{(s-b)(s-c)}{bc} \qquad \text{bas}^2sA = 4\dfrac{s(s-a)}{bc}$

where s is half the sum of the sides.

3·0. Exponential values.

$i\,\text{bas}\,2x=e^{ix}-e^{-ix} \qquad \text{bas}\,s2x=e^{ix}+e^{-ix}$

$\text{bash}\,2x=e^{x}-e^{-x} \qquad \text{bash}\,s2x=e^{x}+e^{-x}.$

4·0. Iteration (see SCIENCE PROGRESS, October 1918).

$$[\text{bas}\,O]^n=O-\frac{n}{3!}\frac{O^3}{2^2}+\frac{n(5n-4)}{5!}\frac{O^5}{2^4}-n\frac{175n^2-336n+164}{3\cdot7!}\frac{O^7}{2^6}+\text{ etc.}$$

$$\text{bas}\left[\frac{O}{2}\right]^n=\text{bas}\frac{O}{2^n}=\left[\sqrt{2-\sqrt{4-O^2}}\right]^n\text{bas}\,O \text{ (from 1·2).}$$

$$\text{bas}\,n\pi\,.\,\sqrt{4-O^2}+\text{bas}\,sn\pi\,.\,O=\left[\sqrt{4-O^2}\right]^n.$$

[NOTE.—It will be observed that the new functions have a periodicity of 4π; and hence, to a value of bas x (say) between 0 and π, only one value of x belongs, whereas to sin x (say) two values belong. Thus it seems that these interesting functions may be also of practical importance in avoiding "the ambiguous case" in the solution of triangles.—P. E. B. J.]

ESSAY-REVIEWS

MATERNITY AND CHILD WELFARE, by T. N. KELYNACK, M.D., on **The Carnegie United Kingdom Trust**: Report on the Physical Welfare of Mothers and Children, Vol. III., Scotland. By W. Leslie Mackenzie, M.A., LL.D., M.D., D.P.H., F.R.C.P E., F.R.S.E., Medical Member of the Local Government Board for Scotland. [Pp. xxviii + 632, with Map, Charts, and Illustrations.] Issued from the Headquarters of the Carnegie United Kingdom Trust, East Port, Dunfermline, Scotland, and Printed by Messrs. Neill & Co., Ltd., Edinburgh.

IN the April number of SCIENCE PROGRESS there appeared a review of volumes of the Carnegie United Kingdom Trust Report on the Physical Welfare of Mothers and Children dealing with the problem as it relates to England and Wales and Ireland. We are now able to draw attention to the last volume of this notable series, "Scottish Mothers and Children," which has been compiled, and in great part written by Dr. W. Leslie Mackenzie. It should be said at once that the members of the Carnegie United Kingdom Trust, in providing for the preparation and issue of these most comprehensive volumes, have accomplished a work of the greatest national importance, the benefits of which will be permanent. Dr. Leslie Mackenzie has been particularly fortunate in obtaining the service of a number of medical experts to assist him in the preparation of his monumental volume, and loyal co-operation has come from authorities and individual workers in all parts of Scotland. The work is a complete and detailed exposition of all aspects of the question of Maternity and Child Welfare as it relates to the Northern Kingdom. The Report opens with a particularly instructive and suggestive introduction descriptive of the Distribution of the Scottish people and the General and Special Aspects of the Problems of Motherhood and Childhood. Then follow chapters on Provisions for Prematurity and Maternity, and the needs generally of Expectant and Nursing Mothers. To this essential part of the subject Dr. J. W. Ballantyne contributes a very complete study on "The Care of the Expectant Mother and her Unborn Infant: a Problem in Present Day Public Health." Dr. Mackenzie's report will long remain a standard work of reference, for it is a mine of statistical and other detailed information touching every part of the great and far-reaching subject of which it treats. The volume is admirable in its arrangement. Each chapter is carefully ordered, the matter being scientifically presented, and having also a helpful section devoted to conclusions. There are special chapters on the working and effects of Maternity Benefit, Provision for Mother and Child under the Poor Law, Care of the Unmarried Mother and her Child, the Employment of Expectant and Nursing Mothers, and the Feeding of Mothers and Children. The Report has been prepared with statesmanlike breadth of view, wide knowledge, sound judgment, deep sympathy, and real vision. It is the most informing, suggestive, and stimulating report of its kind which has ever been published. The subjects dealt with are viewed in their relationship to national, municipal, social, domestic, and economic conditions. There are separate chapters on the

Housing of the Mother and Child, and the Provision for the Protection of Infant Life, and an excellent account is also given of the aims and work of the Scottish Travelling Exhibition of Maternity and Child Welfare Work. An illuminating section is devoted to the consideration of the Birth-rate. The Report is not limited to a discussion of problems of infant life, but a large part of the volume is devoted to the consideration of questions touching the welfare of Pre-School Children and Children of School Age. To the chapter on the General Care for the Medical Supervision of the Pre-School Child, Dr. Lewis D. Cruickshank contributes a statement regarding the Prevalence of Defects among School Entrants. The information given regarding deaths and death-rates of pre-school children, and the causes of morbidity and mortality among this class deserve fullest study. There is an excellent chapter on Provisions for Tuberculous children, and particulars are given of existing agencies for necessitous cases in Children's Hospitals, Convalescent Homes, Centres for Crippled and Invalid Children, Holiday and Rest Homes, Recuperative Schools and other Institutions. A series of Chapters are devoted to the consideration of the Day Care of Children, in Crêches or Day Nurseries, Toddlers' Playgrounds, Play Centres and Open Spaces, Kindergartens and Educational Institutions. Special chapters deal with such subjects as the Provision of Nurses in Scotland, Medical Service in the Highlands and Islands, Reports by medical observers well acquainted with the problems of the district described. The reports regarding the local conditions existing in various parts of Scotland will be of immense value in aiding schemes for reconstruction and readjustment of existing measures dealing with the organisation and administration of central and local agencies working for the improvement of human conditions. Dr. Leslie Mackenzie has devoted the best part of an exceptionally industrious life to scientifically directed endeavour for the betterment of human life in Scotland, and in this volume he has presented in condensed form much of the knowledge he has accumulated. It is no dry and dreary formulation of facts, but is vitalised throughout by the spirit and purpose of the highest patriotism. The volume closes with a striking epilogue from which we make bold to take the following extract: "The war clouds dim our vision of the facts at home. But in war or peace, there is this constant struggle for a living and a life. To-day it is the mother in her distress that needs help and care; to-morrow it is the infant, new born; the third day the one or the other is found—the tale of inadequate service, of danger, of damage, of disease and death is nearly the same, varying a little in local colour but never in substance. . . . The laws that supplement and support the innumerable impulses of philanthropy are not measures that can be formulated, passed, and enforced once for all; they are themselves the means and symbol of a progressive and continuous organisation of the social energies, adjusting them in degree to all the phases of distress, disablement, and disease. The full development of the powers written in the law this generation will not see, but it is in this generation that they have taken their new form. In all the varying phases of these national problems—the concentration of the peoples, the growth of institutions, the rise of new organisations and agencies—there is visible always one uniting principle: the need to preserve the life of the new-born child." We understand the Carnegie Trust are now considering ways and means whereby the Scottish people may be provided with a National Institute of Maternity and Child Welfare. We trust that this great purpose may speedily be brought to fruition. The noble volume of Dr. Leslie Mackenzie and his loyal coadjutors has been as good seed which has fallen on a soil from which rich harvests may be garnered.

REVIEWS

HISTORY OF SCIENCE

Studies in the History and Method of Science. Edited by DR CHARLES SINGER. [Pp. xiv + 334, with 41 plates and 24 illustrations in the text.] (Oxford: At the Clarendon Press, 1917. Price 21*s*. net.)

THIS most remarkable book contains a collection of seven essays; the first-fruits of an effort made by the Editor and his wife to stimulate research on the history of the development of science, for which purpose they have provided £100 a year for five years for the provision of fittings and books in a room set apart for the purpose at the Radcliffe Library at Oxford. Four of the essays deal with mediæval science of the period A.D. 1100—1500, and, with the exception of one on Early Renaissance Anatomy, might better be termed Studies in the History of Superstition and Ignorance. Their interest is undeniable; their usefulness is less obvious, and they can hardly be regarded as a serious contribution to the *History of Science* as that subject is generally understood. This criticism is perhaps a little ungracious; it is directed rather at the title than the contents, and in wishing the volume the success it deserves we are hoping, with its sponsors, that it may become the forerunner of a series of similar publications.

The first essay, written by Dr. Singer, contains an account of the "Scientific Views and Visions of Saint Hildegarde"—a remarkable but a somewhat obscure and uncanonised *religieuse* who flourished in the Rhineland in the middle of the twelfth century. Her views may apparently be regarded as a reflection of those obtaining in the "scientific" circles of her day, and her writings cover the whole field of the theory of the macrocosm and microcosm in an "attempt to demonstrate a relationship between the nature of the Godhead, the constitution of the universe, and the structure of man." The essay is illustrated by some beautifully reproduced diagrams in colour from various manuscripts of her writings.

The Editor's other contribution to the collection is the important essay on Renaissance Anatomy already referred to. It is written round an MS. by Hieronymo Manfredi, in the Bodleian Library, which has hitherto escaped notice. Manfredi was primarily an astrologer, but occupied the Chair of Medicine at Bologna. His Anatomy was an amplified edition of Mondino, and was probably the first complete treatise written in Italian. It gives a very clear conception of the subject as it was known at the end of the fifteenth century.

"The Blessing of Cramp-rings—a Chapter in the History of the Treatment of Epilepsy," by Raymond Crawfurd, and "Dr. John Weyer and the Witch Mania," by Dr. E. T. Withington, are both essays in which the historic rather than the scientific interest is dominant. They are both records of superstition, and the former contains much curious information. Thus:

> "A silver ring, made of five sixpences, contributed by five different bachelors, conveyed by a bachelor to the hand of a smith that was also a bachelor, was another reputed remedy for epilepsy; and its virtue was enhanced, if none of the bachelors knew for what purpose or to whom it was given."

It is suggested that the rings were first used as a remedy for epilepsy as an outcome of the "belief that an epileptic seizure may be aborted by ligature of a limb, or part, above the situation in which the warning 'aura' commences." Dr. Withington's work is a record of the appalling cruelties of the witch mania which swept over Europe in the fifteenth and sixteenth centuries. He discusses its possible origin and the part played by Dr. Weyer in its decline.

The "Tractatus de Causis and Indiciis Morborum," by Reuben Levy, is merely a critical discussion of an unpublished Hebrew MS. bearing this name and attributed to Maimonides (the Rabbi Moses Ben Maimon). It is shown that the work is probably by another author. As an example of a literary research it is, no doubt, valuable and admirable, but it seems somewhat out of place in this collection, even though it be regarded as an introduction to a subsequent discussion of the subject-matter of the MS.

The two remaining essays are on "Vitalism," by Dr J. R. Jenkinson, who, as a Captain in the 2nd Royal Fusiliers, was killed in action in Gallipoli, and on "Scientific Discovery and Logical Proof," by F. C. S. Schiller. Dr. Jenkinson wrote from a philosophic standpoint, historically and critically. He may be said to have leant towards the view that the progress of science would best be served by adherence to the "simpler philosophy" of mechanistic ideas. The last essay forms an incisive attack on the aloofness and uselessness of formal logic, and indicates the path of reform whereby, by the sacrifice of its "inhuman" infallibility, it would come into contact with science and with life, and exercise a salutary influence on the development of both.

D. O. W.

ASTRONOMY

Textbook on Navigation and Nautical Astronomy. By J. GILL. Revised and enlarged by W. V. MERRIFIELD, B.A. [Pp. viii + 438 + (12), with 214 figures.] (London: Longmans, Green & Co., 1918. Price 21*s*. net.)

THIS work was written primarily to meet the requirements of officers of the Merchant Service. The ground which it covers is such that candidates for Board of Trade Certificates will find it a suitable textbook in preparation for all grades of the Examination for Mates and Masters, both as regards theory and practice.

In the new edition several valuable improvements have been made, the chief of which are: (1) An appendix is added giving necessary extracts from the *Nautical Almanac* in the form in which they are given in that work; the examples are such that any Nautical Almanac data required can be obtained from the appendix, and the student is thus familiarised with the use of the Almanac. (2) Extracts from the Admiralty Tide Tables are also given in an appendix, and the chapters dealing with tides have been rewritten to suit these tables. (3) The astronomical examples are fresh and solutions are given, not only by the use of formulæ derived in the text, but, wherever possible, they are also obtained direct from a figure. This is a very wise plan, as it calls the student back to first principles and tends to prevent a rigid and too often unintelligent dependence upon formulæ. (4) The book has been rearranged so that proofs of problems, instead of being collected in one chapter at the end, have been given in with the text of the chapters themselves.

Other modifications include an entire rewriting of the chapters on plane and spherical triangles, and the addition of chapters on projections and the correction of altitudes. The latter is a useful addition; the former seems distinctly out of place as Chapter I, preceding the chapter containing definitions. A student

approaching the subject by himself would not understand this chapter at all, assuming, as it does, a knowledge amongst other things of meridional parts. It is, moreover, extremely condensed. In so far as the purpose for which it was inserted—viz. to enable the student when working any problem to construct a good figure—is entirely commendable, it is the more to be regretted that this chapter was not made clearer and amplified.

An important feature of the work is the wealth of numerical examples which are solved in full in the text, so enabling the student to obtain a thorough grasp of the practical problems. Many other problems are given for the student to work himself, numerical results being given in the appendix.

The remark on p. 143 that "treacle, oil, or tar" would do as well as mercury for the artificial horizon is rather misleading. Though they might in emergencies be used as substitutes, they would be very poor substitutes.

The book is without an index—a regrettable omission, as the student will occasionally find some trouble in searching for information on a particular point, owing to the large amount of material which the book contains. It is to be hoped that in any future edition this want will be supplied.

The book is as complete a textbook as any that we know, and can be thoroughly recommended.

H. S. J.

The Destinies of the Stars. By SVANTE ARRHENIUS, Ph.D. Authorised translation from the Swedish, by J. E. FRIES, Fellow A.I.E.E. [Pp. xviii + 256, with 29 figures and plates.] (New York and London: G. P. Putnam's Sons, 1918. Price *7s. 6d.* net.)

THIS volume contains a series of seven articles on various problems in cosmogony by the well-known Swedish scientist, Dr. Svante Arrhenius, President of the Nobel Institute, Stockholm, the intention of the author being to fill gaps in his previous works on cosmogony. The principal of these are *Worlds in the Making* and *Life of the Universe as conceived by Man from Earliest Ages to the Present Time*. Since these were written considerable progress has been made in our study of the relation of the stars to the Milky Way and in our observational knowledge of our neighbouring planets. Although, therefore, there is a definite line of thought traceable through the seven articles in this volume, any one of them is complete in itself and can be read independently of the others.

The introductory article is a reprint of a lecture delivered before the Fourth International Congress in Bologna, 1911, dealing with the "Origin of Star-Worship." The thought connecting this with the remainder of the volume is that the ancients believed—and the belief persisted until comparatively recent times—that the fates of men could be read in the stars. Although we now know that this is not literally the case, yet in a certain different sense it has proved to be true. A study of the Milky Way and the relation to it of the distribution of the stars through space throws much light on the process of evolution of our stellar systems, whilst from observation of the planets, combined with interpretations based upon the physical phenomena with which we are acquainted on this earth, we can learn the conditions that existed here at the dawn of life, and can predict the ultimate fate that will befall the latter descendants of present generations. It is with such problems that the greater part of the book is concerned.

The titles of the remaining chapters will sufficiently indicate the nature of their contents: The Mystery of the Milky Way; The Climatic Importance of Water

Vapour; Atmosphere and Physics of the Stellar Bodies; The Chemistry of the Atmosphere; The Planet Mars; Mercury, the Moon, and Venus.

There are not many statements in the book which can be criticised; we think, however, that there are few astronomers who will agree to the explanation of the dark holes in the Milky Way as due to the sweeping away by star-throngs of all matter in their courses. Barnard's hypothesis of dark nebulæ has much more to commend it.

The chapter on the planet Mars is well worth reading. Dr. Arrhenius discusses the theory that intelligent men exist on Mars in a critical and broad-minded manner, and his conclusion that the theory must take its place in the shadowy realm of dreams will be acceded to by most unbiassed thinkers. "The trouble with these explanations is that they explain anything, and therefore in fact nothing."

Dr. Svante Arrhenius is a writer who possesses in a marked degree the gifts of lucidity of exposition and of an excellent literary style. These qualities have lost nothing in the translation from the Swedish of Mr. Fries, who is to be congratulated upon its excellences. The book is well illustrated by numerous figures and recent astronomical photographs.

H. S. J.

An Introductory Treatise on Dynamical Astronomy. By H. C. PLUMMER, M.A., Royal Astronomer of Ireland. [Pp. ix + 343.] (Cambridge: University Press, 1918. Price 18*s*. net.)

THERE is a singular deficiency of works in the English language dealing with the important branches of astronomy which are comprised under the general heading of dynamical astronomy. This is surprising and regrettable when the important contributions which have been made to the subject by English and American astronomers are considered. It is sufficient merely to mention the names of Adams, Darwin, Hill, Newcomb, Cowell, Brown, and Leuschner, amongst others. The publication by Prof. Plummer of the volume under review is therefore an event of importance.

The amount of ground which is covered is surprising. The contents of the book may be briefly summarised as follows: Chapters I to VI are devoted to preliminary matters mainly concerned with the undisturbed elliptic motion of two bodies. Chapters VII to XI are concerned with the methods for the determination of orbits, including the orbits of double stars and of spectroscopic binaries. Chapters XII to XVIII contain an outline of planetary theory, including general dynamical principles, absolute, secular, and special perturbations, and secular inequalities. Chapter XIX is devoted to the restricted problem of two bodies which serves as an introduction to lunar theory, an outline of which is contained in Chapters XX and XXI. The rotation of the earth and moon is discussed in Chapters XXII and XXIII, whilst the last, Chapter XXIV, is devoted to formulæ of numerical calculation.

On reading the book we cannot but feel that it suffers from undue compression, and that an omission of some of the matter such as, *e.g.*, most of the chapter on precession and nutation, an account of which is contained in Newcomb's well-known *Compendium of Astronomy*, would have been an advantage, enabling the remaining matter to be treated at somewhat greater length. Although completeness is not aimed at, it is to be regretted that an account of Prof. Leuschner's admirable method for the determination of an orbit, which enables an accurate approximation to be rapidly made, was not included.

Some doubt may be expressed as to whether it was wise to omit numerical applications altogether in a subject whose ultimate aim is always numerical expression. Not to have done so would have enormously increased the size of the book. For this reason, therefore, the author is justified in taking a *via media*. The mathematical formulæ have, wherever possible, been reduced to a form suitable for numerical application, and, in doing so, the author's skill as a computer has proved itself of value. There is truth in the introductory remarks: "The student who feels the need will have no difficulty in finding forms of computation in other works. At the same time the reader who will take the trouble to work out such forms for himself will be rewarded with a much truer mastery of the subject, though he should not disdain what is to be learnt from the tradition of practical computers."

All readers may not agree with the criticisms which we have offered, and much justification can be advanced for the course which Prof. Plummer has adopted. The book is written with a freshness of treatment and a careful elucidation of difficulties which will commend themselves to the student. It meets a long-felt want. The elaborate treatises of Laplace, Tisseraud, Poincaré, and other writers on dynamical astronomy are too detailed to enable a student to study them with advantage without previous preparation. Prof. Plummer's book will make the way easier for him by giving him a general knowledge of the whole subject, which will serve as a stepping-stone to the classical treatises. We are grateful to Prof. Plummer for supplying this want in so admirable a manner.

H. S. J.

PHYSICS

Report on the Relativity Theory of Gravitation. By PROF. A. S. EDDINGTON, M.A., F.R.S. [Pp. vii + 91.] (The Physical Society of London, 1918. Price 6*s.*)

THE first chapter of this Report, dedicated to a brief survey of "the older relativity," is somewhat unsatisfactory, lacking, that is, in clearness of presentation and conceptually incorrect. Thus, for example, any reader not already informed will arrive at the end of page 3 with entirely confused or even wrong ideas about the very fundamental concept of the longitudinal contraction. "The universal nature" of this change—says the author—"makes it impossible to perceive any change at all." The reader will thus believe that there is no real content about that change, and that it concerns only the formal, mathematical side of presentation. Now, for the observer sharing the motion of a body (more correctly, with respect to whom the body rests) there is no "contraction" at all, real or formal, and thus there is no question of perceiving it; but for an outsider there is a real phenomenal change, revealing itself, for example, in the observable properties of electrons (cathode rays), etc. The author fails to make it clear. The following page 4 rather intensifies this obscurity. There are also other minor yet disturbing defects in Chap. I. which cannot be entered upon in a short review. Part of this obscurity pervades, by necessity, the first two or three sections of Chap. II., entitled "The Relations of Space, Time, and Force." Yet it is here of less consequence. At this stage (pp. 18–19) the view taken of a "straight line" is rather objectionable, although not seriously hindering the progress of the Report. Very defective, however, is the enunciation (wording) of Einstein's equivalence-hypothesis. Gravitation is not so much something "introduced by the transformation of the co-ordinates of reference" as by the changed metrical

properties of four-space, by the modified quadratic differential form ds^2, and this is precisely an intrinsic change, *i.e.* such as cannot be transformed away by co-ordinate transformation (and, no doubt, the author knows it well). There are also some obscurities on page 25 in connection with "kinds of space not occurring in Nature." But these are rather harmless. With the exception of these two introductory chapters, the Report, which in its remainder is concerned almost exclusively with the mathematical side of the subject, is carefully written and will be found very useful as a handy substitute for the numerous and scattered original memoirs of Einstein himself, of de Sitter, Dröste, and others. We have here a good presentation of the theory of Tensors, and of Einstein's field-equations, integrated for the case of a gravitating particle (Chaps. III. and IV.). "The Crucial Phenomena," treated in Chap. V., embrace the equations of motion of a particle, illustrated by planetary motion, and leading to the famous formula for the secular motion of the perihelion, the deflection of light and the shift of spectral lines. The former "phenomenon" still awaits its verification, and the latter has been rather disproved by St. John's (and more recently, by Evershed's) observations of the Sun. Chap. VI. treats of the gravitation of a continuous distribution of matter and of the propagation of gravitation. The Principle of Least Action is the leading idea of Chap. VII., which thus reviews the energetic (stress-momentum-energy) side of the subject. Finally, Chap. VIII. treats of the curvature of the four-dimensional world, a subject connected with some more recent innovations introduced by Einstein and de Sitter into the original theory. It is to be regretted that, on the general conceptual side, no account has been taken of a deep-reaching paper by Kretschmann (*Ann. der Physik.*, February 1917), and, on the physical side, of an equally interesting paper by Kottler (*ibid.* August 1918). We repeat, however, that the bulk of the Report is likely to be very useful, and those interested in the subject will be grateful to Prof. Eddington for undertaking this by no means easy task.

L. SILBERSTEIN.

Methods of Measuring Temperature. By EZER GRIFFITHS, D.Sc. [Pp xi + 176, with 81 illustrations.] (London: Charles Griffin & Co., Ltd., 1918. Price 8*s*. 6*d*. net.)

THIS book, from the pen of one of the body of scientific workers in the National Physical Laboratory, is a very welcome addition to the literature of Physical Science, more especially as the subject of which it treats (thermometry) is one in which a considerable body of the progress is due to Englishmen.

Within the compass of its pages will be found very full accounts of the five main methods by which temperature is now measured—viz. the mercurial thermometer, the resistance thermometer, the thermocouple, the full-radiation pyrometer, and the optical pyrometer. The general physicist, whose knowledge of these methods has been obtained from current text-books of Physics, will find a considerable amount of the detail necessary for precision work in each method collected here, and the bibliographies at the end of each chapter give the reader the opportunity for consulting the original sources for further information.

What strikes the reader is the remarkable increase in accuracy of determination of high temperatures which has taken place in this century—an increase which (as Principal E. H. Griffiths in an Introduction points out) is in no small measure due to the work of Prof. Callendar, Principal Griffiths himself, and some other English physicists.

Perhaps the most interesting part of the book, in view of the current discussions on radiation problems, consists of the chapters on radiation pyrometers, which contain accounts of the classical and more recent researches instituted to test the validity of Stefan's, Wien's, and Planck's laws, and also to determine to what extent such radiators as oxide and metallic surfaces differ from the "full" radiator as regards the amount of radiation and the distribution of its energy among the elementary ranges of wave-length. Extrapolation has always been a feature of temperature research, and a very excellent summary of the work which has been done to test the validity of extrapolation, and to determine the true values of melting points of such substances as sulphur, the refractory oxides, platinum, tungsten, and tantalum, and also the temperature of the positive crater of the electric arc will be found in this volume.

There is rather an unfortunate definition of "perfect gas" in the text on page 1, which is, however, corrected in a foot-note, and one or two of the descriptions of apparatus are not quite as lucid as they might be. Apart from these minor faults, the book is one which can be heartily recommended to all physicists as an extremely useful account of its subject.

J. R.

CHEMISTRY

A History of Chemistry. By F. J. MOORE, Ph D. [Pp. xiv + 292.] (New York: McGraw-Hill Book Company, 1918. London: Hill Publishing Co. Price 12*s.* 6*d.* net.)

CHEMICAL history is not a subject which appeals to everybody, and, unless presented in an attractive form, it is apt to be dry and uninteresting. Prof. Moore, however, who appears to possess the gift of presentation in a remarkable degree, begins by introducing his readers to the personal characteristics of the men whose work he is about to describe, and thus invests the dullest of controversies and hypotheses with a living interest. As stated in the preface, the book is the outgrowth of a series of talks which the author has for several years given to his senior students at the Massachusetts Institute of Technology; these lectures have dealt in a direct informal way with the fundamental ideas of the science, their origin, their philosophical basis, and the personalities of those who have contributed to the development of these ideas. The book is divided into twenty-one chapters; the first three deal with the chemistry of the Ancients, Middle Ages, and Renaissance respectively; then follow two chapters on the phlogiston theory and its supporters; subsequent chapters are devoted to the work of individual chemists, or the discussion of special subjects such as the type theory, the valence theory, or the periodic law. The last four chapters deal respectively with organic and inorganic chemistry since 1860, the rise of physical chemistry, and radioactivity and its influence upon the atomic theory. In these chapters are to be found quite exceptionally good, though brief, accounts of modern work on the rare gases of the atmosphere, on Werner's work on metal-ammonias, and the very latest work on atomic disintegration, X-ray spectra, and atomic numbers. All these subjects are dealt with in some detail, as the author rightly remarks that they lie somewhat outside the field familiar to most undergraduates, and, for this reason no doubt, the classical researches on organic chemistry are mainly dismissed with a mere mention of their existence, as the serious student will have already familiarised himself with them before reading this book. The book contains a number of excellent likenesses, and also a selection of reproductions of

famous apparatus, laboratories, or of pages from note-books, etc. We can heartily recommend the book as most entertaining and instructive reading from cover to cover.

P. H.

What Industry owes to Chemical Science. By RICHARD B. PILCHER, Registrar and Secretary of the Institute of Chemistry of Great Britain and Ireland, and FRANK BUTLER-JONES, B.A., F.I.C. With an Introduction by Sir GEORGE BEILBY, LL.D., F.R.S. [Pp. xiv + 150.] (London: Constable & Co., 1918. Price 3*s*. net.)

READERS of the *Engineer* during the early part of 1917 were provided with an interesting series of articles under the title "What Industry owes to Science," in which the endeavour was made, with no small success, to bring home to them the indebtedness of the practical arts to scientific discovery, more particularly in the realms of chemistry. As the articles were anonymous it was a matter of some interest to guess at the identity of the hand that penned them. The mystery is solved by the publication of the present volume, which consists of a reprint of the original articles with a slightly modified title; it is now seen to be the joint production of Mr. Pilcher, Registrar and Secretary of the Institute of Chemistry, and Mr. Butler-Jones, F.I.C., who are naturally in an excellent position to judge of the value of the contributions of chemistry to the applied arts.

The chemist, as Sir George Beilby points out in his introduction, is always at a disadvantage compared with the engineer so far as publicity is concerned: the Forth Bridge, the ocean greyhound, the motor-car, and the aeroplane are at once recognised as the fruits of the engineer's skill and energy; but for the most part the work of the chemist is unobvious and little if at all understood. "These records ought to prove stimulating and suggestive to those whose sons and daughters have not yet selected a calling or profession. The place of chemistry in the national life has been far more important than the majority of educated people have imagined, and this place bids fair to become of vastly increased importance in the near future. The special message for parents and teachers is, therefore, that trained chemists will, in the near future, be in increased demand for industrial and official positions."

There is the usual drawback in this book that occurs in most such publications which attempt to arouse public interest in the importance of chemistry—namely, the complete absence of illustrations; the lay mind is always impressed by size and by mere bricks and mortar, and even a few photographs or diagrams of plant or well-known factories would greatly enhance the value of the book.

F. A. MASON.

Industrial Chemistry. Edited by SAMUEL RIDEAL, D.Sc., F.I.C. (London: Baillière, Tindall & Cox, 1918.)

(1) **Dyeing with Coal Tar Dyestuffs.** By C. M. WHITTAKER, B.Sc. [Pp. xii + 214.] (Price 7*s*. 6*d*. net.)

WRITING nearly a generation ago, Ostwald placed on record his opinion that "the future technologist in England is too practical to study chemical theory if he is going into a dye-house; he prefers to study dyeing itself . . . the obvious result is that whenever any important change takes place in his line of work the English technologist has to start afresh: the German, however, turns over in his

mind the general underlying theories which he has already mastered and soon orientates himself anew."

Allowing for the German scientist's natural bias, there was possibly something to be said for his point of view, though it is doubtful whether he would venture on quite the same opinion at the present day.

In any case, whether the practical dyer nowadays deserves Ostwald's sweeping condemnation or not, he can certainly profit much by a perusal of the volume under review. Mr. Whittaker's position as head of the experimental dye-house of British Dyes, Ltd., is sufficient guarantee of the accuracy of the book, and anyone whose business or profession requires him to deal with dyes or dyed materials will find much to interest and instruct him in its pages.

The book has been written with a view to giving the reader a firm grasp of the chemical principles involved and the methods used in the application of the coal-tar colours.

Perhaps one might wish that a little more attention had been devoted to the theory of the subject, but no doubt the author has purposely refrained from overloading the book with theoretical matter, holding, perhaps, that an ounce of fact is worth a pound of theory.

(2) **Electrometallurgy: Electrolytic and Electrothermal Processes.** By ERIC K. RIDEAL, M.A., Ph.D., F.I.C. [Pp. xii + 247, with 26 figures in the text.] (Price *7s. 6d.* net.)

THE recent Government Report on Electric Power Supply in Great Britain must have made many people realise on the one hand how very wasteful and uneconomical much of our power supply is, and on the other the enormous potentialities which lie before a really cheap and efficient system.

There is no doubt whatever that the near future will see very great developments indeed in electrochemical industries if only the cost of the power can be made low enough. The refining of metals, the manufacture of steels and other alloys, the production of fertilizers, explosives, acetic acid, alcohol and abrasives, to mention only a few examples, will all be carried out by electrolytic or electrothermal processes. For this reason Dr. Rideal's book has appeared at an opportune moment, and should serve admirably either as an introduction to the subject of technical electrochemistry, or as a revision course for those who wish to keep *au courant* with the subject, and have neither time nor inclination to read all the technical journals.

The following summary of the section headings will indicate the scope of the book: Electrolysis in Aqueous Solutions and in Fused Electrolytes; Electrolytic Preparation of the Rarer Metals; Electrothermal Processes; Carborundum and Oxysilicides of Carbon; the Carbides; Electrothermal Nitrogen Fixation; and Iron and the Ferro-alloys. On page 237 a useful reference list is given showing some of the electrolytic properties of the elements.

One could wish that some figures of the actual costs of the various processes were given, as, after all, in the strenuous reconstruction period which will face us after the war, the competition will be of the keenest, and only the most efficient and economical processes will be able to hold their own.

The text is illustrated by a number of clear diagrams, and for those seeking further information there is a useful bibliography at the end of each section, so that the book should prove of use to a wide circle of readers.

F. A. M.

Monographs on Industrial Chemistry. Edited by SIR E. THORPE. (London: Longmans, Green & Co., 1918.)

(1) **Colour in Relation to Chemical Constitution.** By E. R. WATSON, M.A., D.Sc. [Pp. xii + 197, with 4 coloured plates and 65 figures of absorption curves, spectra, etc.] (Price 12*s.* 6*d.* net.)

IF a perusal of Prof. Watson's work leaves one with a somewhat confused feeling—like the Scotchman who, having been presented with a dictionary, remarked later that he found it to contain "a deal of interesting reading, but a wee bit disconnected!"—the fault is not necessarily the author's, as the whole subject is so complex and chaotic that little more can be done by a conscientious writer than to state shortly what is known on the matter, and the special theories of various experimenters. This Prof. Watson has done with considerable skill, and the very large number of absorption curves given offer at least an indication of the results obtained. Hitherto there has been no work on this important subject in the English language, and even in German the latest book—Ley's *Beziehungen zwischen Farbe und Konstitution*—only brings one up to 1911. It is hardly possible in a review to deal in detail with so abstruse a subject as the connection between colour and constitution, but it will suffice to note that successive chapters deal with the Early History of the subject, the Quinonoid Theory, Absorption Spectra and Methods for their Measurement, Spectra of Typical Organic Substances and Dyes, Relationships between Constitution and Depth of Colour, Theories on the Nature of the Vibrations causing Absorption Bands and Colour, Infra-red Absorption Spectra of Organic Substances, Fluorescence, Colour and Spectra of Inorganic Compounds, and, lastly, a Bibliography dealing with the literature of the subject.

Prof. Watson, on p. 29, quite rightly emphasises the importance of the spectrophotometer as a means of estimating colour—further developments may certainly be looked for in this direction. The diagram on p. 35, however, and the statement on p. 36 that "this family of curves is a set of parallel straight lines," are a little difficult to comprehend, as, according to this, the solution should continue to absorb light even at infinite dilutions.

Prof. Watson's work should prove of service to those who are investigating this fascinating borderland where chemistry, physics—and commerce—meet together.

(2) **Coal and its Scientific Uses.** By WILLIAM A. BONE, D.Sc., Ph.D., F.R.S., Professor of Chemical Technology in the Imperial College of Science and Technology, Chairman of the British Fuel Economy Committee (1915-17). [Pp. xv + 491, with numerous illustrations, diagrams, and tables.] (Price 21*s.* net.)

MORE than usual interest attaches at the present moment to Prof. Bone's masterly treatise on Coal and its uses, owing to the strange turn of events which has landed us this winter in the midst of a coal shortage. But even without this pressing reminder of the importance of the subject the book would stand out as a really valuable contribution to scientific literature.

Hitherto there has really been no satisfactory treatise dealing with Coal right from its very origin to the ultimate uses to which it is put both as a domestic and industrial fuel and also as a chemical raw material.

Although the author has written a real scientific book which is likely to become a standard work on the subject, he has, nevertheless, managed to infuse a certain

very human element into it. He is prepared equally to discuss the principles of combustion—which he has made his own special subject—or the latest dodge for extracting the nimble half-crown from the unwary by means of such wonderful preparations as those noted on p. 229, a spoonful of which, dissolved in a pint of water and sprinkled on a ton of coal, will make it last as long as four tons!

Prof. Bone's subject is of such exceptional interest and is dealt with from so many aspects, including that of the scientist, manufacturer, householder, and statesman, that it will certainly prove acceptable to a wide circle of readers.

The subject matter is divided into chapters dealing respectively with the Origin and Formation of Coal; the Chemical Composition of Coal (which is considered in great detail on pp. 35–163); the Combustion of Coal; the Principles governing Combustion and Heat Transmission in Boilers; Domestic Heating; the Smoke Nuisance and its Abatement; the Carbonisation Industries—in which the vexed question of high- *versus* low-temperature distillation is discussed; the Complete Gasification of Coal; Water Gas and its Applications; Fuel Economy in the Manufacture of Iron and Steel; Power Production from Coal; "Surface Combustion," etc.; and a useful Bibliography dealing with the subject of Coal.

At the present day, when a shortage of coal and iron exists, there is a whole story, and a very unsatisfactory one, in Prof. Bone's statement on p. 371, ". . . the author has sometimes come across cases, and especially in steel works, where, for want of proper scientific control, upwards of 100 tons of coal per diem were being gasified in producers under conditions which mean the needless waste of fully 25 per cent. (and sometimes more) of the fuel consumed. And, so far as this country is concerned, there is perhaps no branch of fuel technology in which the substitution of scientific for rule-of-thumb methods would realise greater margins of improvement than in connection with generation and application of producer gas for furnace purposes."

It is to be feared that the same disastrous story must also be told of other industries where fuel is notoriously wasted to a great extent, and if the present work under review should succeed in casting light in dark places and, assisted by the present fuel shortage, in persuading some at least of our industrialists to put their houses in order, no slight victory will have been obtained. Prof. Bone and all concerned in bringing out the book are to be congratulated on the success of their efforts.

(3) **The Applications of Electrolysis in Chemical Industry.** By A. J. HALE, B.Sc., F.I.C. [Pp. x + 148, with diagrams.] (Price 7*s.* 6*d.* net.)

THIS volume covers a good deal of ground and has chapters dealing with the following subjects: General Theories, Methods of Generating the Current, the Electrolytic Refining and Winning of Metals, Production of Hydrogen and Oxygen, Electrolysis of Alkali Chlorides, Chlorine and Caustic Soda, Hypochlorites, Chlorates and Perchlorates, Miscellaneous Inorganic Compounds, and Organic Compounds.

The most obvious defect of the work is due to the limitations of space, as it can hardly be assumed that all the subjects noted above can be satisfactorily discussed in 148 pages of fair-sized print. For instance, the whole subject of electrolytic copper refining is dismissed in eight pages—hardly an adequate treatment of so important a matter; no mention is made in the section on chlorine and caustic soda of the important Moore-Allen cell which is widely used

in the United States; and, again, four and a half pages at the end of the book have to do duty for the whole of the theory and practice of electrolysis as applied to organic compounds. Certainly an examination of this last chapter would scarcely lead the average reader to suppose that anything was known on the subject: what remains to be discovered is indeed far greater than what is known in this respect, but Mr. Hale's book does scant justice to the pioneer work which has already been done in the applications of electrolysis to organic chemistry. The book hardly appears to the unprejudiced observer to be quite up to the high standard already set by certain other monographs in the same series.

F. A. M.

Organic Chemistry for Advanced Students. In 3 volumes. By JULIUS B. COHEN, Ph.D., B.Sc., F.R.S. Second edition. [Pp. viii + 366; vii + 435, with illustrations; vii + 378.] (London: Edward Arnold, 1918.) Price 54*s.* net.

THE first edition of this well-known work appeared in 1907, and at once became the standard book on advanced organic chemistry in the English language; since then it has been freely consulted alike by teachers and students who wished to acquire a comprehensive view of the advanced work done in any particular branch of organic chemistry without the expenditure of time and labour involved in consulting a large mass of original literature. With the lapse of time much additional knowledge has been added to some of the subjects dealt with, and upon the exhaustion of the first edition a thorough revision of the text has accordingly been undertaken. With a view to linking together kindred subjects in a consecutive form the book is now issued in three volumes, entitled Reactions, Structure, and Synthesis respectively Much new matter has been added throughout the book; thus, for example, we find in Volume I. an entirely new chapter, entitled "Abnormal Reactions," in which are grouped together a number of very different and peculiar reactions, while an article on the Walden Inversion forms a valuable addition to the chapter on isomerism and stereoisomerism in Volume II. In Volume III. the chapters on carbohydrates and fermentation have been considerably enlarged, while the chapter on alkaloids has been brought up to date by the inclusion of Perkin's recent work on cryptopin and protopin, as well as Robinson's synthesis of tropinone. A section on chlorophyll has been added to the chapter on proteins, and in this connection it is perhaps permissible to express a regret that the author has not seen his way to include an account of Willstätter's work on the anthocyan pigments. In the preface readers are reminded that the book "is not intended to serve as a reference book, but to furnish a general survey of those fundamental principles which underlie the modern developments of this branch of chemistry," and we may add that the author has succeeded most admirably in his purpose. There is no doubt that in its revised form the book will attain even greater popularity than heretofore.

P. H.

Plant Products and Chemical Fertilisers. By S. HOARE COLLINS, M.Sc., F.I.C. [Pp. xvi + 236.] (London: Ballière, Tindall & Cox, 1918. Price 7*s.* 6*d.* net.)

NOT the least important sign of the chemical revival resulting from the war is the number of new books published which deal with the various aspects of applied or

industrial chemistry. The present volume is one of a series entitled "Industrial Chemistry," produced under the editorship of Dr. Samuel Rideal. As stated in the general preface, ". . . the British Empire has now an opportunity of increasing its industrial output by the application of this knowledge to the raw materials available in the different parts of the world."

The author's plan has been to "pick up the story of those industrial waste products which are useful as fertilisers, and carry it on through the soil and crops, until new products are available for industrial uses." It should be said at once that the author has succeeded in producing a rather remarkable little book, for it contains a large amount of varied information in a comparatively small compass. The book is divided into four parts, dealing respectively with Fertilisers, Soils, Crops, and The Production of Meat; the table of contents includes a large variety of subjects, ranging from the properties and uses of the various fertilisers to the chemistry, characteristics and uses of the crops produced; there are, moreover, sections devoted to the calorific value of foods, the future prospects of scientifically controlled agriculture, and to the discussion of labour difficulties and education of land workers. With such a big programme it is only natural that the information imparted should occasionally be somewhat brief, not to say scrappy, but any deficiencies in this respect are amply made up for by a very complete bibliography which enables the reader to supplement his knowledge in any desired direction.

The author has very pronounced views on the application of scientific principles to agriculture, and quite rightly points out that the unintelligent use of fertilisers can easily do more harm than good, and a knowledge of the proper fertilisers requires not merely a knowledge of the fertilisers themselves, but also of the types of soil to which they are to be applied and the conditions under which the cultivation of the crops is undertaken; or again: "To-day the needs of agriculture in populous countries are often more connected with the mismanagement of the past than with any other one factor." His views on the production of beet sugar in this country are that it can undoubtedly be accomplished successfully, but that only the future can tell whether it would be a profitable undertaking; experience shows, however, that "no nation can afford to be entirely dependent upon outside sources, and at least some fraction of the necessary sugar may have to be grown in Great Britain, even if it is not economically profitable." With regard to the supply of agricultural labour after the war the author considers that the returned soldier who has learnt the use of spade and pick may be well suited to agriculture and forestry, while many women at the end of the war will be more suited than the returned soldiers to the routine work formerly done by men.

In conclusion, it may be said that the book is replete with useful information, presented in a thoroughly readable form, and should be read by all who are interested in the agricultural future of the country.

P. H.

The Chemistry of Synthetic Drugs. By PERCY MAY, D.Sc., F.I.C. Second edition, revised and enlarged. [Pp. xii + 250.] (London: Longmans, Green & Co., 1918. Price 10*s.* 6*d.* net.)

THE author points out in the preface that new drugs are constantly being introduced, but that their constitution is frequently unknown to chemists possessing an otherwise general knowledge of organic chemistry: the present book is designed to remedy this state of affairs and to stimulate interest in a branch of chemistry which affords scope for commercial application. In view of the difficulty experienced in this country in maintaining supplies of essential synthetic drugs at

the outset of the war, it is certainly to be hoped that more attention will be paid in the future to the manufacture of such compounds, and the publication of the second edition of Dr. May's well-known book is therefore very welcome. The general arrangement of the subject-matter has been maintained unchanged, but several useful additions have been made; thus we note a section on the trypanocidal action of the benzidene dyes—Trypan Red and Trypan Blue—and the antiseptic action of dyes of the acridine series, such as Flavine and its derivatives. A short account is also given of recent work upon antiseptics of the chloramine type, and the account of Salvarsan and allied bodies has been brought up to date.

P. H.

BOTANY

Plant Physiology. By VLADIMIR I. PALLADIN. Authorised English edition, with 173 illustrations. Edited by PROF. BURTON EDWARD LIVINGSTONE, Ph.D. [Pp. xxv + 320.] (Philadelphia: P. Blakiston's Son & Co., 1918. Price $3 net.)

THIS translation is based on the German translation of the sixth Russian edition, and on the seventh Russian edition of 1914, and with it Prof. Palladin's famous text-book becomes for the first time available to English readers. The editor states that the body of the text aims to be primarily a true translation of the German edition. The few alterations that have been made consist mainly in some modification of the order of presentation, or slight additions that render certain statements more easily understood, such changes being indicated by being enclosed in brackets. Editorial notes have been added here and there in the form of foot-notes, signed "Ed." Prof. Palladin's leaning towards the chemical side of Plant Physiology is apparent from the much greater space devoted to plant nutrition in Part I. than to growth, movement, and reproduction, which together constitute Part II.; wherever possible formulæ and equations are given to illustrate the chemical changes which take place within the plant, or which may be brought about *in vitro;* the physico-chemical side of the question also receives attention, the book opening with a discussion of the energy content of compounds, and the thermochemical aspect of many changes is discussed. Rather short accounts are given of the chemistry of chlorophyll and the proteins, but any defects in this respect are made up for by frequent reference in the foot-notes to sources from which more detailed information may be obtained; it is, indeed, a feature of the book that it is supplied with an exhaustive bibliography and an immense number of references to original papers. The book, which is well printed on good paper and contains 173 very good illustrations, will form a welcome addition to the rather limited number of smaller books on Plant Physiology.

P. H.

Plant Genetics. By JOHN M. COULTER, Head of the Department of Botany in the University of Chicago, and MERLE C. COULTER, Instructor in Plant Genetics in the University of Chicago. [Pp. ix + 214.] (Chicago: The University of Chicago Press, 1918. Price $1.50 net.)

THIS textbook has originated from a course of lectures to botanical students at Chicago in their last undergraduate or first graduate year, the purpose of the lectures and the book being "not to develop professional geneticists, but merely to initiate students of botany with the point of view of working geneticists, so that they can appreciate an important phase of botanical literature."

Existing textbooks, say the authors, have all emphasised animal genetics more strongly than the student of botany needs. The authors certainly do their best to avoid this state of affairs, but it is doubtful whether the value of a textbook on genetics is improved by the elimination of all reference to animal work that can possibly be avoided. Plant and animal genetics have developed as one subject, and the principles of genetics have been evolved jointly from plant and animal studies. Also it is no bad thing for the student of plants to come into contact with animal science. Moreover, the authors themselves, in spite of their best intentions, cannot keep the animal out, for they deal with Castle's and Jenning's work on modification of unit characters in rats and protozoa respectively, with Morgan's work on linkage in the fruit fly, and with the work of Riddle and others on sex determination in animals.

The greater part of the book is in reality an elementary though comprehensive survey of Mendelism and its consequences. After the first three chapters, entitled Introduction, Early Theories of Heredity, and Inheritance of Acquired Characters, the elementary facts of Mendelism are stated in Chapter IV., entitled Mendel's Law. In the next five chapters the developments of Mendelism since the rediscovery of Mendel's Rule in 1900 are dealt with under the heading "Neo-Mendelism"—a term for the introduction of which it is difficult to see the necessity. In the following chapters a number of topics are considered from the Mendelian point of view, including Parthenogenesis and Vegetative Apogamy, Self Sterility, and Hybrid Vigour. The book closes with chapters on Sex Determination and The Bearers of Hereditary Characters.

The authors evidently consider biometrical methods valueless or outside the scope of their work, as they are not mentioned. But one is surprised to find no mention of the interesting question of graft hybrids, which surely comes within the scope of genetics, although the authors discuss the somewhat allied question of somatic segregation. The book is clearly written and the facts and theories of Mendelism ably stated. It will no doubt form a useful introduction to the study of Plant Genetics for the class of students for whom it is written.

The merits of the book make one regret the more, the unfortunate feature of it, that to readers unacquainted with genetics something of a wrong impression may be given as to those who are responsible for the development of the subject into the important position it occupies to-day. It is true that the work of Correns is referred to on a few occasions, but the majority of the pioneers of Mendelism are passed over This would not matter so much if the authors avoided naming all workers on the subject, although such a course might not result in a work of any value; on the contrary they do not hesitate to quote some of the later workers in this field. Now, undoubtedly, to no one does the development of Mendelism owe more than to Bateson, but he occupies a very inferior position in *Plant Genetics*, and even the title of his *Mendel's Principles of Heredity* is cited incorrectly. There is only slight mention of the really classical work of Bateson's school on flower colour, which includes that of Bateson and Punnett on Sweet Peas, that of Miss Saunders on Stocks, and that of Miss Wheldale on Antirrhinum. Tschermak, one of the rediscoverers of Mendel's Rule, is not even mentioned. It was shown by de Vries in 1900, by Correns in 1901, and by the late R. H. Lock in 1904, that starchy endosperm in maize is dominant over sugary endosperm. Yet this question is introduced in *Plant Genetics* with the statement (p. 46), "East, in crossing starchy and sweet corn, obtained all starchy in the F_1 generation, followed by the usual 3 : 1 ratio in the F_2." But the authors show no lack of appreciation of East's work as, according to the index, his name is mentioned

sixteen times—eleven times more than that of either Correns or de Vries. Indeed, many passages from East's writings are quoted, and it is their number, perhaps, which accounts for a certain amount of monotony in the words introducing the citations. The following passages will make this clear. "A statement by East on this point is pertinent" (p. 112); "In conclusion, the following quotation from East is pertinent" (p. 131); "A law which East has formulated in reference to xenia is pertinent" (p. 151); "A statement by East on the 'value of heterozygosis in evolution' is pertinent" (p. 167). On p. 48, however, there is a welcome variant, "An ingenious theoretical answer to this pertinent question has been suggested by East." It is true that the authors say that the book is entirely inadequate as a work of reference, and much representative material has been omitted, but it would not have been difficult to have made such a selection of material as to give readers a more accurate idea of those who have built up the study of genetics.

This lack of balance in citation is the feature of the book that will chiefly excite criticism. There are, however, some minor points which deserve attention. On p. 49 it is stated that we cannot explain the mechanism of the transmission of the stimulus in the sensitive plant. Now, as theories to account for the transmission of this stimulus have been put forward by, for example, Sachs and Pfeffer, Haberlandt, and Ricca, the statement of the authors is not exact. Inexactness also appears in the short literature lists at the ends of the chapters. For instance, on p. 62 there is a reference "Czapek, P. and M. E., Biochemie der Pflanzen, Jena, 1913." Now, the first volume of the second edition of a work with this title was published at Jena in 1913, and it is by a man named Czapek, but there is only one of him and his initial is F. On p. 176 Keeble and Pellew are cited as publishing in *Genetics* for 1910, whereas this journal first made its appearance in 1916. The reference should be to *The Journal of Genetics*.

The book is produced in the excellent fashion characteristic of the University of Chicago Press.

W. S.

Fundamentals of Botany. By C. STUART GAGER, Director of the Brooklyn Botanic Garden. [Pp. xix + 640.] (Philadelphia: P. Blakiston's Son & Co., 1916. Reprinted, 1917. Price $1.50 net.)

AN introductory textbook of Botany can do little in the way of introducing fundamental facts not contained in already existing elementary textbooks. Novelty must lie rather in the mode of presenting the material and the space devoted to the various aspects of the subject dealt with. Although Dr. Gager's book does indeed introduce a certain amount of material not usually found in existing textbooks—notably in regard to the Economic Importance of Fungi, and on Heredity and allied questions—it is chiefly on account of the way that the whole subject is presented that the book is so thoroughly to be recommended. The majority of elementary textbooks of botany of the last twenty or thirty years are good introductions to the study of plant morphology, containing somewhat supplementary remarks in the nature of a rather disheartening introduction to plant physiology. Gager's *Fundamentals of Botany* is rather an introduction to the study of the Living Plant. One of the things an introductory course of study in botany should do, says the author, is to teach the student the fundamental elementary facts concerning plant life, and it is in this presentation of plants as living organisms rather than as varied structures that Gager's book is an advance

on most works covering the same ground. Among its other good qualities are the extremely numerous, and for the most part good illustrations, the introduction of the names and portraits of botanists who have built up the subject, and, not least, its very reasonable price.

Although it is the plant as a living organism which is the underlying theme of the author's presentation of botany, it is interesting, and it will reassure the morphologist, that Dr. Gager, himself a plant physiologist, devotes upwards of 450 pages, or nearly three-quarters of the book, to the structures and life histories of the various "types," while in the selection of these types there is nothing revolutionary, although in some cases, as in that of the fern, it is not any particular species which is described, but rather a description of the structure and life history of ferns as a group, with reference to numerous different species. And in all that part of the book dealing with structures and life histories, the mode of living of the plant is adequately dealt with, and not its structure alone.

The author holds that an introductory course should not be "presented on the supposition that its main purpose is to pave the way for more advanced courses," but "to introduce the pupil to a new realm of thought" . . . "not to make the subject simple—to remove all difficulties—but to make it really interesting." With these views we must agree, and we must also agree that Dr. Gager has put them into practice in his *Fundamentals of Botany*.

W. S.

Soil Biology Laboratory Manual. By A. L. WHITING, Ph.D. [Pp. x + 143, with numerous schedules.] (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1917. Price $1.25 net, or 6*s*.)

IN his preface the author states that "Soil Biology treats of the micro-organisms which inhabit soils in their relation to soil fertility, crop production, and permanent agriculture." But in our opinion, to limit the use of the term biology simply to organisms of microscopic size is an unjustifiable circumscription of the word biology. To take just one example, any work on "Soil Biology" that takes no account of the part played by earthworms in the formation of moulds, aeration, drainage, etc., is obviously not fulfilling its title. In the present case the volume would have had its contents more accurately indicated had it been termed "The Micro-organisms of the Soil," or some such title suggestive of its more limited scope. Apart from this the book is an arrangement of practical exercises well collected and destined to give the student not only a knowledge of protozoology, bacteriology, and mycology in as far as they affect soil, but also a series of useful biochemical tests by which their presence may be detected and their work estimated. It should prove valuable in any institution in which courses in agriculture or horticulture are given, and the explanations provided are sufficiently clear in most cases to enable a student of average ability to carry out the experiments for himself.

C. H. O'D.

An Outline of the History of Phytopathology. By HERBERT HICE WHETZEL, Professor of Plant Pathology at Cornell University, Ithaca, New York. [Pp. 130, with 22 portraits.] (Philadelphia and London: W. B. Saunders Company, 1918. Price 7*s*. 6*d*. net.)

THIS little book does not pretend to be more than an account in outline of the most important features in the history of the developmept of plant pathology; it makes no attempt at a complete history of the science.

The author divides the history of plant pathology into five eras—the ancient era, extending from the earliest times to the fall of the Roman Empire (476 A.D.); the dark or middle era, including the middle ages, and covering roughly the time from the fall of the Roman Empire to the beginning of the seventeenth century; the pre-modern era, extending from the beginning of the seventeenth to the middle of the nineteenth century; the modern era, from about 1850 to 1906; and what the author calls the present era, from 1906 onwards. It seems rather doubtful whether the important events of the last twelve years are really such as to justify the separation of this time from the modern era. The author bases this separation on the following events: the establishment of chairs of plant pathology in American universities; Smith's work on crown gall; the founding of the American Phytopathological Society and its journal, *Phytopathology*; the enactment of the United States Quarantine Act of 1912; the introduction of sulphur in place of copper fungicides; the development of disease-resistant crops; and the outbreak of the chestnut blight in the Eastern United States.

The four earlier eras are each divided by the author into *periods*, "phases in the development and crystallization of the prevailing point of view of the epoch." These periods are named after the men who are chiefly responsible for moulding the thought of their respective periods. The resulting nomenclature strikes one as being in some cases rather cumbrous, for, although "Kühnian Period" and "Ungerian Period" are reasonable enough, the expressions "Zallingerian Period" and "Millardetian Period" seem rather clumsy, and they are scarcely likely to come into general use.

The book consists very largely of biographies of those plant pathologists to whose energies the development of the science is due. The style is clear and readable, and the insertion of twenty-two portraits of eminent plant pathologists adds to the interest of this little book.

W. S.

ZOOLOGY

The Wonders of Instinct. By J. HENRI FABRE. Translated by ALEXANDER TEIXEIRA DE MATTOS and BERNARD MIALL. [Pp. 320, with 16 plates.] (London: T. Fisher Unwin; Toronto: The Musson Book Company, 1918. Price 10*s*. 6*d*., or $3 net.)

IT is not often that one looks forward with pleasure to reviewing a scientific book, as, although it may contain much of interest, yet there is almost invariably a great deal that has to be read as a duty and sometimes a stern duty at that. The case is entirely different with any further instalment of chapters translated from the ten volumes of J. Henri Fabre's *Souvenirs Entomologique*. The present volume contains fourteen such chapters—two translated by Bernard Miall and the remainder by Alexander Teixeira de Mattos—and is to be most cordially recommended.

The chapters are selected to illustrate various experiments conducted to test the extent and limitations of the working of instinct in insects and the possibility of the exercise of reason by these animals. No one can fail to be impressed by the acuteness of the author in devising crucial tests to try the powers of the insects, the patience shown in carrying out the experiments, and the typical Gallic clearness with which the results are analysed and expressed. The results all point in the same direction and show that, so far as can be discovered, instinct alone governs the animals' activities. Although this is developed in some cases to

an almost unbelievable degree of complexity, yet its limit can be reached, and, when faced with a problem outside that provided for by instinct and requiring only a modicum of reason, the insect fails completely. A more miserable failure to solve a simple problem than that instanced in the case of the caterpillar of the pine processionary moth it would be difficult to imagine.

Throughout the book the reader's attention is closely held, although, perhaps, the chapters on the *smia* are a little drawn out, and, in order to press a point, an important one, there is rather too much repetition. It must be added that this is in no way due to the translators, who have retained the letter and, what is more remarkable, the spirit of the original text with striking fidelity. A translator's note on p. 21 states that the Zoophytes include "Starfish, Jellyfish, Sea-anemones, and Sponges," whereas it was a term originally used to include Sponges, Hydroid Cœlenterates, and Polyzoa.

Written in a style that is as remote from the "dry as dust" manner only too often adopted by scientific writers as can well be imagined, and yet containing a wealth of facts for Biologist, Naturalist, and comparative Psychologist, the book should be assured of a wide circulation. The translators have laid the reading public under another debt in providing a further instalment of the works of one of the greatest students of insect life in the last half of the nineteenth century.

C. H. O'D.

Principles of General Physiology. By WILLIAM MADDOCK BAYLISS, M.A., D.Sc., F.R.S., Professor of General Physiology in University College, London. 2nd edition, revised. [Pp. xxiv + 858, with 261 illustrations.] (London: Longmans, Green & Co., 1918. Price 24*s*. net.)

PROF. BAYLISS' *Principles of General Physiology* is peculiarly difficult to review. Its title does not convey its contents; it contains a great deal that is not General Physiology. For so wide a range it would be hard to find a title much more precise than "Leaves from my Notebooks." Moreover, these leaves are of exceptional value, and have met with very cordial appreciation, which has culminated in the call for a second edition within two years.

The earlier chapters deal with Protoplasm and its environment. It is here that the principles of General Physiology are most obvious; and here that Prof. Bayliss is at his best. The later chapters form a less coherent chain of particulars and generalities, less well arranged, and treated, for the most part, in less detail.

Prof. Bayliss has, naturally, given prominence to recent work from the laboratory with which he is associated. He has done this very fully—so much to the omission, or disparagement, of other work as to diminish, in some cases, the value of his presentation. Waller is omitted, completely, from the account of the electrocardiogram; Gotch from that of the knee-jerk; and Mrs. Tribe (who anticipated Fuhner and Starling) from that of the pulmonary vasomotors. Locke and Rosenheim's work on the consumption of dextrose by the heart is most unfairly represented on p. 449. The Bibliography, though very full, contains no reference to Dastre, to Luciani, to Popielski, to Rubner, or to Swale Vincent. Surprising, again, is the treatment of Ringer. Although a portrait of Ringer is given and his work described as of fundamental importance, this work is dismissed with a brief and bald account of some of its details without any adequate reference to the principles that Ringer established. If this is the

treatment of Ringer, in Ringer's College, who can criticise Loeb's claim that the conception of "physiologically balanced solutions" dates from his own publication in the year 1900?

But, notwithstanding drawbacks of this nature, we repeat that Prof. Bayliss's work is of great value. It is, naturally, of greatest value to those who can read between the lines. We have already indicated that it covers a very wide field, and would add that it conveys such a wealth of information that few physiologists will care to be without it.

W. L. S.

MEDICINE

An X-ray Atlas of the Skull. By A. A. Russell Green, M.B., B.S., M.R.C S., Capt. R.A.M.C. (T.), M.O i/c of X-ray Dept., 2/1 Southern General Hospital, etc. [Pp. x + 27, with 5 coloured plates.] (London: Longmans, Green & Co. Price 10*s*. 6*d*. net.)

This work is intended to serve as a guide in the interpretation of skiagrams of the head. For this purpose the author has made use of the dry skull. By rendering special parts opaque, outlining sutures with wire, etc., he has obtained radiographs showing what may be seen or located in the skull when taken from different angles. In the diagrams only one side of the skull is so treated, so that there are two complementary halves in each figure—one half showing as an untouched print, while the other shows the anatomical landmarks and structures by means of schematic colouring.

The apparatus consists of a tube-holder, capable of moving laterally; a plate-holder, with struts for immobilising the head; and a diaphragm between the two, consisting of an aperture in the block of wood to which these are attached, which is capable of sliding up and down between the uprights of the back of a chair.

The method of localisation described has, the author states, stood the test of more than two and a half years' trial in a large military hospital. For foreign bodies in the face good antero-posterior and lateral views are quite sufficient. In the cranium the method is more complicated, but granted certain measurements are kept constant—viz., from target to plate 50 centimetres, and from primary to secondary position of tube 10 centimetres—then the distance of a foreign body from the plate can be directly read off from the given table—*e.g.*, a shadow shift of 10 mm. gives a depth of 45 mm. or $1\frac{25}{32}$ in.

The Atlas is well printed on good paper. Exception might be taken to the radiographs of living subjects (Plates I. and II.) which are indistinct. The small figures on succeeding plates err on the side of size, being too small; otherwise the idea is good. The Atlas should prove a useful book of reference in the X-ray

Animal Parasites and Human Disease. By Asa C. Chandler, M.S., Ph.D. [Pp. xiii + 570, with 245 text-figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1918.) Price $4.50 net.

In spite of the enormous amount of research that has been done on *Animal Parasites and Human Disease* during the last fifteen years, it remains for the most part buried in periodicals or special volumes, although it certainly constitutes one

of the most fascinating and important advances of biological science. Hence it is almost inaccessible to the ordinary public, and even medical practitioners, health officers, and sanitary inspectors have rarely the time necessary to delve in these mines and extract the vital parts of the information they contain. The present volume is, so far as we know, the only one of reasonable size that does this for them in a thoroughly satisfactory way.

The author in his preface defends the time and labour he has spent on the production of this book against those who deem all time not spent in research as time wasted. But good work never requires an apology, and in probably no branch of medical biology was such a book more urgently needed. It contains an extraordinary amount of information for its handy size, and all in a readily assimilable form. The illustrations and copious index add to its value.

The parasites are arranged under three great Zoological groups—the Protozoa, the Worms, and the Arthropods. The first two groups, of course, include by far the larger number of serious parasites, and the last, although not such dangerous forms in themselves, are of enormous importance because of the rôle they play in the transmission of disease. Then, too, they are of larger size, and so more easily recognised and controlled. Each group is dealt with in turn, and the various sections treated individually after the particular section has had a general introduction. In every case the diagnosis, distribution, and treatment of the disease is discussed.

The style adopted is throughout vigorous, and even if somewhat "American" for the British reader, it is nevertheless incisive and clear. The author's remarks on Syphilis and on Vivisection are worth reading and will be pretty generally agreed to.

It is a volume that can be heartily commended, and its utility and force should assure it a wide circle of readers both professional and lay, for its treatment of the subject is such that it can be readily followed by all.

C. H. O'D.

ENGINEERING

Alternating Current Electrical Engineering. By PHILIP KEMP, M.Sc., A.M.I.E.E. [Pp. xi + 494, with 399 illustrations.] (London: Macmillan & Co., 1918. Price 17*s*. net.)

THERE are many text-books available for students of alternating current technology; some of the older ones were treatises on applied mathematics in which the conditions controlling the flow of currents in alternating current circuits were almost a by-product of a mass of mathematical analysis, at the other extreme has been the text-book which has attempted to deal with the subject without the use of any higher mathematical analysis at all—a method which, though useful for many evening-class technical students, was unnecessarily laborious, and clumsy in many of its developments.

This book strikes a mean between the two extremes, and the result is a text-book which should have a wide field of usefulness.

The book starts with a definition of the alternating current, its frequency, amplitude, and wave shape; it then proceeds to discuss practical methods of measurement. This is followed by the study of the effect of Resistance and Self-Induction in an alternating current circuit.

Since the book is intended primarily for those not possessing much knowledge of mathematics, it would seem desirable to substitute one of the well-known

graphical methods of treating the rise of current in a circuit possessing self-induction when connected to a D.C. supply, for the mathematical solution of the problem now given. Next follow chapters on Alternating Current Power and its measurement, and on the effect of capacity in Alternating Current Circuits. A chapter on Circle Diagrams is a useful introduction to the study of these diagrams in connection with Induction Motors. There is also a chapter on the Magnetisation of Iron when subjected to alternating magnetisation.

The chapter dealing with wave forms and the analysis of wave form is very complete and elaborate, and will be of great value to engineers who have to deal with the problem. Luckily for the student and the engineer the number of wave forms met with in practice that depart very much from the sine wave shape is rapidly decreasing, and this process will undoubtedly continue, as it becomes more universally recognised how many of the difficulties that arise in A.C. supply systems are caused by wave forms that differ from the pure sine shape. Polyphase currents are next dealt with, and then follow two valuable chapters on alternating current instruments. These include, besides the descriptions of commercial ammeters and voltmeters, wattmeter and power factor indicators, such instruments as the oscillograph, both of the bifilar pattern and of the form in which cathode rays are used to delineate the wave shapes. There are also descriptions of frequency meters and leakage indicators. The chapter on transformers is clear and good, but it is to be regretted that in dealing with performance and testing, the author has made no mention of the triangle diagram developed by Morris for determining regulation from a short-circuit test. This diagram is much the most useful for practical work that has been devised. There is a long chapter on alternators, which includes not only testing but design schedules.

There are chapters on power transmission, synchronous motors, phase advancers, rotary converters, and, finally, induction motors and single phase motors. The author is to be congratulated on his book; it is very complete in its treatment up to the standard which it proposes to reach; the chief criticism that can be urged (and that is not really a drawback) is that it includes a good deal of matter which is beyond the practical requirements of the student for whom it is primarily intended.

E. W. M.

MISCELLANEOUS

Life and Letters of Sir Joseph Dalton Hooker, O.M., G.C.S.I. Based on materials collected and arranged by Lady Hooker. By LEONARD HUXLEY. [Vol. I., pp. x + 546, with 4 plates and 1 map; Vol II., pp. vi + 569, with 5 plates.] (London: John Murray, 1918. Price 36*s*. net.)

IT is no easy task, even in the 1100-odd pages of these two volumes, to compass an account of a life that extended over a period of nearly a century; a life replete with experiences, and one which embraced some seventy-three years of active scientific work.

The story of Sir J. D. Hooker's life could not fail to be of interest from his eminence as a botanist, but added to this his most active years marked the renaissance period of modern scientific thought, and were enriched by the friendship of those biologists whose names stand foremost as pioneers of the doctrine of Evolution. The friend and confidant of Darwin, he was one of the earliest converts to the new view-point for which he was mainly instrumental in supplying the botanical data.

As the son of Sir William Hooker, Joseph had all the advantages of environment and heredity. He first came into public notice as Botanist in the *Erebus* during Ross's expedition to the Antarctic, and the observations made in these regions not merely laid the foundation for his *Flora Antartica*, but were the beginning of that interest in Geographical distribution on which he later became a recognised authority. For a short time after his return he was connected with the Geological Survey, to which fact we owe the writings on *Stigmaria Lepidostrobus* and *Trigonocarpon*.

At the age of thirty we find him on his way to India, where he spent three years botanising in the Himalayas, and not only established a reputation as an explorer but made those extensive collections which formed the basis of the *Flora Indica*. During all this period he was an indefatigable correspondent, and the letters and extracts selected by Mr. Huxley form a valuable commentary to his published journals.

Hooker was essentially a systematist, but what gives interest to his writings is that probably more than any other botanist of his day and generation he combined a rare faculty of critical observation of detail with a comprehensive philosophical outlook. It was thus that his great experience of the species of the Antarctic and of India was synthesised into the broad generalities that alone could have given support to the Evolution hypothesis and enabled him with his collaborator, Bentham, to produce with such marked success the monumental *Genera Plantarum*.

The character of his taxonomic work is summed up by a sentence in one of the letters reproduced, which was written to Harvey, in which he points out that "reducing a bad species is far better than making a new."

Opinions are expressed on many important subjects, which are as pertinent to-day as then. For example, writing to Henslow in 1855 he defends Botany as a subject for medical students because the mental training is the best means of becoming skilful in diagnosis. Or, again, the comments on the relation between Science and the State have lost nothing of their importance by the lapse of time.

It is, however, as always in biography, the man rather than his career that holds our chief interest, so the chapter entitled "Personal," though one of the shortest of the fifty in these two volumes, is most luminous with those vivid sidelights that best convey the atmosphere of character. Hooker's attitude, at a time when Science and unprogressive religious orthodoxy were at daggers drawn, is in striking contrast to that of many of the controversialists on both sides, and indicates that high capacity for distinguishing essentials from dogma, that placed him so frequently in advance of contemporary thought. We owe no small debt to Mr. Huxley for the admirable selection of these letters, which throughout represent that happy mean between lack of personality and indelicate intimacy.

Many names of men famous in Science, Literature, and Art necessarily figure in these pages, and for most of these short biographical footnotes are furnished which greatly add to the reader's interest. Letters and extracts are welded together by suitable explanatory matter and grouped in the numerous chapters under various headings. Evolution naturally claims a large share, but every espect of Hooker's many-sided activities are well represented. The work is one which has its appeal to the general reader no less than to the student of scientific thought.

E. J. SALISBURY.

The Processes of History. By FREDERICK J. TEGGART. [Pp. ix + 162.] (New Haven: Yale University Press; Oxford University Press, 1918. Price \$1·25.)

PROF. TEGGART, of the University of California, aims in this book at dealing with historical material by the methods of science. He desires to do for human history what biologists are doing "for the history of the forms of life," and attempts to analyse "the factors and processes manifested in the history of man."

The book is typical of a large proportion of modern American literature. It embodies an idea of considerable interest and value; it shows penetration and shrewdness; but suffers from a certain vagueness and lack of colour in working out the idea in the concrete. It is easy to read the book, without disagreeing with any part of it, and yet to feel that nothing particular has been accomplished in doing so. The book is a discussion, rather than a treatise leading up to positive conclusions. Positive conclusions, nevertheless, there are: though of an ill-defined character. The author points out the fallacy of supposing that "progress" is natural to man. On the contrary, he holds progress to be exceptional; and the first subject of study must be the processes "that make for fixity and stagnation." These in the main are the limitations of thought, imposed upon every individual by the traditions of his human environment, by his education, and by the general social tendency towards a fixation of existing ideas. Various factors tend, however, to bring about change by slow and small steps. Deeper and more sudden changes are due to national conflicts which subvert the former "idea-systems" of the groups involved in the struggle, emancipate the mind from its normal shackles, and leave the ground ready for new systems. Such subversions may be felt as public calamity or personal loss, but they lead to a break-up of the old, and to initiation of new ideas.

In doctrines of such wide generality, there is not much to be said by way of criticism. The author is well equipped for his task; and it may be hoped that he will later on pursue the subject into somewhat more precise channels and more concrete conclusions than are here presented.

HUGH ELLIOT.

War. By RONALD CAMPBELL MACFIE, M.D., LL.D. [Pp. viii + 72.] (London: John Murray, 1918. Price 3*s*. 6*d*. net.)

A MAN with a fly in his eye cannot do justice to a landscape, however beautiful. In discussing Dr. Campbell Macfie's fine poem, it is advisable first to get rid of the fly. No sane critic will object to an archaism or neologism as such. *Le poète prend son bien où il le trouve*, and the only criterion of success is "does it come off?" Now, when Dr. Macfie, talking of the beginnings of vegetable life, speaks of "a spark of green, a little speck, a tiny spore . . . on the vast *savannahs* of the shore," the word "savannahs" seems to us admirable; or, again, when describing the cooling of the earth, he says "A clattering *clinker* fell of iron snow," we not only seem to witness but actually hear the very effect he describes.

But there are several words and phrases which unfortunately evoke reminiscences of the Jabberwock or of Oliver Wendell Holmes' "evase, erump," such as "the tenuous ghosts that . . . *swither and sweal*," the "*blore* of tempest," "the *phrentic* tide," "*luting*" (twice), "incalescent," "the *tabid* tundra-land."

And now we have disposed of the fly, let us hasten to do homage to a magnificent theme often magnificently thought out, especially in its opening

section. Taking war as his subject, Dr. Macfie describes in a series of splendid pictures the making of the globe, beginning with the nebula curdling to an incandescent ball of fire, around which "burned and boomed a plangent sea," and on which—

> There hissed a heavy hail of falling stars
> Whose flick upon the lava's filigree
> Made rosy scars.

Lack of space prevents our quoting many passages in which the imagination of the poet can only be described as white-hot, as those depicting the formation of strata, the birth of the sea, or the evolution of sea-marsh and mountain range. In fact this first section of the poem would be a splendid thing to read aloud to a classical VI. form in order to wake in them that wonder which is the soul of science, and whose aftermath is reverence. The rest of the poem deals with the present war. It, too, is on a high level, but the theme has been so frequently treated by others, there is less room for originality.

CLOUDSLEY BRERETON.

BOOKS RECEIVED

(Publishers are requested to notify prices)

De Wijsbegeerte der Wiskunde van Theistisch Standpunt. By D. H. Th. Vollenhoven. Amsterdam: Wed. G. van Soest, 1918. (Pp. xv + 444.)

Mathematical Tables and Formulæ. By P. Abbott, B.A., Head of the Mathematical Department, The Polytechnic, Regent Street, W. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. iv + 58.) Price 2*s*. net.

Numerical Trigonometry. By P. Abbott, B.A., Head of the Mathematical Department, the Polytechnic, Regent Street, W. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. x + 33, with diagrams.) Price 5*s*. net.

Projective Geometry. By Oswald Veblen, Professor of Mathematics, Princeton University, and John Wesley Young, Professor of Mathematics, Dartmouth College. Volume II, by Oswald Veblen. London: Ginn & Company, and Boston, New York, and Chicago. (Pp. xii + 511.) Price 21*s*. net.

Elements of the Electromagnetic Theory of Light. By Ludwik Silberstein, Ph.D., Lecturer in Natural Philosophy at the University of Rome. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. vii + 48.) Price 3*s*. 6*d*. net.

An account, in vectorial notation, of the fundamental part of Maxwell's theory, translated from the author's Polish treatise on Electricity and Magnetism. The treatment, which is extremely compact, includes a discussion of the propagation of light in crystalline media, while, in addition, eleven pages are devoted to an admirable discussion of the difficulties (due to the longitudinal waves and boundary conditions) which have confronted the "elastic" theory, and to the attempts which have been made to overcome them.

Simplified Method of Tracing Rays through any Optical System of Lenses, Prisms, and Mirrors. By Ludwik Silberstein, Ph.D., Lecturer in Natural Philosophy at the University of Rome. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. ix + 37, with diagrams.) Price 5s. net.

The Teaching of Physics in Schools. The Physical Society of London. London: Fleetway Press, Ltd., 1, 2, and 3, Salisbury Court, Fleet Street, 1918. (Pp. 43.) Price 1*s*. net.

New Reduction Methods in Volumetric Analysis. A Monograph. By Edmund Knecht, Ph.D., M.Sc., Tech.F.I.C., Professor of Technological Chemistry, Victoria University of Manchester, and Head of the Chemical Department, Municipal College of Technology, Manchester, and Eva Hibbert, M.Sc., Tech. Assistant Lecturer in Applied Chemistry, Faculty of Technology, Victoria

University of Manchester. Reissue with additions. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Calcutta, Bombay, and Madras, 1918. (Pp. x + 135.) Price 5*s*. net.

This reissue of the earlier 1910 edition, dealing with the use of titanous salts for volumetric estimations, particularly of synthetic dyes, should prove of considerable value at the present moment. Various additions, including a section on the estimation of Alizarine and other dyes, are given as addenda.

Food Gardening. For Beginners and Experts. By H. Valentine Davis, B.Sc., Lecturer in Rural Science and Gardening in the Cheshire County Training College, Crewe; Member of the Royal Horticultural Society's Panel of Expert Gardening Advisers. Second Edition, Revised and Enlarged. London: G. Bell & Sons, 1918. (Pp. viii + 133). Price 1*s*. net.

The Portal of Evolution. Being a Glance through the Open Portal of Evolution at some of the Mysteries of Nature. By a Fellow of the Geological and Zoological Societies. London: Heath Cranton, Ltd., Fleet Lane, E.C. (Pp. xvi + 295.) Price 16*s*. net.

Land and Freshwater Mollusca of the British Isles. A Monograph. By John W. Taylor, F.L.S., Member Honoraire de la Société Malacologique de France, Ex-President of the Conchological Society of Great Britain and Ireland, Late Editor of the *Journal of Conchology*. With the Assistance of W. D. Roebuck, F.L.S., the late Chas. Ashford, and other well-known Conchologists. Leeds: Taylor Brothers, 1900. Part VI. (Pp. 321-384, 4 plates.) Price 6*s*. net.

A Junior Course of Practical Zoology. By the late A. Milnes Marshall, M.D., D.Sc., M.A., F.R.S., Professor in the Victoria University; Beyer Professor of Zoology in Owens College, Manchester; Formerly Fellow of St. John's College, Cambridge; and the late C. Herbert Hurst, Ph.D., Lecturer in the Victoria University, Demonstrator and Assistant Lecturer in Zoology in Owens College, Manchester. Eighth Edition. Revised by F. W. Gamble, D.Sc., F.R.S., Mason Professor of Zoology and Comparative Anatomy in the University of Birmingham. London: John Murray, Albemarle Street, W., 1918. (Pp. xxxvi + 515.) Price 12*s*. net.

War Neuroses. By John T. MacCurdy, M.D., Psychiatric Institute, Ward's Island, New York, Lecturer on Medical Psychology, Cornell University Medical School, New York, with a Preface by W. H. R. Rivers, M.D., Fellow of St. John's College, Cambridge. Cambridge: at the University Press, 1918. (Pp. ix + 132.) Price 7*s*. 6*d*. net.

Equilibrium and Vertigo. By Isaac H. Jones, M.A., M.D., Laryngologist, Philadelphia General Hospital. With an Analysis of Pathological Cases, by Lewis Fisher, M.D., Laryngologist and Otologist, Mount Sinai Hospital, Philadelphia. Philadelphia and London: J. B. Lippincott Company. Pp. xv + 444, with 130 illustrations.) Price 21*s*. net.

Dr. John Radcliffe. A Sketch of His Life, with an Account of His Fellows and Foundations. By J. B. Nias, M.D., F.R.C.P., Radcliffe Travelling Fellow, 1882–5. Oxford: at the Clarendon Press, 1918. (Pp. 147.) Price 12*s*. 6*d*. net.

The Life and Letters of Joseph Black, M.D. By Sir William Ramsay, K.C.B., F.R.S. With an Introduction dealing with the Life and Work of Sir William Ramsay by F. G. Donnan, F.R.S. London: Constable & Co., 1918. (Pp. xix + 148, with 7 plates.) Price 6*s*. 6*d*. net.

Men of Science Series. Edited by S. Chapman, M.A., D.Sc. London: Society for Promoting Christian Knowledge, 1918. Price 2*s*. net each.

Alfred Russel Wallace. The Story of a Great Discoverer. By Lancelot T. Hogben, B.A., B.Sc., Formerly Senior Scholar of Trinity College, Cambridge. (Pp. 64.)

The Life and Discoveries of Michael Faraday. By J. A. Crowther, Sc.D., Late Fellow of St. John's College, Demonstrator in Experimental Physics in the Cavendish Laboratory, Cambridge. (Pp. 71.)

Galileo. By W. W. Bryant, F.R.A.S., of the Royal Observatory, Greenwich. (Pp. 64.)

Colour and the Child. By Peshoton Sorabji Goolbai Dubash, D.Sc., M.R.San.I., Phys.M., F.B.E.A., with Foreword by Dr. T. H. Hayward, Litt.D., B.Sc., the Inspector of London County Council Schools. Published for the International College of Chromatics, 3, Finsbury Square, London, E.C.2. (Pp. 40.) Price 2*s*. net.

Civic Biology. A Textbook of Problems, Local and National, that can be solved only by Civic Co-operation. By Clifton F. Hodge, Ph.D., Professor of Social Biology in the University of Oregon, and Jean Dawson, Ph.D., Department of Sanitation, Board of Health, Cleveland. London: Ginn & Company, and Boston, New York, and Chicago. (Pp. viii + 381.) Price 7*s*. net.

Messrs. Adam Hilger, Ltd., of 75A, Camden Road, N.W.1, announce that their Abbe Refractometer with Water-Jacketed Prisms, October 1st, 1918, is now on the market.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

RECENT ADVANCES IN SCIENCE

PHILOSOPHY. By Hugh Elliot.

Recent philosophical publications have not been unimportant from the standpoint of metaphysics, but have in general not been such as to interest the worker in science or to promote the advancement of natural knowledge. The tendency to spiritualistic, as opposed to materialistic interpretations continues unabated. It would appear that the gross *ethical* materialism involved by war has reacted against all forms of *scientific* materialism by confusion of two ideals that have in truth nothing whatever in common with each other. In our last philosophical review we referred to the increasing interest taken in pure spiritualism. In the region of twilight on the borders of every science, speculators find scope to exercise their fancy till such time as their imaginings are dissipated by the rising light of knowledge. Yet the twilight falls so gradually that it is often hard to draw the line between genuine and spurious speculation. Thus Newton warned Physics to beware of Metaphysics ; but modern researches on the constitution of matter show how difficult it is to draw the dividing line. Still more gradually does biology merge into meta-biology, vitalism and so on.

Some important articles on the philosophic bearings of the electron theory of matter have been published by M. L. Rougier in the *Revue Philosophique*, under the title " La Matérialisation de l'Energie." He sets forward two alternative theories, the first of which he calls the dematerialisation of matter. This is the theory of æther, in which matter is regarded as the location of strain, condensation or destruction, in a *milieu* possessing inertia and mechanical properties. This theory he rejects owing to the incompatible properties which it is necessary to attribute to the æther. The second theory is that of the materialisation of energy : energy passes out of the " phantom domain of the imponderable " and comes forth possessed of inertia, weight, and structure, manifesting itself in the two

forms of matter and radiation. This theory he adopts as making unnecessary the conception of æther. The dualism still remains, however; if it is no longer matter and energy, it is positive and negative electricity. The antithesis is to some extent reduced, but it is still there; nor is speculation likely to cease until some hypothesis of æther is framed which renders monism intelligible, or until it is shown that monism can never be reached, but that dualism is a fundamental fact, not capable of further analysis.

Prof. James Gibson has published a very complete study of Locke's Theory of Knowledge. He makes a microscopical examination of the *Essay Concerning Human Understanding*, aiming at exposition and a true interpretation, rather than criticism. The main value of the book is therefore historical rather than philosophical. An elaborate study is carried out of the relations of Locke to Scholasticism, to Descartes, Hobbes, Leibnitz, and Kant. The line of thought culminating in Hume is omitted from this examination, which nevertheless is an important contribution to the history of philosophy.

Prof. J. S. Mackenzie has aimed at a more ambitious project in his *Elements of Constructive Philosophy*. This is a survey of the whole field of philosophy, in which however most attention is devoted to ethical conceptions and to the subject of infinity. Prof. Mackenzie's preoccupation with ethics somewhat militates against the scientific interest of the book. Science seeks what is true; ethics seeks what is right; a mixture of these two aims does not conduce to lucidity of thought. Thus, we fail to see what result is gained by an argument in favour of "The right to hope." Prof. Mackenzie urges that where the solution of a certain problem cannot be proved, we still have "the right to hope" that it is true, and we have also the right to entertain so much belief as is necessary to support the hope. That in plainer words is saying that, if we desire a thing to be true, we are *ipso facto* justified in attaching a certain amount of credence to it. We are far removed here from the scientific spirit, where belief is determined by exclusive reference to objective facts, to the complete suppression of personal predilections. The instance given is the proposition that the Universe can only be made intelligible "by thinking of it as constructed in accordance with the idea of perfection." But perfection is a mere idea of the human mind: the æsthetic sentiments

are no criterion of what actually exists in nature. The very same argument of the necessary perfection of the cosmos led the ancients to believe that the orbits of the planets must be circles, for circles are the only " perfect " curves ; and that their number could not be more than seven, for seven is the only " perfect " number. Ideas of perfection change so radically from age to age, and from race to race, that it is impossible to suppose the cosmos to be constructed on so shifting a foundation. Here we have an example of the confusion of science and ethics. The " laws " of science are the same in all places and times. Those of ethics are relative to the people who live under them, and to the different circumstances in which they find themselves.

A translation has been published of M. Deshumbert's *La Morale fondée sur les Lois de la Nature*, under the English title of *An Ethical System: Based on the Laws of Nature*. This presents a system of Evolutionary Ethics of the type made popular last century by Herbert Spencer. It defines Good as " everything that contributes to the conservation and the enlargement of life," and Evil " everything that diminishes life to no purpose." There is nothing new in this : such novelty as may be found in the book seems to be in the association between the ideas of the Chinese philosophers with the idea of evolutionary ethics. The confusion between science and ethics is carried to a higher degree than in the volume previously noted. Nature is personified, after the manner of a god. The author deems it necessary to answer objections to the theory that ethics is founded on natural laws, such as the objection that " Nature is cruel." It is hard to see what this has got to do with it ; harder still to perceive why M. Deshumbert thinks it incumbent on him to deny the cruelty of nature. He is thus at some pains to prove that the experience of being eaten by a lion is neither terrifying nor painful ; and as for being frozen to death, it is one of the most agreeable forms of quitting our earthly existence. A number of instances are given to prove the " intelligence " of nature : the argument being that many natural processes resemble processes invented by man ; thus the sting of a wasp resembles a hypodermic syringe ; the human eyelid resembles a wet rag used for cleaning windows. Finally a number of " duties and precepts " are enunciated, such as those of avoiding alcohol, not giving beer to children, consulting

a dentist twice a year, loving the "whole," frequently washing one's hands, and not being obstinately stupid.

The British Journal of Psychology for October 1918 prints several contributions to a Symposium at a Joint Session of the British Psychological Society, the Aristotelian Society, and the *Mind* Association, on the subject "Why is the 'Unconscious' Unconscious?" The discussion was opened by Capt. Maurice Nicoll, who followed the doctrines of Jung, in their more metaphysical aspect. He was opposed by Dr. W. H. R. Rivers and by Dr. Ernest Jones, the latter of whom likewise contributes an article on "The Theory of Symbolism." An interesting article is contributed by Prof. Carveth Read on "The Mind of the Wizard," in which he endeavours to account for the hold made by superstition on primitive peoples.

Finally we must allude to Prof. James Ward's *Psychological Principles,* in which that well-known writer greatly expands and renders in a permanent form his famous article on "Psychology" in the *Encyclopædia Britannica.* The work is probably the most important contribution to psychology of recent years, though it pays little attention to many of the more modern lines of inquiry, and approaches the subject rather from the purely psychological than from the physiological standpoint.

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

FOR the sake of brevity in the following account, which aims at some completeness, we will refer, for those papers which have not been examined at first hand and which seem to be important, to the last part of the *Revue Semestrielle des Publications Mathématiques* (1918, **26** [2]), which is supposed to contain references, titles, and often short accounts of all papers in pure and applied mathematics published between October 1917 and April 1918. Thus, when a man's name in what follows is followed merely by a number enclosed in parentheses, the number is a reference to the page of that part of the *Revue* just mentioned. The *Revue* is not supposed to contain critical remarks on the papers of which it gives an account, but this part contains an exception: a fundamental error is pointed out in A. Kempe's (51) attempt to prove the impossibility of an algebraic solution of the equation of degree higher than the fourth. Critical accounts have been attempted here, however

slightly, where it has seemed possible and necessary. It may be added that the London agent for the *Revue* is now D. J. Bryce, of 149, Strand, W.C.2.

There is an account of the contents of several new volumes of the *Encyklopädie der mathematischen Wissenschaften* on pp. 68–9 of the above part of the *Revue* : the volumes referring to pure mathematics which were published in 1914 and 1915 are concerned with elliptic, automorphic, and modular functions ; parts of the theories of ordinary and partial differential equations and of algebraic analysis ; projective, descriptive, and elementary geometry ; systems of co-ordinates ; algebraic curves and surfaces ; contact transformations ; and geometrical theory of differential equations.

G. A. Miller (*Amer. Math. Monthly,* 1918, **25**, 383–7 ; cf. 428) discusses, with an example, how a mathematical encyclopædic dictionary should be written. A contribution to this discussion is made in another article in the present number of SCIENCE PROGRESS.

Among the papers read at the annual meeting of the Mathematical Association on January 1 and 2, 1919, were T. P. Nunn's Presidential Address on " Astronomy as a School Subject," W. P. Milne's account of the work of the Association in the application of mathematics to industry, B. A. Howard's discussion of the teaching of elementary geometry, and S. Brodetsky's graphical treatment of differential equations (*Nature,* 1919, **102**, 395–6).

Further detailed suggestions for programmes of undergraduate mathematical clubs (cf. SCIENCE PROGRESS, 1919, **13**, 345) are given (*Amer. Math. Monthly,* 1918, **25**, 316–20, 358–60, 411–4) respectively on geometry of four dimensions (by H. P. Manning), constructions with a double-edged ruler, and the cattle-problem of Archimedes.

J. Nyberg (*ibid.* 337–40) shows how, in conformity with his papers of 1916 and 1917, he would introduce exponential and logarithmic functions. G. A. Miller (*ibid.* 287–90) gives a useful exhibition of various contradictory definitions current of a " discriminant."

History.—L. M. Klinkenberg (51, 52) gives an account of the origin and history of geometry in Egypt, and with Thales, Pythagoras, and Plato and his contemporaries. H. B. Fine (10) gives a commentary on part of the fifth, sixth, seventh,

and tenth books of Euclid. H. Wieleitner (20) writes on new researches on the most ancient Arabian mathematics.

W. H. Bussey (*Amer. Math. Monthly*, 1918, **25**, 333–7) gives a description of Fermat's "method of infinite descent," which is a kind of parallel to the method of mathematical induction on which Bussey wrote previously (cf. SCIENCE PROGRESS, 1918, **12**, 362). F. Cajori (*Amer. Math. Monthly*, 1918, **25**, 291–2), who in 1910 discovered grounds for the attribution of "Rolle's theorem" to Rolle, inquires as to the origin of the name "Rolle's curve." Some information as to the probable connection, through Barrow, of the mathematical methods of Galileo and Newton is given by Philip E. B. Jourdain (*Monist*, 1918, **28**, 629–33). A very full annotated translation of C. I. Gerhardt's article of 1891 on the influence of Pascal on Leibniz is given by J. M. Child (*ibid.* 530–66). Gertrud Klein (18) discusses Euler's solution of the isoperimetric problem.

F. Amodeo (47) studies the life and work of Ottavio Colecchi (1773–1847), who was interested in the philosophy of mathematics. P. Barbarin (35) writes on the dilemma of Johann Bolyai. With regard to Gauss, the twelfth report of F. Klein (25) on the state of the edition of Gauss's *Werke* has been published; of F. Klein, M. Brendel, and L. Schlesinger's (14, 70) *Materialien für eine wissenschaftliche Biographie von Gauss*, the fourth part (A. Galle) and fifth part (P. Stäckel) were published at Leipzig in 1918; and A. Loewy (18) publishes a contribution to Jewish chronology from the papers that Gauss left behind him.

Of the accounts of the life and work of more modern mathematicians we may mention O. Blumenthal's (15) of Karl Schwarzschild, F. Dingeldey's (15) of Sigmund Gundelfinger, E. Study's (16) of Franz London, Th. Schmid's (16) of E. Janisch, S. Pincherle's (47) of Cesare Arzelà, and G. Helm's (12) of Ernst Mach. A notice by M. J. M. Hill of the life and work of Olaus Henrici (1840–1918) is given in *Nature* (1918, **102**, 189–90), and a short sketch of the work of Maxime Bôcher, who also died in 1918, in the *Amer. Math. Monthly* (1918, **25**, 373–4). Gaston Milhaud, who died on October 1, 1918, in his sixty-first year (*Nature*, 1918, **102**, 370), had been engaged for some time on a special study of Descartes (cf. SCIENCE PROGRESS, 1918, **13**, 177).

S. C. van Veen (51, 52) gives an account of the contributions

to the theory of prime numbers of Stifel, Fermat, Lambert, Legendre, Gauss, Dirichlet, and Tschebychef.

Logic, Principles, and Theory of Aggregates.—J. S. Taylor (9–10) gives a complete existential theory of Bernstein's set of four postulates for Boolean algebras. D. Hilbert (25) makes what seems to be an exaggerated claim that "everything which can be an object of scientific thought falls, as soon as it is ripe for the formation of a theory, to the axiomatic method." L. E. J. Brouwer (48) gives a foundation of the theory of aggregates which is independent of what he thinks is the (mathematically) unpermissible logical theorem of the excluded middle: apparently this point of view is due to the fact that it appears to some that this theorem leads to contradictions in mathematics. B. Russell (*Monist*, 1918, **28**, 495–527) gives two lectures of his on the "philosophy of logical atomism," to which mathematical considerations have led him, and deals with facts and propositions, particulars, predicates, and relations. It seems to be unfortunate that Russell persists in regarding a proposition as a "symbol," although he admits that propositions are either true or false. Miss D. M. Wrinch (*ibid.* 620–3) gives an account of recent work in mathematical logic. Philip E. B. Jourdain (SCIENCE PROGRESS, 1918, **13**, 299–304, cf. 178) shows clearly that his method does not depend on arbitrary selections.

On the theory of relativity, A. Einstein (11–12) gives some cosmological considerations, and C. Cailler (67) applies quaternions to Lorentz's transformation.

H. M. Westergaard (*Monist*, 1918, **28**, 613–20) gives a very useful comparison of work on the conception of probability and adheres to the "statistical method" due to Montessus. A. Berger (62) shows that, for an infinity of cases of observation, it follows from the existence of a most probable value that this value is the arithmetical mean. R. Schumann (56–7) has a long paper on the determination of a straight line by the method of least squares. G. Pólya (58) writes on geometrical probabilities. S. D. Wicksell (66–7) writes on the genetic theory of frequency.

N. Lusin and W. Sierpinski (33) write on a property of the continuum, and also (32) on a decomposition of an interval into a non-enumerable infinity of non-measurable aggregates. H. Rademacher (60) gives a theory of measurable correspon-

dences (*Abbildungen*). M. Fréchet (38) gives an extension of the theorem which Borel based on one of Heine to the theory of abstract aggregates.

Arithmetic and Theory of Numbers.—A. Schreiber (12) gives arithmetical and geometrical considerations on the "golden section." E. Cahen (32) studies the best sequence of approximation to a given number. J. H. Baudet and F. Schuh (50) give some properties of certain fractions. Cf. also M. Kiseljak (62).

Questions in indeterminate analysis are dealt with by W. de Tannenberg (34), E. Maillet (39), and F. Pollaczek (57); congruences by S. Bergmann (62), A. Arwin (63–4), and V. Voss (27); binary quadratic forms by G. Humbert (32, two papers); division of the circle by S. Szilárd (55), G. Rados (54), and A. Loewy (15); partition of numbers by L. von Schrutka (59); various numerical functions by J. G. van der Corput (50) and K. Szilysen (53); the theory of algebraic numbers by J. A. Schouten (22), Ph. Fürtwangler (58, 60), M. Bauer (55, second paper), O. Mühlendyck (62), A. Ostrowski (18), K. Hensel (18–19), and E. Landau's (70) book of 1918; and Borel's theorem on "normal" numbers by W. Sierpinski (40) and H. Lebesgue (40).

Algebra.—An extension of Rolle's theorem to the case of many variables is given by M. T. Beritch (36); the solubility of equations is considered by G. Rados (52); the theory of Galois' groups by E. Noether (22) and F. Seidelmann (22); the determination of roots by M. Bauer (55), E. Bálint (55), and K. Bohlin (65); and C. Runge (13) gives graphical methods for finding complex roots. Algebraic forms are discussed by E. Waelsch (56, 57) and Gy. Farkas (53); combinatory analysis by G. Usai (47), F. Schuh (50), and J. du Saar (51); determinants and symmetric functions by Sir T. Muir (1, two papers), G. Frobenius (12), C. Kostka (19), and O. Szász (62–3); matrices by H. T. Burgess (9) and A. Loewy (14, 24); and rational substitutions by P. Fatou (34), G. Julia (34, 35) and S. Lattès (34, 35).

C. C. Bramble (*Amer. Journ. Math.* 1918, **40**, 351–65) derives a collineation group isomorphic with the group of the double tangents of the plane quartic. The isomorphisms of the general, infinite group with two generators are studied by J. Nielsen (25), and the primitive, metacyclic congruence groups with three or four variables by G. Bucht (64).

Sir Ronald Ross (SCIENCE PROGRESS, 1918, **13**, 288–98) gives many theories on his "verb-functions," mostly connected with "operative involution."

D. N. Lehmer (*Amer. Journ. Math.* 1918, **40**, 375–90) gives an arithmetical theory of certain Hurwitzian continued fractions, of which the origin was the empirical discovery of such facts as that the denominator of the convergent of order $3n$ in the regular continued fraction which represents e is divisible by n. The theory of continued fractions is also treated by I. Schur (12), O. Szász (54), G. Humbert (32, 33), A. Arwin (67), and A. Pringsheim (27). Questions about infinite series in general are investigated by F. Carlson (11), A. Pringsheim (26), M. Petrovitch (32), and W. Gross (62, first paper); and special series (product for trigonometric functions) by G. Kowalewski (18), and (Tschebychef's polynomials) by J. Chokhate (35).

Analysis.—On the theory of functions of real variables—continuity and differentiation: H. Hahn (17, 57), K. Knopp (17), C. Burstin (60, 61), A. Haar (23), Ch. J. de la Vallée Poussin (39), and C. Carathéodory's (68) book of 1918; integration: G. A. Bliss (8), S. Lefschetz (46), O. Perron (27), F. Riesz (17, 55), and A. Denjoy (31). On trigonometric series, see U. Dini (46), M. Angelesco (32), G. H. Hardy and J. E. Littlewood (34), W. H. Young (32), M. Akimoff (34), G. Szegö (53–4), and L. Fejér (61); and on orthogonal functions G. D. Birkhoff (8), W. Sternberg (8), and H. Laudien (19).

The general theory of analytic functions of complex variables is dealt with by R. König (13, 17), J. von S. Nagy (15), L. Fejér (15–16, 26–7), W. Gross (17), J. Schur (18), G. Hamel (22), G. Pick (23), R. Jentsch (23), G. Pólya (23), L. Bieberbach (23), G. Rémoundos (32), M. Fréchet (33), H. von Koch (65, 67), F. R. Berwald (65–6), S. Wigert (65), A. Denjoy (35, 48, 49), F. Iversen (35), D. Pompeiu (36), G. Valiron (41), M. Beke (53), F. Riesz (54), and P. Csillag (55); and conformal representation by G. Pick (58), R. Fürth (56), and E. Hille (64).

For special functions, we have (Gamma functions, factorials, etc.), N. Nielsen (11), P. H. Dojes (49–50), E. Stridsberg (64) arithmetical functions, E. Hecke (14); Legendre's functions, A. Haar (21), W. H. Young (33), P. Humbert (34); Dirichlet's series, E. Cotton (40) and M. Fekete (54). A. B. Coble (*Amer. Journ. Math.* 1918, **40**, 317–40) showed that the connection

which appeared, in a special case, in his studies (1915–17) of point sets and Cremona groups, between the point set and theta modular functions, must subsist in the general case. On elliptic functions, see O. Gruder (57–8); and on algebraic functions, G. Giraud (34), A. Buhl (35, 36), E. Fischer (19), and G. Scorza (32).

To the calculus of variations belong papers by I. A. Barnett (9), A. Haar (52), W. Gross (60, 61), and L. Lichtenstein (61); and to the functional calculus those by C. A. Fischer (8, 9), E. Hecke (25), M. Fréchet (32), W. de Tannenberg (33), E. Picard (34), P. Fatou (35), T. Lalesco (36, 40), H. B. A. Bockwinkel (49), L. Crijns (50), and F. Riesz (54).

W. Van N. Garretson (*Amer. Journ. Math.* 1918, **40**, 341–50) considers the asymptotic solution of the non-homogeneous linear differential equations of the *n*th order, and determines a particular solution in the form of quadratures. Use is made chiefly of the work of Dini (1898–9) and Love (1914). Reduction of certain ordinary differential equations of the first order is treated by W. D. MacMillan (9, two papers); differential equations with fixed critical points by W. Gross (42); and a new existence-proof for integrals of a system of ordinary differential equations by O. Perron (24–5). See also R. Garnier (35), P. Appell (40–41), F. Mertens (57), and A. Winan (66). On the differential equations of dynamics, see F. Engel (12-13), A. Einstein (12), and K. Bohlin (66). On total differential equations, see E. Goursat (33); on partial differential equations, see H. Duport (32) and P. E. Gau (36); on boundary problems with linear partial differential equations, see R. Bär (21); on functional equations, see above and E. Hilb (21); and, on a non-linear difference equation, see J. Horn (17).

Geometry.—On principles, generalities, and *analysis situs*, see O. Veblen (8), M. Pasch (19), G. Haessenberg (22), S. Strascowitz (24), A. Denjoy (35–6), P. Heegaard (38), A. Kowalewski (57, 59), and J. Lense (59). On the geometry of the triangle, see R. Goormaghtigh (51). Sir R. Ross's (SCIENCE PROGRESS, 1919, **13**, 485–6) new trigonometric functions have advantages in the solution of triangles.

A. Emch (*Amer. Journ. Math.* 1918, **40**, 366–74) shows how the theorem given by Pohlke in his work (1860) on descriptive geometry, its generalisations, and some related propositions,

may be proved in a comprehensive manner by making use of affine collineations in space. On Pohlke's theorem see also E. Kruppa (57).

On curves and line-complexes, see T. Cohen (17), L. P. Eisenhart (7, 8), F. Kölmel (15), H. R. Brahana (9), W. Gaedecke (15), H. Mohrmann (16, 21, 25, 26, 28), S. Jolles (18), J. Thomae (19), H. Falkenberg (22), H. Liebmann (25), H. Beck (27), K. Fladt (28), J. de Vries (49), H. J. van Veen (50), K. W. Rutgers (50), D. Postma (50), Gy. Nagy (55), G. Kohn (56), A. Plamitz (56, 58, 59), H. Beck (58), H. Rothe (59), P. Roth (60), L. Theisinger (60), E. Muller (61), K. Mack (61), and L. Braude (62).

On surfaces, G. M. Green (8) gathers together a few of his more important results concerning the general theory of curved surfaces and rectilinear congruences. On the generation of surfaces of the second order by correlations, see F. London (16); on quadrics, see H. Lebesgue (37); on algebraic surfaces, C. H. Sisam (37), F. Enriques (47); on infinitesimal geometry of surfaces, Z. Geöcze (54, two papers), C. Guichard (33–4), E. Turrière (38), A. F. Carpenter (46), S. W. Reaves (47), E. Cartan (39), A. V. Bäcklund (64); on curves on ruled surfaces, see C. Juel (11); on minimal surfaces, see J. K. Whittemore (9), E. R. Neovius (11); on the rigidity of convex polyhedra and other convex bodies, H. Weyl (12); theorems on convex bodies, W. Blaschke and G. Hessenberg (16; cf. Blaschke, 17); on Lie's transformation, M. Fouché (39); see also E. Muller (58, 59), L. Zängl (59), P. Lehmann (59), K. Zindler (61), and P. Roth (62). On gauche surfaces M. d'Ocagne (35) and R. de Montessus de Ballore (36).

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

A New Theory of Jupiter's Satellites.—Prof. W. de Sitter has published in the *Annals of the Leiden Observatory*, **12**, part 1, 1918, the outlines of an important new mathematical theory of Jupiter's satellites. A much abridged summary is also given in *K. Akad. Amsterdam, Proc.* **20**, 1289, 1918. The principal difficulty in the theory of the four old satellites of Jupiter arises from the fact that the mean motions of the three inner satellites are commensurable. The mean motion of the fourth satellite is not commensurable with those of the others, and its theory

does not present any particular difficulties, in so far as the periodic inequalities are concerned; it is similar to, but simpler than the lunar theory, for the ratio of the month and year is only about 1/260. The secular perturbations of the centre are, however, intimately connected for the four satellites, so that it is impossible to separate them, and the apparently simple and obvious course of having one theory for the three inner satellites and another theory for the fourth cannot be adopted.

In the old theory of the motion of the satellites, the undisturbed Keplerian eclipses are taken as intermediary orbits for the four satellites, and from these orbits by the addition of perturbations and variations, the complete solution is derived. This method of procedure is the best in the case of the fourth satellite, but not for the inner three. The new theory of de Sitter is chosen so as to be the best for the case of the inner satellites, and this more than compensates for the somewhat increased difficulties in the case of the fourth satellite.

The motion of the satellites can be described as a uniform motion in a circle with superposed inequalities. The inequalities in longitude and radius-vector can be classified according to period into four separate groups: (1) Short-period inequalities with periods not exceeding seventeen days, and including the "equations of the centre" and the "great inequalities"; (2) Inequalities with periods between 400 and 500 days: these inequalities are negligible in the radius-vector and are zero for the fourth satellite; (3) Librations of the inner satellites with a period of about seven years; (4) Long-period inequalities with periods longer than twelve years. The "great inequalities" and those of groups (2) and (3) arise through the commensurability of the mean motions and in the usual theory have small divisors.

In the new theory, intermediary orbits are used in which the expressions for the mean longitudes, mean anomalies, and longitudes of the perijoves of the satellites are rigorously satisfied. The special features which make this solution a good first approximation in the case of the three inner satellites are the moving perijove and the fact that the induced equations of the centre or "great inequalities" are larger than the free or ordinary equations. For the fourth satellite the free eccentricity is larger than the induced.

In the new theory the equations of the centre are treated

in much the same way as in the usual theory, but the great inequalities are not treated as perturbations, since they are of the first order. The intermediary orbit is not periodic, but contains only the principal terms of the periodic solution : to form the complete solution from this orbit are added perturbations arising from those parts of the perturbative function which were originally neglected and variations due to the fact that the actual constants of integration do not exactly fulfil the conditions of the intermediary orbit. With this method of treatment, the small divisors which arise in the old theory are avoided ; they enter the equations of condition of the intermediary orbit, but do not reappear after these have been solved.

The publications referred to above treat of the theory of the longitudes and radii-vectores ; the theory of the latitudes is to be treated in detail in the second part of the volume.

In connection with the derivation of the potential function an interesting point is noted. Certain terms were neglected by Laplace which contain a small factor whose value is dependent on the form of Jupiter and the distribution of its mass, but which there was then no means of evaluating. Similar terms were neglected in the complementary part of the perturbative function. The neglect of these terms was justifiable in view of the limit of accuracy which Laplace had set himself. Subsequent writers, however, have copied Laplace's perturbative function, neglecting the terms in question, although in the development of other terms quantities of the same or higher order of smallness have been included. The supposed degree of accuracy has therefore not been obtained.

The Motions of the B-Stars.—Reference was made in these notes in the last issue of SCIENCE PROGRESS (p. 359) to a new theory advanced by Prof. Perrine to the effect that many of the characteristics of spectral class amongst the stars generally are due to external causes depending upon location in the stellar system and associated with variations in the amount of cosmic matter in different parts. Further evidence bearing on the same theory is advanced in a paper in which the excess of the outward motion of the B-type stars is discussed in *Astroph. Journ*, **48**, 145, 1918. It is a well-established fact that after the elimination of the solar velocity, there is a strong

positive tendency in the residual line-of-sight velocities of the B-stars. This has tentatively been suggested by Campbell as due to a pressure-effect in the atmospheres of these stars. Perrine assumes that it represents a real outward motion, and attempts to find an explanation for it. Separating the stars into two classes of large and small proper-motion, which separation gives in the case of the B-stars a fairly complete sorting into near and distant stars, he finds that the outward motion occurs principally with the brightest and nearest stars. For the fainter stars, the proportion with velocities of approach increases for fainter magnitudes and smaller proper-motions. The preponderance of outward motion is not found with the late-type stars. Perrine accounts for these results on his theory that the great brightness and peculiar spectral condition of the B-stars result from the action of cosmical matter in the galactic region in which the B-stars are concentrated by supposing that a process of selection has been at work.' He imagines a ring of cosmical matter, the stars which are moving outwards pass into this ring of matter and become B-type stars. Some stars also enter it from outside, but such stars are approaching the observer and are fainter. In such a theory we would naturally expect the brighter and nearer stars of Class B to show almost entirely outward motions. These conjectures are interesting, although, as previously pointed out, their acceptance involves difficulties. Further facts need to be accumulated before the theory can be viewed in its proper perspective.

The Distribution and Dimensions of Stellar Clusters.—Results of considerable importance are obtained by Harlow Shapley in the seventh of his series of studies based on the colours and magnitudes in stellar clusters (*Astroph. Journ.* **48**, 154, 1918). It has been previously explained in these notes (SCIENCE PROGRESS, **12**, 552, 1918) how several collateral lines of investigation, based upon the properties of Cepheid variables, have enabled estimates of the distances of stellar clusters to be made with an accuracy far exceeding what would be possible by direct measurement. Shapley has applied these methods to determine the distances and distribution in space of sixty-nine globular clusters. One important result which is thus obtained is that there is a definite relationship between the

parallax and apparent diameter of a cluster, so that a measure of the latter enables an accurate determination of the parallax to be deduced. The average probable error of a parallax is only about 20 per cent.

The distribution of the clusters in space exhibits several important features. There is a strong tendency to concentrate towards the galaxy and the distribution with respect to the galaxy is remarkably symmetrical. The distribution in longitude is surprising; the region from 41° to 195° is completely devoid of clusters, whilst there is a remarkable concentration about a longitude of 325°. The most unexpected feature in connection with the distribution is the existence of a galactic belt within which no clusters are found: there is no cluster within 1,300 parsecs (about 5,000 light years) of the galaxy. Several possible explanations of this effect are considered, but all are rejected. In the *Observatory*, **42**, 82, 1919, Shapley shows that it is not an obscuration effect due to a cloud of absorbing matter. Thus, whilst no explanation of the result is arrived at, it is of importance as showing apparently that the clusters are related to the galactic system. If this is so, we must considerably enlarge our views as to the dimensions of the latter. Some further evidence on this subject is to be given in a later paper.

The derivation of the parallaxes of the clusters permits their actual dimensions to be calculated. The cluster Messier 3 may be taken as typical: it is 250,000 million million miles from the earth. To cross the cluster, light must travel 470 years. Its total mass is from one-quarter to half a million times the solar mass. A star of the brightness of the sun would appear to be of magnitude 21·5, whilst the variable stars in the cluster are nearly six magnitudes brighter.

Einstein's Relativity Theory of Gravitation.—Reference has already been made in these notes (Science Progress, **11**, 623, 1917) to Einstein's generalised relativity theory of gravitation, and to the remarkable success which the theory has obtained in providing a quantitative explanation of the motion of the perihelion of the planet Mercury. This success at once brought the theory into prominence and, in spite of certain difficulties arising from the nature of the boundary conditions which must be satisfied at infinity, it combined with the beauty of

the ideas involved to give the theory a wide degree of acceptance. The Physical Society of London has published a *Report on the Relativity Theory of Gravitation*, by Prof. A. S. Eddington, F.R.S. (London: Fleetway Press, Ltd., 91 pp. 6*s.* net), in which may be found an admirable summary of the ideas underlying the new theory, of their mathematical formulation, and of the results which are deducible therefrom. This is the best connected summary of the theory which has appeared in English and can be recommended to those interested in the subject.

The theory is already faced with difficulties. One of its deductions requires a displacement of the solar lines towards the red by an amount 0·008 tenth-metres, corresponding to a Doppler displacement of 0·63 km. per sec. We have previously referred to the result obtained by Dr. St. John at the Mount Wilson Observatory (SCIENCE PROGRESS, **13**, 15, 1918), who from a measurement of certain lines in the solar spectrum concluded that "within the limits of error, there is no evidence in these observations of a displacement to longer wave-length either at the centre or the limit of the sun, of the order of 0·008 A., as required by the principle of relativity." Owing to the importance of confirming or refuting this adverse conclusion, further evidence on this matter was of fundamental importance. Some results have recently been obtained by J. Evershed of the Kodaikanal Observatory (*Observatory*, **41**, 371, 1918, and **42**, 51, 1919), and these are also adverse to the theory. Mr. Evershed found that certain lines of cyanogen showed a displacement in the sun as compared with the arc of + ·005 Å. at the centre of the disc, or + ·008 Å. at the limb. These lines are not subject to a pressure shift, and the displacement might therefore be attributable either to motion in the line of sight or to a gravitational effect, as predicted by Einstein's theory. The former objection seemed improbable, as it would apparently require the earth to be controlling the motion and driving away the gas at all points on the sun's limb. On the other hand the displacement was of approximately the amount required by Einstein's theory: nevertheless, certain discrepancies were found in the displacements at various parts of the sun's disc, the magnitudes of which differed, not only *inter se*, but also from the predicted amount. In order further to test the theory therefore, Mr. Evershed conceived the beautiful idea of obtaining the spectrum of the *back* of the sun, by

photographing the integrated light of the sun reflected by Venus. A series of photographs was obtained when the angle at the sun was approximately 45°, 75°, 95° and 135°, and the surprising result was found that there was a progressive diminution of wave-length of the solar iron lines as Venus passed towards superior conjunction. With some lines a shift towards the *violet* was found in certain positions. It is therefore evident that the shift towards red of the solar lines only occurs on the side of the sun facing the earth, whereas a gravitational effect would be constant all over the sun. Not a single plate gave a contrary result. The displacements obtained appear therefore to be due to a recessive motion of the solar gases controlled by the earth, the cause of which is, however, at present obscure. There seems to be therefore no escape from the conclusion that the prediction from theory of a gravitational displacement of the solar spectrum lines is not verified.

It may be mentioned that in order to make accurate measurements of small displacements of the solar lines, Mr. Evershed devised a special method of measurement in which a positive spectrum is superposed on a negative and the double displacement measured. The method is described in the *Observatory*, **41**, 275 and 443, 1918.

It is possible that the remaining prediction of the theory which is susceptible of testing may soon be verified or disproved. The theory requires a displacement of the rays from a star by the gravitational field of the sun. The only possibility of testing this conclusion at present is by photographing the sun and the neighbouring stars during a total eclipse. In general, however, at the time of totality, the sun does not happen to be in a region of the sky containing sufficiently bright stars. At the forthcoming eclipse of 1919, May 29, however, the sun is very favourably situated on the Hyades and a unique opportunity is provided of testing the theory. Two expeditions have left this country under the auspices of the Joint Permanent Eclipse Committee of the Royal and Royal Astronomical Societies, one of which has gone to the island of Principe off the West Coast of Africa, whilst the other has gone to Brazil. Standard astrographic objectives of 13-inch aperture are being used, and the eclipse photographs will be compared with photographs of the same field obtained at night during last

winter with the same objectives. Although the weather prospects are not too favourable it is greatly to be hoped that one at least of the expeditions will meet with success. In the present state of the theory, the importance of this further test cannot be over-estimated.

The following is a selection of the more important papers which have appeared:

CRUM, W. L., The Perturbations caused by a Close Approach of Two Asteroids, *Ast. Journ*, **31**, No. 742, 1918.

HIRAYAMA, K., A Possible Explanation of the Gaps in the Distribution of the Mean Motions in Asteroids, *Proc. Math. Phys. Soc. Tokyo*, **9**, 264, 1918; Groups of Asteroids probably of Common Origin, *ibid.* **9**, 354, 1918.

STROMGREN, E., and FOSCHEN, J., Uber eine Klasse einfach periodischen retrograden Bahnen um die beiden endlichen Massen im problème restreint, *Ast. Nach.* **207**, No. 4968, 1918.

PUISEUX, P., and JEKHOWSKY, B., Sur les inégalités systématiques du contour apparent de la Lune, *Bull. Astr.* **35**, 161, 1918.

HARPER, W. E., The Orbits of the Spectroscopic Components of Boss 5173, *Journ. R.A.S.C.* **12**, 392, 1918; The Orbit of the Spectroscopic Binary 19 Lyncis, *ibid.* **12**, 395, 1918; The Orbit of the Spectroscopic Binary *h* Draconis, *ibid.* **12**, 446, 1918.

LUNT, J., *a* Centauri as a Spectroscopic Binary, *Astroph. Journ.* **48**, 182, 1918.

SEARES, F. H., and SHAPLEY, H., The Variation in Light and Colour of RS. Boötis, *Astroph. Journ.* **48**, 214, 1918.

PLUMMER, H. C., On the Distribution of the Stars, *M.N., R.A.S.* **78**, 668, 1918.

SHINJO, S., and WATANABE, Y., On the Rotation of Celestial Bodies, *Mems. Coll. Sci. Kyoto Impl. Univ.* **3**, No. 7, 1918.

HIRAYAMA, S., On the Mean Distances of Stars of Different Spectral Types, *Proc. Math. Phys. Soc. Tokyo*, **9**, 361, 1918.

LINDBLAD, B., Die photographisch effektive Wellenlänge als farbenäquivalente der Sterne, *Ark. for Mat. Ast. och Fysik.* **13**, No. 26, 1918.

EDDINGTON, A. S., On the Conditions in the Interior of a Star, *Astroph. Journ.* **48**, 205, 1918.

Mention must also be made of the following important volumes recently published:

Bigourdan, G., Observations des Nebuleuses et d'Amas Stellaires (5 vols., *Publications of the Paris Observatory*, 1918). These volumes contain in a collected form the whole of Bigourdan's important series of observations of nebulæ and clusters, which were previously to be found only in a scattered form throughout many publications.

Aitken, R. G., *The Binary Stars* (pp. xiv + 316, New York, 1918). Dr. Aitken is one of the foremost of living double-star observers. This volume, which is one of the Semi-Centennial Publications issued by the University of California, should be in the hands of every double-star observer.

PHYSICS. By James Rice, M.A., University, Liverpool.

The Origin of Spectra.—This subject is discussed in the Guthrie Lecture delivered by Professor McLennan of Toronto University and printed in the *Proc. Phys. Soc.* vol. xxxi. part i. December 1918. In this address certain experimental work carried out by Prof. McLennan and his pupils together with similar work carried out by other observers is summarised and brought under review. References are given in the reprint of the lecture and also in two papers contributed to the *Phil. Mag.* December 1918, by McLennan and two co-workers. One of these papers in fact discusses more briefly the points raised in the lecture.

The gist of the address is an attempt to show the connection between the fundamental frequencies of the series of lines exhibited in the spectra of various elements and the ionisation potentials of those elements, and the bearing which this connection has on the problem of atomic structure, especially Bohr's suggested solution of that problem. To follow the address the reader is assumed to be aware, in outline at least, of the work which has been done by Balmer, Rydberg, Kayser, Runge, Ritz and others on the discovery of formulæ which enable one to calculate all the lines in a given spectral series from the knowledge of a few constants. Perhaps the best known of these is the formula of Balmer which expresses the lines of the hydrogen spectrum ; it is

$$\cdot N\left(\frac{1}{4}-\frac{1}{n^2}\right).$$

Here ν represents the "wave-number," that is, the frequency divided by the velocity of light, or the number of waves in

1 cm. of a train of such waves; it is, in fact, $10^8/\lambda$ where λ is the wave-length in Angstrom units (10^{-8} cm.). N is a universal constant, known as Rydberg's constant, and has the value 109675 according to the most recent measurements. (Bohr has recently shown that this constant is very closely dependent on and calculable from the fundamental electronic charge and Planck's constant.) If now the integral series of values 3, 4, 5, 6, etc., be given to n in turn we obtain a set of numbers approximating with remarkable closeness to the wave-numbers of the lines of the hydrogen spectrum, the value $n = 3$ giving the well-known H_α line in the red (6563 Å.U.), $n = 4$ giving H_β (4861·5 Å.U.), and so on. It will be seen that as n gets greater the wave-number steadily increases, and one progresses by steps up into the ultra-violet region towards the limit $N/4$ or 27419, corresponding to a wave-length 3646 Å.U. Such a limit is referred to as the "convergence wave-number" of the series of lines. In the cases of other elements whose line spectra have been investigated the facts and formulæ are not so simple and much work is still required, not only to determine perfectly satisfactory formulæ, but also to give a theoretical basis for them. Bohr's theory of the atom does indeed give a very complete explanation of the Balmer formula for hydrogen and similar formulæ for some of the lighter atoms such as helium and lithium. The elements which have been most thoroughly investigated and for which some order has been evolved from the apparent chaos of their line spectra are the alkalis, the alkaline earths, and some of the metals, such as zinc, cadmium, magnesium. These elements are individually found to possess several series of lines (not merely one, as in the case of hydrogen) and in most cases also lines which have not yet been brought under any series law. There are, as might be expected, certain simple relations found to hold between the formulæ which express the different series for one element. The whole matter is somewhat involved, and a fairly complete account is given in Baly's *Spectroscopy*, 2nd edition, Chap. xvii. For our purpose at the moment suffice it to say that for any of these elements very good agreement is obtained by the use of the Rydberg formula

$$\nu = \frac{N}{(m+\alpha)^2} - \frac{N}{(n+\beta)^2}$$

where N is the universal Rydberg constant referred to above, m and n take integral values (or occasionally integral values $+ \cdot 5$, such as $1 \cdot 5$, $2 \cdot 5$, etc., and α and β are two constants (between zero and unity) which are definite for a given series of a given element, but which differ for different series even of the same element. In particular, for each of these elements there exist what are called " principal " series and what are called " sharp " series, which are related as follows: writing m equal to 1 (or perhaps $1 \cdot 5$) and then giving successive integral values to n we obtain the principal series; writing $n = 1$ (or $1 \cdot 5$) and giving successive integral values to m we get the sharp series (the minus signs which make their appearance in some cases are to be disregarded). Obviously the convergence wave-number of the principal series is $N/(1 + \alpha)^2$ or $N/(1 \cdot 5 + \alpha)^2$ as the case may be, and of the sharp series $N/(1 + \beta)^2$ or $N/(1 \cdot 5 + \beta)^2$. For convenience such an expression as $N/(m + \alpha)^2$ is devoted by the symbol (m, α), so that the general Rydberg formula for series of lines can be written

$$\nu = (m, \alpha) - (n, \beta)$$

Thus the Balmer series for hydrogen can be written

$$\begin{aligned} \nu &= N/(1 + 1)^2 - N/(n + 1)^2 \\ &= (1, 1) - (n, 1) \quad [n = 2, 3, 4, \text{etc.}] \end{aligned}$$

As another example there are series of lines in the arc spectrum of sodium which can be included in the formula by put ting $\alpha = \cdot 152$ and $\beta = \cdot 146$; *i.e.*

$$\nu = (m, \cdot 152) - (n, \cdot 146)$$

Thus putting $m = 1 \cdot 5$ and $n = 2, 3, 4$, etc., we get a principal series (the first principal series of sodium), the first line of which ($n = 2$) is, in fact, the well-known D_2 line (the more refrangible of the well-known doublet in the yellow). The D_1 line corresponds to $m = 1 \cdot 5$ and $n = 2$ in the second principal series of sodium which is

$$\nu = (m, \cdot 152) - (n, \cdot 145) \quad [m = 1 \cdot 5 ; n = 2, 3, 4, \text{etc.}]$$

This indicates (what is, as a matter of fact, the most usual occurrence) that series may be doubled or even trebled, the series being really series of doublets or triplets. It should be

mentioned in passing that besides the constants α and β, there are for most of the elements investigated a third set of constants γ which determine other series for each element than the principal and sharp, series usually called "diffuse" or "nebulous," these being given by the β and γ constants, viz.

$$\nu = (m, \beta) - (n, \gamma)$$

m being but equal to 1 or 1·5 and n taking up the successive integral values. Other names in the literature of the subject for the nebulous and sharp series are "first subordinate" and "second subordinate" respectively. Furthermore accompanying the lines of the series mentioned there have been observed for some elements neighbouring lines called "satellites" which are amenable to similar formal expression for their wave-numbers. As stated, the whole matter is involved, and requires careful study in order to avoid pitfalls, and these remarks are only to be taken as a hint of the methods which have been employed to bring order into an apparently unrelated mass of phenomena.

Coming now to Prof. McLennan's paper, he refers to a series of researches in which the heated vapour of some metals when traversed by electrons with sufficient kinetic energy are stimulated to emit line spectra. Thus, to quote a now classical instance, if heated mercury vapour is traversed by electrons with the kinetic energy acquired in passing between points, from the one to the other of which there is a fall on potential of 4·9 volts, the vapour emits a *monochromatic* radiation 2536·72 Å.U. This result is closely related to the quantum hypothesis, for it is easily calculated that

$$Ve = hf$$

where V is the P.D., 4·9 volts, expressed in electrostatic units, e is the electronic charge, h Planck's constant, and f the frequency corresponding to the above wave-length ($f = \nu.c$, ν being the wave-number and c the velocity of light).

McLennan, among others, has carried on researches in similar directions. Thus with zinc vapour the radiation emitted was 3075·99 Å.U., with cadmium 3260·17 Å.U., with magnesium 2852·22 Å.U. Moreover the impact voltages necessary to stimulate this radiation agreed with the quantum relation written above.

In these experiments no additional radiation was emitted as the electrons were speeded up to greater energies by increasing the impact voltages until such values were attained as caused an arc to be struck in each case between the terminals, producing the P.D., when the vapour in question emitted its full series arc spectrum. This arcing potential is 10·4 volts for mercury vapour. Further investigation revealed the fact that the above statement had to be qualified. Two investigators, Bergen and Davis, discovered that when the P.D., in rising from the value 4·9 volts, attained the value 6·7 volts, the mercury vapour was stimulated to emit radiation 1849 Å.U. in addition to the original 2536·72 Å.U., but still without arcing, *i.e. without ionisation*, and without emission of the full line spectrum which makes its appearance on ionisation. A further investigation of this point was made by McLennan and his pupils. For zinc it was found possible to stimulate radiation 2139·33 Å.U. as well as 3075·99 Å.U., mentioned above, and with cadmium vapour 2288·79 could be brought out as well as 3260·17 on reaching the suitable impact voltages as calculated by the quantum theory. The paper gives an account of the experimental methods employed, and practical details are also given in the *Phil. Mag.* papers mentioned above, and in other papers therein referred to. From the theoretical standpoint there are several interesting points. Each of the substances treated possesses definite series of lines, in particular a principal series defined by

$$\nu = (1{\cdot}5,\ \alpha) - (n,\ \beta) \quad (n = 2,\ 3,\ 4\ \ldots)$$

α and β being known constants for each element. It appears that the convergence wave-number $(1{\cdot}5,\ \alpha)$ of each series corresponds for each element to the ionisation potential calculated on the quantum basis (*e.g.* 10·4 volts for mercury vapour). Now this has a bearing on atomic structure and in particular on Bohr's theory of stationary electronic orbits. When it was discovered that mercury vapour could be stimulated to emit the radiation 2536·72 by an impact voltage of 4·9, it was thought that this corresponded to the ionisation potential of mercury, inasmuch as the vapour acquired conductivity. However, this conclusion was resisted and controverted by other work which pointed out that the conductivity was due not to the electrons bombarding the vapour, but to photo-

electrons ejected from the metallic electrodes (and possibly condensed layers of mercury near the cathode) by the stimulated radiation. Had this low value of the ionisation potential been maintained, it would have proved a serious obstacle to acceptance of the Bohr theory. For this theory postulates a number of stationary, non-radiating orbits in which the electrons travel around a central positive nucleus, the total energy of an electron (potential and kinetic) being greater the further out the orbit in which it is moving. Circumstances arise which cause an electron to "jump" from one orbit to another; if from an outer to an inner, the balance of energy between the two orbits is emitted as radiation with a definite frequency, f, which is determined by the quantum relation hf = the energy emitted; if, on the other hand, the jump is from inner to outer, energy of a corresponding amount must be absorbed. The further out an orbit is situated from which an electron jumps, and the closer in the orbit to which it jumps, the greater the frequency of the emitted radiation. In particular the greatest frequency for an atom would correspond to the passage of a "free" electron (*i.e.* one outside the influence of the atom and regarded "at infinity") into the innermost orbit, and conversely a jump from an orbit to a state of "freedom" would correspond to ionisation. Now if an atom is ionised, *i.e.* if it has lost an electron, there is no apparent reason why the return of an electron to the atom should give only one kind of radiation, since, there being several orbits, there are obviously possibilities for the emission of as many monochromatic radiations by an electron entering the atom from without. Consequently had the view that 4·9 volts is the ionisation potential of mercury vapour prevailed, we would have had to regard the bombarding electrons at this impact voltage imparting sufficient energy to electrons within certain of the atoms to eject them from these atoms, and that these electrons in returning to the ionised atoms only emitted a monochromatic radiation (2536·72 Å.U.), *i.e.* in all cases returned to a particular orbit. However, as pointed out, it appeared that 4·9 volts was too low a value for the ionising potential, and that probably the course of affairs consists in the bombarding electrons imparting energy only sufficient to carry the electron from an inner orbit to the next one, and the monochromatic radiation corresponds to a return.

In addition, a higher value for the impact voltage would give the bombarding electrons power to transmit sufficient energy to the orbital electrons to carry them from an inner orbit to the next but one, and then would arise the possibility of the electron returning to the next orbit as well as to the inner, giving rise to the emission of two monochromatic radiations, a view for which there is some experimental evidence referred to earlier. In fact the theoretical possibilities widen out with increasing impact voltages up to the ionisation limit, although we still await more precise experimental proof; and it would appear that the following view is probably sound. "It is possible to cause a vapour bombarded in a vacuum by electrons to emit a radiation consisting of one wave-length and one wave-length only, provided *all* the bombarding electrons possess kinetic energy given by the quantum relation $Ve = hf$, f being the frequency of the monochromatic radiation stimulated; moreover, it would appear that by gradually increasing the speed of the bombarding electrons the vapour may be caused to emit at successive stages radiation of shorter and shorter wave-lengths, each particular wave-length being stimulated only when the bombarding electrons have attained velocities corresponding to its frequency as indicated by the quantum relation."

The natural limit of this process is, of course, when ionisation occurs and then all the lines of the element's spectrum become possibilities, and of course among them the one of shortest wave-length and greatest frequency or wave-number; thus one would expect the convergence frequency to appear when the ionisation potential is reached; and it is a striking fact, already mentioned, that the ionisation potentials of mercury, zinc, cadmium, and magnesium (viz. 10·4, 9·4, 9 and 7·6 volts respectively) correspond to the convergence wave-numbers of their singlet principal series

$$\nu = (1\cdot5,\ a) - (n,\ \beta) \quad n = 2,\ 3,\ 4 \ldots$$

this convergence wave-number being $(1\cdot5,\ a)$ corresponding to $n = \infty$. Such a convergence number corresponds in fact to the return of an electron from without to an innermost orbit, just as the numbers corresponding to $n = 2, 3$, etc., correspond probably to electrons jumping from the second, third, etc., orbits to the inner one.

As McLennan points out, there have been hitherto two methods available for finding the ionisation potential of an element, viz. determination of the minimum arcing potential of its vapour, or using a specially designed electrical method. We now possess a third possibility, viz. by determining for the element the wave-lengths of its line spectrum and selecting those which constitute its singlet principal series $\nu = (1{\cdot}5, a) - (n, \beta)$. Calculation of a and β from the series of wave-numbers will give the convergence frequency, and the quantum hypothesis will yield the required result.

Of course, as is to be expected, this view has its own difficulties to meet, and among others, the fact that under special experimental conditions (for example, with high temperature and considerable vapour density) arcs can be struck, or, when struck, can be maintained by potentials lower than the ionisation voltages given above. The latter part of the address is devoted to these topics and the discussion shows that they are not insuperable difficulties. Under the experimental conditions necessary for the striking or maintenance of the arc with these abnormally low voltages, it is very probable that the energy of the bombarding electrons, apparently too low for ionisation, is supplemented by the photo-electrons ejected from the cathodes by the incidence of the radiations from the vapours, or by the thermionic electrons ejected from the cathodes by reason of their high temperature. The work of McLennan and his pupils is certainly providing extremely valuable data in a very interesting field of research.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Fluorescence.—In connection with this phenomenon a very interesting investigation has been carried out by J. Perrin (*Annales de Physique,* **10,** 133, 1918). Perrin deals mainly with the fluorescence of dissolved substances, such as uranine, fluoresceine, phenosafronine, chlorophyll, esculine, anthracene in different solvents. The fluorescence exhibited by these substances in thin layers is observed by means of a microscope, the lighting being so arranged that the fluorescence alone enters the eyepiece.

The most striking observation is the gradual fading of the fluorescence which takes place on exposure, the fluorescence,

once destroyed, no longer returning. This irreversibility is to be explained, according to Perrin, as a result of definite chemical reaction. The substances examined are all ring compounds, and it is probable therefore that the mechanism of fluorescence is the same for all. In the case of anthracene it is well known that exposure to light brings about polymerisation of the single molecule to the double molecular form; consequently it is probable that a similar process occurs in other cases. Perrin concludes that it is only at the moment of destruction of a fluorogene molecule, that is, at the instant at which the chemical change takes place, that fluorescent light is emitted. This is in general agreement with what we would expect on the basis of the electronic structure of atoms and molecules, such as that of Rutherford as developed by Bohr.

A further point substantiated by Perrin's experiments is the independence of fluorescence in respect of temperature change. Even at liquid air temperatures the effects appear to be the same as those observed at ordinary temperature. It is evident from this that although the fluorescence itself may be due to a true chemical process, it must be very different from ordinary or thermal chemical changes. It is well known that photochemical changes as ordinarily carried out are scarcely affected by temperature and to an even greater extent is radio-active change independent of temperature. Perrin draws attention to the analogy between fluorescence and photo-chemical change and radio-activity.

Another phenomenon associated with fluorescence is considered by Perrin, namely the existence of an optimum concentration. With extremely dilute solutions the fluorescence is necessarily weak. With very concentrated solutions the fluorescence is likewise feeble, the effect being most marked at some intermediate concentration which varies somewhat from one substance to another. To explain this Perrin introduces the idea of the *fluorescent power* possessed by a single molecule, such fluorescent power being a function of the environment or concentration, in the sense that it diminishes as the concentration increases. At great dilution the fluorescent power P reaches a finite maximum value P_0 per molecule. Further dilution has no effect on the value of P_0, but it will naturally have an effect upon the total intensity of fluorescence owing to the diminution in the number of fluorogenes or fluorescent

molecules. With increasing concentration, on the other hand, although the number of fluorogenes is greater the fluorescence diminishes also, because of the diminution in the fluorescent power, which, according to Perrin, finally approximates to zero. On this basis the existence of the optimum concentration can be accounted for.

Perrin considers the following case. Let us suppose that a layer of the fluorescent solution of concentration c possesses a thickness d cm. If I_0 is the intensity of the incident exciting light, the intensity I of the light which has penetrated to a distance x from the first exposed face may be written thus

$$I_0e^{-\alpha cx}$$

where α is the coefficient of absorption of the solution for the exciting light. The intensity of fluorescence emitted by a very thin slice of the solution at the position x is then given by :

$$I_0Pe^{-\alpha cx}cdx$$

and the intensity of this slice as observed across the face of incidence is :

$$I_0Pe^{-\alpha cx}cdxe^{-\beta cx} = I_0Pe^{-(\alpha+\beta)cx}cdx$$

where β is the coefficient of absorption of the solution for the fluorescent light. On integrating from zero to d we obtain for the total observed fluorescent intensity the expression :

$$\frac{I_0}{\alpha+\beta} \cdot P \cdot (1 - e^{-(\alpha+\beta)cd})$$

The variable portion of this expression is the product of two positive factors of which one is the fluorescent power, P, the other involving the absorption effects, $(1 - e^{-(\alpha+\beta)cd})$. The latter factor increases steadily as the concentration increases. If P steadily diminishes as concentration increases there will be a certain concentration for which the fluorescent intensity will be a maximum.

Photometric measurements made by Lepine show that beyond a certain dilution the fluorescence of a given mass remains constant—that is, the fluorescent power reaches a finite limit. This is the same as saying that beyond a certain dilution the chance of destruction of a fluorogene molecule remains constant. The chance of destruction diminishes

as the concentration increases. A rough calculation shows that the energy emitted by an active fluorogene molecule is of the order 10^{-12} erg. Photo-electric effects are of the same order of magnitude.

Supporting the view expressed above, Perrin has observed that the fluorescence of a concentrated solution exposed to light at first increases and then diminishes to zero. This is due to the continuous destruction of the fluorogenes in the chemical sense. As they are decomposed their concentration diminishes, the fluorescent power of each increasing at the same time, with the result that at a certain stage in the decomposition the fluorescence attains a maximum, finally vanishing when all the fluorogene molecules are decomposed. Perrin has not yet examined a sufficiently large quantity of material to determine what this decomposition actually is.

The next point is to explain how the fluorescent power of a molecule diminishes as the concentration increases. It is evident that contiguity of other molecules affects the fluorescing power of any individual. Perrin makes two suggestions, of which one is that the effect is to be ascribed to a kind of mutual induction or resonance between the molecules when they approach within a certain distance of one another, whereby the energy which is being absorbed in the process of activating a single molecule becomes spread over others, with the result that none of them attains the energy content required. This involves emission of energy in a degraded form, *e.g.* heat. This aspect of the subject requires further quantitative treatment.

Finally Perrin has applied his conclusions on fluorescence to the problem of the black spot exhibited by capillary films of great tenuity, and has succeeded in discovering several definite stages or ranges of thickness in such films, any one of which is a whole multiple of an elementary thickness having the approximate value 5×10^{-7} cm.

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

A contribution to the question of the surface action of enzymes is furnished in a recent paper by Bayliss (*Arch. Néerland. Physiol.* 1918, **2**, 621); starting from the assumption that the activity of enzymes is due to the absorption upon

their surface of the reacting substances, and that the reaction velocity is thereby increased owing to more intimate contact, it was argued that the activity of an enzyme should be diminished by the addition to the reacting mixture of an indifferent substance which might be preferentially absorbed. Knowing that charcoal absorbs saponin more readily than urea, some saponin was added to a mixture of urease and urea on the assumption that the enzyme might absorb some of the saponin to the exclusion of a certain amount of the urea. It was found that the activity of the urease was in fact delayed and also that bile salts and amyl alcohol had a similar effect. While the observed facts fit in with the author's hypothesis, it must be acknowledged that the surface forces controlling enzyme action are as yet not sufficiently understood to justify any very definite conclusion being drawn from these experiments.

Willstätter and Stoll, whose combined efforts have added so much to our knowledge of chlorophyll, have now (*Annalen*, 1918, **416**, 21) undertaken a thorough investigation of enzymes with a view to throwing some light on the three following questions: (1) whether enzyme action is due to the co-operation of a number of different substances or to a single chemical individual; (2) whether metals play an essential rôle in enzyme activity; and (3) what particular groups are associated with such activity. The particular enzyme selected for experiment was the peroxidase contained in horse-radish. A somewhat elaborate process of extraction was adopted, of which the following is an outline. Five kilos of finely sliced horse-radish were first washed for some days in running water, to remove soluble substances by dialysis through the cell walls; the material was then warmed for a few hours with a 0·4 per cent. solution of oxalic acid, whereby the enzyme was precipitated, presumably absorbed on the coagulated protein; the residue was next crushed and washed on a filter with 15 litres of very dilute oxalic acid; it was then ground up with baryta water to liberate the enzyme and the expressed liquid was treated with carbon dioxide to precipitate the barium. The addition of alcohol precipitated slimy substances which were removed and the filtrate was then evaporated to 50 cc.; the addition of five times this bulk of alcohol precipitated the crude enzyme; the latter was purified by

solution in water containing a trace of sulphuric acid and reprecipitation with alcohol. The resulting product was found to be a mixture of the enzyme with a nitrogenous glucoside; the latter forms an insoluble compound with mercuric chloride, and was precipitated out of solution by this means and the filtrate was once more precipitated with alcohol; after several more solutions and reprecipitations, the enzyme was obtained free from adhering glucoside.

In the best preparation the yield of enzyme obtained from 5 kilos of horse-radish was 0·45 gram, which was about 60 per cent. of the total enzyme present. From the mercuric chloride compound, about 3·4 grams of the glucoside were obtained by liberation with 2N hydrochloric acid. A new method of estimating peroxidase has been worked out which is free from the errors inherent in Bach and Chodat's method; it depends on the production of purpurogallin from pyrogallol and hydrogen peroxide in the presence of the peroxidase. As a standard of measurement the authors have fixed upon the "purpurogallin number" which represents the number of milligrams of purpurogallin which would be produced by 1 mgm. of the vacuum-dried preparation. This number is about 0·25 for well-pounded horse-radish, 360 for the crude enzyme, before purification by means of mercuric chloride, and about 670 for the purest sample of the enzyme so far obtained. The enzyme itself also appears to be a nitrogenous glucoside containing over 30 per cent. of a pentose and an equimolecular proportion of a hexose; if it contains only two sugar molecules its molecular weight would appear to be about 500 with three atoms of nitrogen; it contains 5·5 per cent. of ash, and about 0·46 per cent. of iron. The companion glucoside has a higher molecular weight, and gives Millon's and the xanthoproteic reactions; it contains about 50 per cent. of pentose and a hexose residue as well, and three atoms of nitrogen to every two molecules of pentose. It has been stated that oxyhæmoglobin does not differ from peroxidase in activity, but this error is no doubt due to the fact that peroxidase has never before been obtained in such a high degree of purity; as a matter of fact, oxyhæmoglobin is only about one-thousandth part as active as a quantity of peroxidase containing the same amount of iron.

The mechanism of alcoholic fermentation has been

studied by Neuberg and Reinfurth (*Biochem. Zeitschr.* 1918, **89**, 365). The former author has already shown that yeast contains a ferment capable of decomposing pyruvic acid into carbon dioxide and acetaldehyde and that the latter compound is readily reduced to ethyl alcohol during yeast fermentation. It is now shown that considerable quantities of acetaldehyde may be isolated from such fermentation if the latter is allowed to take place in the presence of sodium sulphite which combines to form the bisulphite additive compound as follows :

$$C_6H_{12}O_6 + Na_2SO_3 + H_2O = C_6H_{10}O_5(OH).O.SO_2Na + NaOH$$

and then

$$\tfrac{1}{2}C_6H_{10}O_5(OH).O.SO_2Na \rightarrow H_2 + CH_3C(OH).O.SO_2NaCOOH \rightarrow$$
$$\rightarrow CO_2 + CH_3CH(OH).O.SO_2Na$$

According to the authors' theory each glucose molecule should yield one molecule of acetaldehyde and one molecule of carbon dioxide, and, as a matter of fact, the yield of acetaldehyde obtained corresponded to 73·45 per cent. of the one molecular proportion required by their theory.

Some time ago Kleinstück (*Berichte*, 1918, **51**, 108) put forward the suggestion that carbonic acid was probably reduced in the plant by hydrogen peroxide to formaldehyde according to the equation

$$H_2CO_3 + H_2O_2 = HCHO + H_2O + 3O.$$

Wislicenus (*Berichte*, 1918, **51**, 942) now points out that while the reduction of carbon dioxide to formic acid, but *not* formaldehyde, has been achieved in a variety of ways in the laboratory, these ways by no means resemble those taking place in the plant. On the other hand hydrogen peroxide, which is a common substance in nature, can effect the reduction to formic acid and this is probably the first action in the plant ; the further reduction of the formic acid to formaldehyde he considers to be more difficult, requiring light-energy and chlorophyll acting as a catalyst. Attention is drawn to the fact that with the exception of the per-acids carbonic acid is the only acid whose anhydride and charged or uncharged ions have a peroxide structure, and the action of hydrogen peroxide

upon carbonic acid must be regarded as the mutual reduction of two peroxides according to the equation

$$H_2O_2 + \begin{matrix}H\\K\end{matrix}\left[\begin{matrix}O\\|\\O\end{matrix}\!\!>CO\right] = H_2O + O_2 + HCOOK$$

The best results for the above reaction are obtained by the action of a 10 per cent. solution of hydrogen peroxide upon a saturated solution of potassium bicarbonate. It has also been found that a formate is similarly produced around the anode during the electrolysis of a carbonate or bicarbonate since the peroxidic anion is then in contact with hydroxyl ions.

The constitution of carnosine, one of the chief bases contained in extract of meat, has now been finally established (Baumann and Ingvaldsen, *J. Biol. Chem.* 1918, **35**, 263) by removing the amino radicle of this substance by means of barium nitrite and sulphuric acid and hydrolysing the resulting product with 70 per cent. sulphuric acid; a theoretical yield of histidine was obtained, thus showing that carnosine is β-alanylhistidine I., and not histidyl β-alanine II.

$$\begin{matrix}NH-CH\\|\\CH=N\end{matrix}\!\!>C.CH_2.CH(NHCOCH_2CH_2NH_2)COOH$$

I.

$$\begin{matrix}NH-CH\\|\\CH=N\end{matrix}\!\!>C.CH_2CH(NH_2).CONH.CH_2CH_2COOH$$

II.

The constitution I. has been further confirmed by the synthesis from histidine and β-iodopropionyl chloride and treatment of the resulting compound with ammonia; attempts to synthesise the other compound (II.) have so far been unsuccessful.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., F.R.S.E., University, Glasgow.

Petrology.—F. F. Grout discusses the possibility of convection as a means of producing heterogeneity in igneous magmas (*Journ. Geol.* 1918, **26**, 481–99). In starting a convection current he believes that the increase in aggregate density of a portion of the magma, due to the growth of crystals, is more effective than the separation of gas or of non-consolute liquid

phases. The magmatic circulation that, with the aid of simple cooling, is thus started, is believed to explain such phenomena as the alternation of varying mineralogical bands, and the parallelism of these bands to the walls of the magmatic chamber.

In an interesting study of the origin of serpentine (*Amer. Journ. Sci.* 1918, **46**, 693–731) W. N. Benson emphasises the view that the larger antigorite and chrysotile masses are due to the alteration of pyroxenic peridotites through the agency of magmatic waters appertaining to the same cycle of igneous activity as the ultrabasic rock itself. In regard to the formation of "serpentine" from olivine in volcanic rocks by ordinary weathering, he doubts whether antigorite or chrysotile varieties are formed in this way, although the minerals iddingsite and bowlingite, often recorded as serpentine, unquestionably are.

A. L. Du Toit describes an unusual occurrence of corundum-aplite (plumasite) from Natal (*Trans. Geol. Soc. South Africa*, 1918, **21**, 53–73). These rocks are dykes intrusive into serpentine and granulitic gneiss. In contact with the latter rock the dykes consist of normal quartz-bearing granite-aplite; but where they cut the serpentine quartz disappears, the felspar becomes an acid plagioclase, and corundum appears in considerable quantity. This change is ascribed to the desilicating influence of the serpentine upon the acid magma of the dykes, an action which is confirmed by the transformation of the adjacent serpentine to phlogopite and talc, requiring the transference of large amounts of silica, alumina, potash and fluorine, from the aplite magma. Some titaniferous magnetite rocks are described in the same paper. These are due to magmatic segregation within a gabbro-pyroxenite mass on the River Tugela. Green spinel, and the ferruginous olivine hyalosiderite, are included within the iron ores. P. A. Wagner describes an occurrence of corundum in a plumasite-pegmatite from the Zoutpansberg corundum fields of the Transvaal (*ibid.* 37–42). This is a coarse rock containing andesine and biotite beside corundum, and is mineralogically similar to Lawson's plumasite from California, as well as to the rock described in the preceding paragraph. The field relations of this rock are unknown, and consequently its mode of origin cannot be determined.

In a third collection from South Georgia the author has

found a suite of rocks belonging to the spilitic series (*Geol. Mag.* 1918, 483–9). This observation tips the balance of evidence definitely against Suess' interpretation of the structure of this island as part of a great eastwardly-directed loop, homologous with the Antilles, connecting the Patagonian Andes with the mountains of Graham Land. The continued absence of andesites in collections from the island, and the presence of a spilitic suite, favours rather the interpretation that South Georgia and the South Orkneys are remnants of a continental land which once occupied the South Atlantic.

Foye, W. G., Notes on a Collection of Rocks from Honduras, Central America, *Journ. Geol.* 1918, **26**, 524–31.

Smith, H. G., The Basic Intrusions of Gelli Hill, Radnorshire, *Geol. Mag.* 1918, 500–7.

Although not geological in its aim, G. V. Wilson's paper entitled "Notes on the Formation of Certain Rock-forming Minerals in and about Glass Furnaces" (*Trans. Soc. Glass Tech.* 1918, **2**, 177–216), contains material of much value to geologists, especially in connection with the separation of minerals such as wollastonite, augite, tridymite, quartz, and cristobalite, from high temperature melts, and under conditions approximating to those of contact metamorphism.

A very notable contribution to metamorphic petrology, worthy to be ranked with recent Scandinavian work, comes from Australia (F. L. Stillwell, The Metamorphic Rocks of Adelie Land, Section 1, *Australian Antarctic Expedition*, 1911–14, *Sci. Repts.*, Ser. A, vol. iii. pt. 1, 1918, pp. 230, 35 pls.). The work concerns only the rocks collected *in situ*, and is a necessary preliminary to the study of large collections of morainic material, which are the only samples of the rocks of the greater part of Adelie Land ever likely to be obtained. At all localities save one there are found the metamorphic equivalents of both acid and basic igneous rocks; and it has been established that the latter were originally dykes intruding the granitic series and the associated sedimentary rocks.

Many points of theoretical importance are dealt with in this memoir. In some inclusions of amphibolite within granodiorite-gneiss the boundaries between the rocks have been destroyed and replaced by a gradual transition. This is believed to have been caused by a process of *metamorphic diffusion* of material across the pre-existing margins.

Inclusions of epidosite, chlorite-rock, hornblende-schist, and biotite-felspar-schist, within amphibolite, are described as due to a migration and segregation of material during metamorphism, a process which is called *metamorphic differentiation.* Rocks identical with the charnockite series of India were found, and are believed to represent a suite of igneous rocks that have suffered complete recrystallisation under the conditions of Grubenmann's kata zone of metamorphism.

W. Lindgren discusses volume changes in metamorphism (*Journ. Geol.* 1918, **26**, 542–54), and upholds the view previously advanced by him in relation to metasomatism in mineral deposits, that metamorphism by replacement does not normally involve changes of volume. The volume of replacing mineral equals the volume of mineral replaced, the substitution taking place particle by particle, not molecule by molecule. This view is based on the geological evidence that little or no change of volume has occurred in rocks that have undergone metamorphism, and on the frequent preservation of texture and structure in the altered rocks.

A. F. Rogers illustrates an occurrence of a core of periclase within brucite in a crystalline limestone from California (*Amer. Journ. Sci.* 1918, **46**, 581–6), and shows that the hydration of periclase to form brucite has probably been accomplished by ascending hydrothermal solutions. At a later stage the brucite has often been converted to hydromagnesite.

Origin of Sedimentary Rocks.—A. N. Winchell and E. R. Miller publish an account of a remarkable dust-fall that took place over Wisconsin and Michigan on March 9, 1918, illustrating the importance of wind as a geological agent (*Amer. Journ. Sci.* 1918, **46**, 599–609). The evidence presented shows that a single storm transported over a million tons of rock a thousand miles or more. From the nature of the material it is believed to have been derived from an arid region to the south-west (Arizona or New Mexico), where siliceous felspathic rocks are abundant.

The loess of Louisiana is shown by F. V. Emerson to be of æolian origin (*Journ. Geol.* 1918, **26**, 532–41). From the evidence of amount, thickness, and chemical composition of the material it is inferred that the principal depositing winds were southerly and westerly.

The bulky volume recently issued by the Carnegie Insti-

tute of Washington, entitled *Papers from the Department of Marine Biology* (vol. ix. 1918), contains much geological material relative to the origin of marine sediments, particularly limestones. A very complete study of calcareous bottom sediments, chiefly from Australian and Floridan localities, has been made by T. W. Vaughan and his collaborators (pp. 239–88), and includes the consideration of the following subjects: mechanical analyses; study of the composition of the separates of different sizes and the determination of the percentage composition of each separate according to the origin of its constituents; the chemical composition of each constituent; the chemical composition of the entire sample; the correlation of the chemical composition of the entire sample with that of its different constituents according to their percentage; the conditions under which the deposit is formed, viz. its relations to land areas, the configuration of the bottom, winds, and currents, and the depth, temperature, and salinity of the water in which formed; the areal extent, and if possible the volume of the deposit.

M. I. Goldman has made a detailed study of thirteen sediments from the Upper Cretaceous of Maryland (*Maryland Geol. Surv.* 1916, 111–84). These are all of shallow marine origin, and are classed as deltaic, estuarine or lagoonal, or of open water glauconitic type. An illuminating discussion of the occurrence and origin of glauconite is appended.

A. M. Phillips discusses the discovery of a small percentage of vanadium in the ash of a brown holothurian (*Sticopus mobii*) and in the blood of an ascidian; and the possibility of the vanadium found in certain sedimentary rocks originating from similar organic sources (*Amer. Journ. Sci.* 1918, **46**, 473–5).

For the explanation of the remarkable zig-zag suture-like lines in the Tennessee and other marbles and limestones, known as stylolites, C. H. Gordon (*Journ. Geol.* 1918, **26**, 561–8) prefers the solution theory proposed by Fuchs in 1894 and later supported by Reis and Wagner, to the differential compression theory first advocated by Quenstedt.

The huge deposits of phosphate rock in the western United States belong to horizons in the Upper Mississippian and Permian, of which the latter is by far the more extensive. They are regarded by G. R. Mansfield (*Amer. Journ. Sci.*, 1918,

46, 591–8) as original marine sediments; and their constant oolitic texture is believed to be closely connected with their origin. The waters in which they were deposited were warm, shallow, and bordered by low-lying land contributing little or no terrigenous material to the sediments. Phosphatisation was accomplished by solutions derived from decaying organic matter at a period subsequent to the formation of the oolites.

In an exhaustive memoir ("Monograph on the Constitution of Coal," *Dept. of Sci. and Indust. Research*, 1918, pp. 58) M. C. Stopes and R. V. Wheeler conclude that coal is a mass of morphologically organised plant tissues, with other unorganised plant substances such as resin, mingled with the comminuted degradation products of tissues and cells, or such products in the form of members of the ulmin group. Not only are resistant tissues preserved, but also much more delicate structures, provided that they are immersed at an early stage in the aseptic matter arising from a special kind of breakdown of plant tissues.

Economic Geology.—The new Geological Survey volume of Special Reports on the Mineral Resources of Great Britain (vol. vii. *Mem. Geol. Surv.* 1918, pp. 67) deals with lignites, jets, the Kimmeridge oil shale, mineral oil, cannel coals, and natural gas. In view of the rumours, "more or less ill-founded, as to the existence of copious untapped sources" (see Preface) of mineral oil in Britain, the warning paragraph on p. 41 dealing with the occurrence of mineral oil in Derbyshire should be read in connection with V. C. Illing's convincing article on "Borings for Oil in the United Kingdom" (*Nature*, Jan. 16, 1919, 385–8), in which is demonstrated the great geological improbability of oil being struck in commercial quantity in the borings now being sunk or projected in various parts of the country. An anonymous article in the *Glasgow Herald* (Shipbuilding, Engineering, and Commerce in 1918, Dec. 28, 1918) on Oil Fields, British Resources, takes practically the same view.

Wagner, P. A., Mineral Oil, Solid Bitumens, Natural Gas, and Oil Shale, *South African Journal of Industries*, Industrial Bull. Ser. No. 3, Oct. 1917 (1918), pp. 29.

Clements, J. M., Petroleum in Japan, *Econ. Geol.* 1918, 13, 512–23.

G. S. Rogers discusses the origin of the great salt domes

that occur beneath the coastal plain of Louisiana and Texas. He shows that they are arranged along lines related to the main structural features of the region, and that they are accompanied by very sharp and local upthrust and doming of the normally flat-lying sediments. The salt deposits themselves show intricate folds resembling the flow structures of ancient rocks. Rogers believes that the plugs are offshoots of deeply-buried stratified deposits which have been subjected to great pressure or thrust, and have been squeezed upwards along lines of weakness in a semi-plastic condition. The evidence for their tectonic origin is at least as strong as that for the European salt stocks (*Econ. Geol.* 1918, **13**, 447–85).

O. P. Jenkins discusses the origin of two small lakes of epsomite in Washington and British Columbia which present a peculiar spotted appearance owing to numerous shallow pools of brine upon the white epsomite. The origin of the mineral is ascribed to the action of sulphuric acid, derived from the weathering of pyritic and pyrrhotitic ore deposits, upon the magnesian igneous rocks of the locality (*Amer. Journ. Sci.* 1918, **46**, 638–44).

Whitaker, W., and Thresh, J. C., The Water Supply of Essex from Underground Sources, *Mem. Geol. Surv.* 1916, pp. 510.

Hall, A. L., On the Mode of Occurrence and Distribution of Asbestos in the Transvaal, *Trans. Geol. Soc. South Africa,* 1918, **21**, 1–36.

Wagner, P. A., Reports on Certain Minerals used in the Arts and Industries. I.—Asbestos, *South African Journal of Industries,* Industrial Bull. Ser. No. 6, Nov. 1917 (1918), pp. 22. II.—Magnesite, *ibid.*, Bull. No. 12, March 1918, pp. 11.

Stratigraphical and Regional Geology.—A stratigraphical point of some importance is discussed by Prof. E. C. Case in a paper on Permo-Carboniferous Conditions *versus* Permo-Carboniferous Time (*Journ. Geol.* 1918, **26**, 500–6). It was prompted by the observation that " Permo-Carboniferous Conditions " (red beds with reptilian and amphibian fauna) appeared much lower in the stratigraphic sequence in the east of the United States than in the west. This is explained as due to the fact that an environment favourable to the development of a certain fauna begins in a limited area and spreads slowly, thereby involving different levels of one or

more geological periods. A fauna or flora may therefore advantageously be correlated as belonging within the limits of a certain environment independent of stratigraphic levels.

The Gatooma country (S. Rhodesia), as described by A. E. V. Zealley and B. Lightfoot, consists of quartzites, banded ironstones, and greenstones, into which large batholiths of granite and numerous masses of quartz-porphyry have been intruded (*Geol. Surv., S. Rhodesia,* Bull. No. 5, 1918, pp. 68). Gold is the chief mineral product, and is found in replacement deposits of gold-bearing sulphides disseminated in fissure-zones, and in fissure veins of gold-bearing quartz. Antimonite and scheelite are also found. In a further paper Zealley demonstrates that the rocks in the Gatooma area, hitherto known as "banded ironstones," are really ferruginised and silicified felsites and quartz-porphyries (*Trans. Geol. Soc. South Africa,* 1918, **21**, 43–52).

Dixey, F., and Sibly, T. F., The Carboniferous Limestone Series on the south-eastern margin of the South Wales Coalfield, *Quart. Journ. Geol. Soc.* 1918, **73**, pt. 2, 111–64.

Trechmann, C. T., The Trias of New Zealand, *ibid.,* 1918, **73**, pt. 3, 165–246.

Marshall, P., Geology of the Central Kaipara (New Zealand), *Trans. New Zealand Inst.* 1917, **49**, 433–50.

Emerson, B. K., The Geology of Massachusetts and Rhode Island, *U. S. Geol. Surv.* Bull. 597, 1917.

Hawkins, A. C., Notes on the Geology of Rhode Island, *Amer. Journ. Sci.* 1918, **46**, 437–72.

Powers, S., Notes on the Geology of Eastern Guatemala and North-western Spanish Honduras, *Journ. Geol.* 1918, **26**, 507–23.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., East London College, University, London.

Anatomy.—The aerenchyma of *Epilobium hirsutum* has been studied by Miss L. Batten (*Journ. Ecology,* Nov.), who finds that it is developed from the phellogen in May and that its development depends on external conditions. The aerenchyma arises within the endodermis and may consist of a loose tissue of irregularly arranged rounded or radially elongated cells, or of concentric zones separated by air spaces and bridged by

radial rows of cells. Its production only appears to take place in parts which have been submerged in mud or water, the former producing the most marked effect.—Miss Collins (*Proc. Linn. Soc. N.S. Wales*) describes the anatomy of the leaves of *Scævola crassifolia*, a sand-dune plant belonging to the Goodeniaceæ. The leaves assume a vertical position, and are large and fleshy, the palisade and central regions respectively containing occasional branched mucilage cells and water-storing tracheids. When young the leaves bear numerous capitate glands whose resinous secretion serves for the protection of the juvenile foliage within the bud. This secretion ceases, however, as the leaves approach maturity.

Taxonomy.—Dr. Wernham (*Journ. Botany*, Nov.) describes new species of Rubiaceæ belonging to the genera *Mussenda Sabicea, Stipularia, Tricalysia, Vangueria, Cuviera, Ixora, Rutidea, Globulostylis*, and *Amaralia*. In the same journal Mr. Grove describes new species of fungi belonging to the genera *Ceuthospora, Ascochyta, Diplodina, Melasmia*, and *Glæosporina*. Further species of Uredinaceous fungi are described by J. C. Arthur (*Amer. Journ. Bot.*) for the following genera: *Ravenelia* (5 spp.), *Uropyxis, Skierka, Pucciniosira, Uromyces* (3 spp.), and *Puccinia* (12 spp.). A new genus of Cœlastraceæ, *Eutetramonas*, is described by L. B. Walton (*Ohio Journ. Science*, Feb. 18).

Morphology.—The Phyllode theory of de Candolle, which conceived the leaf of the Monocotyledons as the equivalent of the leaf-base and petiole of the Dicotyledons, has been revived and elaborated in an extremely interesting paper by Mrs. Arber (*Annals of Botany*, Oct. 1918). The author points out that this theory affords an explanation of the parallel venation of the former group which is also met with in the phyllodic structures amongst Dicotyledons. This interpretation is applied also to the monocotyledonous cotyledon, and support is given to the hypothesis of Henslow that the leaf-blades of *Sagittaria, Arum*, etc., are "pseudo-laminæ" developed from the phyllode apex. The chief contribution of the paper under review to this vexed question is a consideration of the anatomical evidence. It is pointed out that some petioles are characterised by the possession of inversely orientated bundles, which feature is also met with in some, though not all, dicotyledonous phyllodes. Inverted bundles are only known to

occur in the midrib and principal veins of some dicotyledonous leaf-blades, so that their presence outside the midrib in the "laminæ" of Monocotyledons may be regarded as indicative of a phyllodic origin. Sixty-two genera belonging to thirteen families are recorded from the Monocotyledons in which such inverted bundles have been recognised. It may be noted that these include the "pseudo-laminæ" of *Sagittaria, Hydrocharis, Eichornia, Heteranthera,* and *Pontederia.* The feature is most frequent in the Helobiæ, Liliifloræ, and Farinosæ. Some years ago the reviewer showed that there is good reason for regarding the amount of vascular tissue in the petiole as definitely related to the transpiring activity, whilst the arrangement of the vascular strands appears to depend largely on mechanical considerations. Where there is little or no cambial activity, the strands are more numerous, which, owing to special relations, may result in the presence of inverted bundles. Before, therefore, one can accept the anatomical structure as indicating a phyllodic origin, it will be necessary to establish that the former is not the outcome of physiological needs.

Genetics.—A. St. Clair Caporn contributes three papers to the *Journal of Genetics* (Aug. 1918), of which the first two deal respectively with the inheritance of "tight" and "loose" paleæ and early and late ripening in oats. With regard to the former, *Avena nuda* was crossed with three varieties of the common oat all characterised by tight paleæ in contrast to the loose paleæ of *A. nuda.* In the latter, however, "tight" grains may be as numerous as 40 per cent., pure "tight" forms never having been obtained. In extracted pure "tights" the number of grains in the spikelet never exceeded four, whilst in *A. nuda* their number varied from six to ten. In the second paper the author agrees with the results of Hoshino (*Imp. Univ. Sopposo Japan,* 1915) on peas and rice in regarding early and late ripening as Mendelian characters, probably, however, dependent on three factors. In F3 a comparatively early type is segregated. The third paper deals with crossing of *Triticum polonicum* (av. glume length 29·23 mm.) and *T. eloboni* (av. glume length 10·58 mm.). In the F1 generation the glumes were of intermediate lengths. In F2 the variation curve for glume lengths shows three periods representing the overlapping curves for homozygous long, homozygous short, and heterozygous intermediate forms. An interesting feature

is that the extracted long-glumed type exhibited a smaller range and diminished average length, as compared with the parent, *T. polonicum.*

Miss I. Sutton contributes to the same journal a report on tests of self-sterility in plums, cherries, and apples. Lists are furnished of sixteen self-sterile plums, five partly self-fertile, and eighteen self-fertile varieties. The majority of the cherries proved self-sterile (17 vars.), three were self-fertile, and two partially so. Of the apples experimented upon, viz. thirty-four, eight proved self-sterile and sixteen self-fertile. The results obtained are not inconsistent with the view that self-sterility is a recessive and the partially self-sterile individuals may be heterozygous. Except for the "Coe" varieties of plum and "Jefferson," which appear to be sterile with other pollen besides their own, the pollen of any other variety would appear to serve in the case of self-sterile individuals.

Ecology.—In the June number of *Soil Science,* M. I. Wolkoff contributes a paper of fifty-nine pages dealing with the influence of ammonium sulphate on the germination and growth of barley. It is impossible to summarise all the results obtained, many of which are of agricultural rather than ecological interest. The following conclusions are of general significance. The osmotic concentration of the soil solution subsequent to the addition of a fertiliser becomes important when there is a considerable reduction of the water content below the optimum. The rigidity of straw is regarded as dependent upon a proper balance in the nutrient solution. With increase of yield there is a decrease in the amount of water utilised in the production of each grain of dry weight. In the same journal for July R. E. Stephenson discusses methods of determination of soil acidity in which, however, that devised by Hutchinson and MacLennan is ignored.

Determinations of the osmotic concentration of tissue fluids in various epiphytes have been made by J. A. Harris (*Amer. Journ. Bot.* Nov. 1918). The results (3·3–5·6 atmospheres) show a far lower osmotic concentration than in terrestrial vegetation. Estimations from the Jamaican montane rain forest gave concentrations which were only from 37–60 per cent. that of the ordinary herbaceous flora and from 28–45 per cent. that of the ligneous vegetation.

Prof. Osborne (*Trans. Roy. Soc. S. Australia,* 1918), in re-

cording the occurrence of *Isoetes drummondii* and *Phylloglossum drummondii* from South Australia, furnishes an interesting account of the plant association in which they occur in the National Park, Belair. The habitat is an alluvial flat which is dry in summer and water-logged in winter. A list of sixty-seven phanerogams is given, comprising three trees, two shrubs, seven under-shrubs, nineteen annuals, and thirty-six herbaceous perennials. Of these last no fewer than twenty-five are geophytes with subterranean storage organs, whilst fifteen of the nineteen annuals are ephemeral.

J. Bär has recently described and mapped the vegetation of the Val Onsernone (Zurich, 1918). This region is one of high rainfall and the soils are almost exclusively siliceous. The major part of the area described, to an altitude of over 2,000 m., is occupied by forest and scrub. On the northern exposures the zonation is Chestnut (250–900 m.), Beech (800–1,100 m.), Silver Fir (1,150–1,300 m.), Spruce (1,200–1,700 m.), and Larch (1,600–1,900 m.). On the southern exposures the Chestnut attains a higher altitude (1,000 m.), and so does the Beech (1,700 m.), thus extending throughout the altitudinal range of the woods of Silver Fir and Spruce, though the latter locally forms woods from 1,400–1,750 m. The Beech is followed by the Larch up to 2,100 m. Birch woods are local, especially on northern slopes, from 600–1,200 m., where the soil is poor but well supplied with humus. Lime (*Tilia cordata*), too, forms local woods, in the Chestnut and Beech zones, on precipitous rocky slopes and, locally also, Oak woods of *Quercus sessiliflora* and *Quercus pubescens* are found up to 1,400 m. Hazel scrub due to grazing of goats is a conspicuous feature near inhabited areas up to 1,600 m. Various types of Heath are described, dominated respectively by *Sarothamnus scoparius*, *Erica carnea*, *Calluna vulgaris*, *Salix herbacea*, *Loiseleuria procumbens*, and *Vaccinium* spp. The dominant species of the meadow types include *Bromus erectus*, *Brachypodium pinnatum*, *Festuca ovina* s.spp., *Nardus stricta*, *Agrostis tenuis*, *Calamagrostis arundinacea*, *Trisetum flavescens*, *Cynosurus cristatus*, and *Poa alpina*.

Economic.—The improvement of Hill pasture is dealt with by Dr. W. G. Smith in the *Scottish Journal of Agriculture* (July 1918). Under this heading is included pasture land that is never ploughed and is moreover unenclosed. Such areas tend

to deteriorate both on account of the continuous cropping and the leaching of the surface. The best type of Hill pasture (*Agrostis, Festuca, Cynosurus*) is associated with flushes, where the water contains lime in solution, and where moreover movement of the surface water ensures good aeration and, in times of spate, brings about a dressing of silt. By open drains the flush grassland can be extended provided the stream water is "hard," but the "soft" water from peat has a reverse effect. *Pteris aquilina* is commonly associated with flush grassland, but is kept in check by the trampling of cattle, and can be suppressed by repeated cutting or spraying. Heather grown for fodder should be burned at least every fifteen years. Rough pasture of *Nardus stricta, Molinia cœrulea*, and *Juncus squarrosus* has a low feeding value, but can be improved by thorough grazing and repeated burning at intervals of two to three years. The author points out that improvement depends almost entirely on an increase in available labour.

PLANT PHYSIOLOGY. By R. C. Knight, D.Sc., Imperial College of Science and Technology, London. (Plant Physiology Committee.)

Transpiration.—The diversity of the direction of modern studies of transpiration is an excellent indication of the realisation, which has been forced upon researchers, that the problem of the movement of water through a plant is a rather more complex question than it was formerly considered to be. Much progress has been made from the stage when transpiration, admittedly of vital import in the life process of the plant, was considered to be adequately explained as a process resulting from root-absorption and evaporation from the leaves "at the discretion of the stomata." The conditions affecting the transpiration stream have been studied at every stage of its progress, and factor after factor has been successively added to the list until the stomata themselves cut but a poor figure in the array of regulating influences. In fact, in the opinions of some workers stomatal action is normally negligible compared with the controlling activities of other factors. It is recognised that external conditions, as represented by the soil and the atmosphere, as well as the internal conditions peculiar to the plant, exercise an important regulating influence.

The question of the water supply from the soil has been

investigated, and Yuncker (*Plant World*, **19**, 151–61, 1916) has determined its influence on transpiration in the corn plant. He found that the "water requirement" (the quantity of water transpired per unit production of dry matter) of his plants was greater in a moist soil than in a dry one. Iljin (*Journ. Ecol.* **4**, 65–82, 1916) working on slightly different lines has obtained exactly similar results. Experimenting on rather dry ground in the open, he found that a drought-resisting capacity in a plant is accompanied by a correspondingly low transpiration rate, relative to the amount of assimilation taking place. Bowman (*Proc. Nat. Acad. Sci. U.S.A.* **2**, 585–8, 1916), working with mangrove seedlings, found a greater transpiration rate resulting from an increased concentration of the sea water with which he watered his (sand) cultures. These results appear to resolve themselves into a question of the reduction of concentration of nutrient substances, in the one case by addition of water, and in the other of salt, but further speculation is liable to prove unprofitable. The question arises from Iljin's results, whether the capacity of some types to employ the water supply to greater advantage than others might be applied to the cropping of semi-arid regions.

Livingston (*Johns Hopkins Univ. Circular*, 1917, 176–82) finds from water culture experiments that a plant may go through all stages of water loss from incipient drying to temporary, and finally, permanent wilting without any reduction in the power of the surrounding medium to supply water to the roots. He therefore concludes that reduction of water content may occur as the result of purely internal factors.

The comprehensive work of Dixon and Joly has caused some attention to be directed to a physiological study of the wood of stems, with a view to obtaining knowledge of the passage of water. Bailey (*Bot. Gaz.* **62**, 133–42, 1916) finds the bordered-pit closing membrane of conifers to be perforated and readily permeable to gases and liquids. Nordhausen (*Jahrb. f. wiss. Bot.* **58**, 295–335, 1917) finds the problem of the ascent of sap no nearer solution owing to various difficulties, including interference with the continuity of the water columns by air bubbles, and the impossibility even at great tensions of drawing through a stem sufficient water to cover the losses by transpiration. Work in this direction appears continually to marshal more facts than can be explained by the simple cohesion and

mechanical theory of ascent of sap, and Nordhausen is driven back to the "living cell" explanation of water conduction, inadequate and unsatisfactory as this position is.

Farmer (*Proc. Roy. Soc.* **90**, B, 218–31, 232–50, 1918) has measured the conducting properties of the wood of a variety of stems, finding that evergreens exhibit a relatively low conductivity compared with that of deciduous species, whilst the latter show more readiness to react to environment. The suggestion is made that wood structure rather than habitat is correlated with xerophytism.

Confirmation is being obtained from many sources of the presence of a regulating factor in plants which retards transpiration towards noon and decreases the transpiring power whilst atmospheric conditions are still tending to increase the rate of water loss. Livingston (*Carn. Inst. Publ.* No. **50**, 1906, and *Bot. Gaz.* **53**, 309, 1912) first established the existence of the phenomenon, and attributed it to the lowering of leaf water content. The results have been confirmed by several workers using various methods, including the hygrometric paper test, which has now been thoroughly standardised and tested and may be regarded as satisfactory. See Shreve (*Carn. Inst. Publ.* No. **194**, 1914), Trelease and Livingston (*Journ. Ecol.* **4**, 1, 1916), Bakke and Livingston (*Physiological Researches*, **12**, 1916) and Knight (*Ann. of Bot.* **31**, 221–40, 1917).

Progress has been made in the study of atmometry, first adapted to transpiration work by Livingston (*Plant World*, **18**, 21, 51, 95 and 143, 1915). Recognition of the complexity of the climatic factors influencing water loss by the plant has led to considerable development of the methods of control. Johnston and Livingston (*Plant World*, **19**, 136–40, 1916) have used a convenient atmometer adaption which permits the frequent measurement of evaporation rate over periods of a few seconds. Blackman and Knight (*Ann. of Bot.* **31**, 217, 1917) have devised an "air flue" for the control of air movement in transpiration experiments, and this has also been used with advantage.

Other aspects of the climatic factor have been presented in various papers. Thomas and Ferguson (*Ann. of Bot.* **31**, 241–55, 1917) discuss the calibration of atmometers by comparison with water surfaces, and show by experiment that the method is not justifiable except under very particular conditions.

Evaporation from a water surface is shown to be proportional to (radius)$^{1.5}$ and the possible error in previous calibrations is considered to be as high as 40 per cent. The same authors have shown (*Phil. Mag.* **34**, 308–21, 1917) that the power 1·5 (above) is unaffected by changing external conditions, but approaches 2 as the water level sinks below the rim of the containing vessel.

Briggs and Shantz (*Journ. Agr. Res.* **9**, 277–92, 1917) have developed a point suggested by their earlier work (*Journ. Agr. Res.* **5**, 583, 1916) that the response of atmometers and plant to climatic changes is not necessarily similar. Various types of atmometer were used and differences were found between these types as well as between atmometer and plant. Wind velocity and solar radiation were the most disturbing factors. Knight (*Ann. of Bot.* **31**, 351, 1917), also using various types of atmometer, demonstrates the difference in response of plant and atmometers to changes of wind velocity. Briggs and Shantz find that a shallow tank corresponds most nearly to the plant in its response to changing climatic conditions, and Knight considers that the more convenient atmometers may be used if wind velocity is controlled. These results have cast some doubt upon the value of the results of many earlier transpiration experiments in which atmometers were used without controls of any kind, and close examination of the earlier papers reveals figures and statements clearly demonstrating the unreliability of simple atmometer readings as corrections for climatic changes.

Briggs and Shantz (*Journ. Agr. Res.* **7**, 155–212, 1916) have continued their extensive large-scale open-air trials on transpiration and crop yield. Various crops have been dealt with and the extent of correlation of the climatic factors with transpiration rate has been calculated. As before, it was found that some plants produce far more growth than others per unit quantity of water transpired.

The influence of stomatal change on transpiration, though attracting less attention than formerly, has been the subject of some work. Stalfelt (*Sven. Bot. Tid.* **10**, 37–45, 1916) finds no simple relationship between stomatal aperture and transpiration, nor between the turgor of the guard cells and the aperture. He prefers to allot shares in the control of stomatal movement to a variety of factors, including water supply,

assimilation and translocation. Hagen (*Beitr. z. allg. Bot.* **1**, 261–91, 1916) more definitely asserts that stomatal opening results from sugar formation from starch in the guard cells. Boysen Jensen (*Bot. Tid.* **36**, 144–55, 1917) concludes that the opening of stomata in some plants may cause an increase in the transpiration rate which would otherwise be lower than normal as the result of xeromorphic modifications.

The purely physical side of the question of the stomatal control of transpiration has been discussed and developed by Jeffreys (*Phil. Mag.* **35**, 270–80, 431–4, 1918) and Larmor (*Phil. Mag.* **35**, 350–52, 1918). The question of the influence of the activities of a stoma upon those of its neighbours is approached, and whilst Larmor prefers to agree with Brown and Escombe that interference is normally small, Jeffreys maintains that with a normal size and distribution of stomata on a leaf, the mutual effect is profound. He concludes that only a small portion of the vapour pressure over a stoma is maintained by that stoma, and that consequently the rate of transpiration is not decreased by stomatal closure until a very small aperture ($\frac{1}{50}$ normal opening) is reached. This point is said to reconcile the differences of the Darwin-Lloyd controversy, but while it certainly may clear one statement of that discussion, it does not materially affect the two main contentions. These two papers should provide a fruitful source of experiment on evaporation and transpiration.

Bakke (*Bot. Gaz.* **66**, 81–116, 1918) has continued his experiments on wilting plants and has confirmed his previous result, in finding that permanent wilting occurs at a definite point, namely when the water columns in the plant are broken. Moreover, this point can be readily determined by the sudden rise in transpiring power as indicated by the hygrometric paper method. During wilting a period is reached when the ratio of day-transpiration to night-transpiration is unity, and the duration of this period is probably an indication of the power of the plant to resist water loss. Bakke is of the opinion that the increase of transpiration following permanent wilting is not due to the temporary stomatal opening reported by other workers, and, indeed, it should be remarked that this opening probably occurs long before the permanent wilting stage is reached.

Martin (*Journn. Agr. Res.* **7**, 529–48, 1916) and Shive and

Martin (*Plant World*, **20**, 67–86, 1917) report a marked increase in transpiration rate following spraying with Bordeaux Mixture. No attempt is made to explain the phenomenon, but the important bearing of the point should lead to further investigation.

ANTHROPOLOGY. By A. G. Thacker, A.R.C.Sc.

Of recent anthropological papers the most important are once more concentrated in the *Journal of the Royal Anthropological Institute*, vol. xlviii. pt. 1, January to June 1918. The first article in this number is the Presidential Address of the Anthropological Institute by Sir C. Hercules Read. The address is more brief than those usually given by the Presidents and is entitled "Primitive Art and its Modern Developments." The author is himself not only a scientist, but an artist, and is therefore singularly competent to write upon this subject. He naturally refers once again to the marvellous art of the later Cave-men. It is interesting to have his opinions on these. He says:

"As to the artistic perfection of his (the Cave-man's) drawings of the fauna of the time (and in my opinion perfection is a term that is but little in excess of the truth), I fear that words can be of but little avail. A long and sympathetic study has led me to believe that it is hard to use any but superlative terms in referring to them. But, however sympathetic we moderns may be towards Cave-art, the word is weak and inadequate when one tries to realise the relation of Cave-man to his animal models. This relation was refined and intensified by a superhuman understanding of every attitude and every detail of the beast to be represented, and such was his confidence as an artist that he often performed marvels in the subtle indication of characteristic features."

His study of these drawings and paintings appears to have led Sir Hercules Read to the opinion that such perfect representations of animals would have been impossible if the art of their domestication had not been understood. This is of course an unorthodox opinion to hold, but coming from so great an authority it will naturally arrest attention immediately.

The author then proceeds to advance a theory to explain the extraordinary forms sometimes assumed by art in our own day. He describes the tendency which white men in

savage countries sometimes display to revert to savagery themselves. And he thinks that the wild, bizarre forms of art are psychologically analogous to this weird tendency to revert to savagery. And he makes this suggestion notwithstanding the fact that the art produced by savages themselves is of an entirely different and indeed much higher character.

The last point brought forward in this interesting paper is that the artistic efforts of persons suffering from mental disorders display in an exaggerated degree " the same desire to escape from the shackles of civilised complexity and revert to simpler conditions." The Presidential Address is certainly the most original contribution to the study of primitive art which has been published for some time.

The remaining articles in this number of the Journal, though few in number, are of unusually great interest. Prof. Giuffrida-Ruggeri contributes an article entitled " A Sketch of the Anthropology of Italy." He discusses in small space, but in very considerable detail, the distribution, according to provinces, of the different Italian types; and the criteria of these types which he takes relate to height, colouring, character of hair, cephalic index, so-called nasal index, cranial capacity, and weight of the brain. In regard to colouring, it may be mentioned here that in Italy generally the pure brown type (that is, black hair and black or dark brown eyes) amounts to just over 25 per cent. of the population. The pure blond type is represented by only 3 per cent. The remainder of the population is made up of what he calls the mixed brown and the mixed blond types, with the former much predominating.

The last paper in this number is also unusually suggestive. This is entitled " Anthropology and our Older Histories," and is by H. J. Fleure and Miss L. Winstanley. The authors put forward the hypothesis that according to recent anthropological and archæological evidence the ancient histories and legends would seem to contain a greater element of fact than has been generally supposed during the last few decades. Among other matters they instance the undoubted connection between Spanish Galicia and the Celtic fringe of Europe, and they even think that the ancient stories about Stonehenge may not be altogether devoid of truth.

There is also a well-written article by Carveth Read entitled

" No Paternity," combating the theory recently put forward by Spencer and Gillen and also by Malinowski that various savage tribes (*e.g.* some tribes of Central Australia and some Melanesians) do not understand the phenomena of paternity. This paper will repay study, and Read, after arguing the subject at length, ends with the following sentence: " I have assumed throughout that (1) the natives and (2) the observers are able to discriminate between what people believe and what they are accustomed to say. In Europe this is impossible." In addition to the contributions noticed above there is a fourth and concluding article by H. Ling Roth on " Studies in Primitive Looms," and Captain F. R. Barton has a paper entitled " Tattooing in South-Eastern New Guinea."

It is some years since the *Journal of the Royal Anthropological Institute* has contained so many extraordinarily interesting papers.

In the recent numbers of *Man*, the articles are as usual extremely brief. In the number published in August 1918, Captain E. G. Fenton publishes a rejoinder to Prof. Boyd Dawkins' criticism of his former paper on the so-called Maltese cart-ruts (see SCIENCE PROGRESS, October 1918, p. 239). Captain Fenton contends once more that these really are cart-ruts, and he urges the consideration that though they sometimes run straight they " much more often curve in every direction possible." The September number of the same magazine includes, among other contributions, an article by Sir C. Hercules Read, " On a Carved Ivory Object from Benin in the British Museum," and one on Central American Chronology by R. C. E. Long entitled " The Maya and Christian Eras." In the November number these supposed cart-ruts are again discussed by Commander H. N. M. Hardy, who believes that they are artificial and says that they are always found in conjunction with Stone Age buildings.

ARTICLES

RECENT WORK ON THE SPECTRA OF X-RAYS

By PROF. W. H. BRAGG, C.B.E., D.Sc., F.R.S.,
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In the years immediately preceding the war the study of X-ray spectra had just been begun. A new line of research had been opened up, and it was seen that it must lead to results of very great importance. The war necessarily interfered with the progress of the work, arresting it almost completely in the countries that were most deeply involved. But in some of the countries that were neutral, and in America until the energies of the researchers were diverted into the work of the war, considerable progress was made.

It is proposed to give here a sketch of the present position as it is now constituted after recent advances.

When we approach once more the subjects from which our attention has been diverted for the past few years, we are all in the position of learners again and it may be convenient to restate the first principles of the subject.

Every natural element may be made to emit X-rays characteristic of the element, the rays constituting a bright line spectrum in which the lines have much shorter wavelengths than in visible light. They range from two or three Ångstrom units (10^{-8} cm.) to a fraction of a unit, whereas in ordinary light the lengths are many thousands of times greater. In his classical survey of the X-ray spectra Moseley showed that their general characteristics varied very little, if at all, from element to element; only in the frequency of any representative line was there a steady march from one element to the next in the natural order of the periodic table. See, for example, fig. 3 below, p. 576, which shows the K series in the spectra of silver, palladium, and rhodium. If the frequency of a

representative line emitted by an element, or any function of the frequency, is plotted against the atomic number of the element, that is to say, a number which represents the position of the element in the periodic table of the chemical elements, a smooth curve can be drawn through the indicating points. Such a thing cannot be done if the atomic weights are used instead of atomic numbers, and this alone shows that we are now dealing with some very fundamental characteristic of the atom.

If it is the square root of the frequency which is plotted against the atomic number, the smooth curve referred to becomes very nearly a straight line. Whether it does so exactly is a matter of exact measurement and correct interpretation. If the curve is smooth, that in itself is one fact of first-rate importance; it will be an additional conclusion of importance if a function of the frequency can be found, such as the square root of the frequency, for which the curve becomes exactly a straight line.

The X-ray spectrum of each element contains a K series consisting of four lines and an L series (which probably ought to be divided into L_1, L_2, and other series) of more numerous lines and longer wave-lengths. There are doubtless other series as well, but the K and L lines are the most commonly observed and measured.

The stimulus which must be applied to cause an element to emit its lines may consist of a projectile stream of electrons of sufficient speed, or a beam of X-rays of sufficiently high frequency. The limiting velocity in the one case and the limiting frequency in the other have been the subject of recent careful measurement and will be referred to later.

The spectrometer which is used in analysing X-rays resembles the ordinary spectrometer in general form, but differs materially in details. No lenses or prisms can be used, since they cannot refract or reflect the X-rays; the collimater is replaced by two narrow slits and the telescope by an ionisation chamber, or by a photographic plate. The ordinary diffraction grating, with its lines, say, 20,000 Å.U. apart, is replaced by a crystal whose atoms are arranged in planes which are regularly spaced at intervals of two or three Å.U. The minute spacing is appropriate to the shorter wave-lengths to be dealt with.

Very naturally much of the recent work has been devoted to the detailed improvement of the spectrometer with the object of attaining more accurate measurements. A series of excellent papers on the subject has appeared during the last year or two in the American *Physical Review.*

Lack of sharpness in the image, or, more generally, of "resolving power," is due mainly to the following causes:

(1) The slits must be wide enough to pass sufficient X-ray energy, or else the apparatus is too insensitive.
(2) The diffraction effect which gives rise to the reflected beam of X-rays is not a simple reflection occurring at the crystal surface, but a volume effect in which the whole of the crystal takes part, so far at least as the X-rays penetrate.

So far as has been observed no lack of sharpness is due to an insufficiency in the number of waves in the X-ray train, an effect which might have been supposed to be a possibility. Also, crystals can easily be found whose accuracy of construction is so good that no effects due to the lack of it can be discovered.

As regards the first of the difficulties stated above, it is clear from fig. 1 that the width of the image must be at least as great as the width of any slit which limits the width of the beam of X-rays. Such a slit is usually set at a width of from two- to three-tenths of a millimetre.

As regards the second difficulty, the depth to which the X-rays penetrate in the crystal adds further to the width of the image; and this effect can only be diminished by using a very thin crystal. Taking these two points together, the definition of the image can only be improved by making the slit narrow and the crystal thin. Every step taken in this direction makes the image weaker and renders it advisable to employ as strong a source of X-rays as possible.

The limits which are set in this way make it difficult to separate lines which are close together; the resolving power of the instrument is small. In a case given by Dershem (*Physical Review*, June 1918), it was found that the resolving power was considerably less than 200, that is to say, it was not possible to separate two lines whose relative frequencies

differed by ½ per cent. But the accuracy with which the wave-lengths of separate lines can be compared is probably very

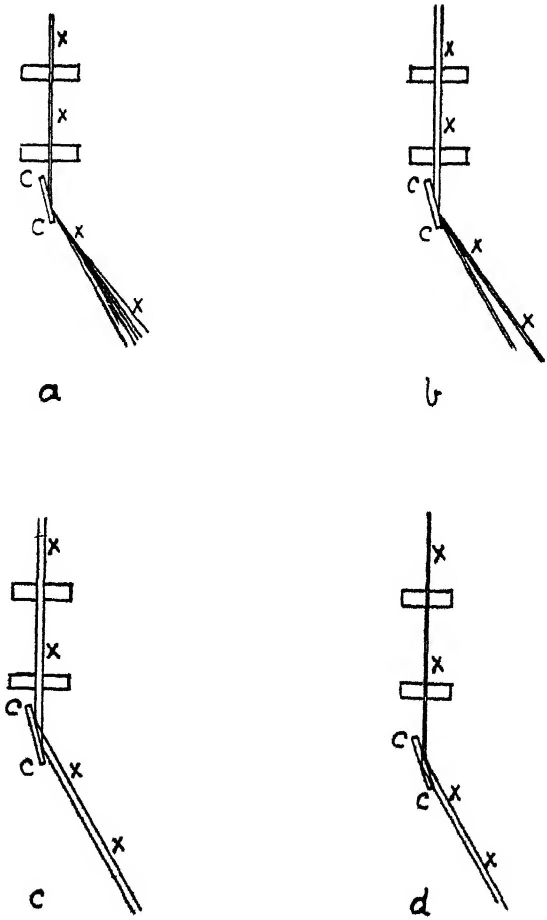


Fig. 1.

(*a*) Shows what would happen if there were an insufficient number of reflecting planes in the crystal or an insufficient number of waves in the X-ray train. A fine pencil of X-rays limited by slits in blocks of lead would, after striking the crystal. be opened out into a diffused and spreading pencil.

(*b*) Shows what would happen if the crystal was badly built: for example, if there were two portions of crystal, not quite in alignment with each other where the X-rays fell upon them, the original ray might be broken into two.

(*c*) Shows how, even if everything was perfect and the X-rays were reflected very close to the surface of the crystal the reflected beam must be as w de as the original beam.

(*d*) Shows how the reflected beam must be wider than the incident beam on account of the fact that the reflection is from the body of the crystal and not only from the surface.

much higher than such a figure would seem to imply, and this is true even of the absolute measurements themselves. For

many purposes accurate determinations of the relative values of wave-lengths are all that is wanted.

As an example of closeness of repetition and comparison the following may be quoted from the paper by Dershem already referred to. It shows the results of four separate measurements of the K series of tungsten (atomic weight 184, atomic number 74).

TABLE I

(From *Physical Review*, p 473, vol. xi. No. 6)

K SERIES OF TUNGSTEN

	(1)	(2)	(3)	(4)
α_2 . .	0·2121	0·2126	0·2118	0·2126
α_1 . .	0·2075	0·2075	0·2069	0·2078
β . .	0·1833	0 1818	0·1831	0·1837
γ . .	0·1784	0·1786	0·1778	0·1785

These may be compared with the K series shown in fig. 3.

In this case the width of the slits which limited the incident pencil of X-rays was 0·032 cm., and the thickness of the rock salt crystal, which had been ground to a fine slip, was 0·019 cm. The distance apart of the extreme edges of the right and left images on the photographic plate is not actually given in the paper for these particular lines, but would be very nearly 10 cm., and could be measured with great accuracy.

A method described by Uhler and Cooksey (*Physical Review*, December 1917) is novel in certain respects and seems very simple and accurate.

A photograph is taken first with the plate close up to the crystal as at P′P′ fig. 2, and again when the plate has been moved back to PP, through a distance which can be measured very exactly. Reference points R and R′ were obtained by exposing the plates for very short intervals to the direct stream of X-rays, the crystal being removed.

The order of experiments was—

(1) Brief exposure at P′P′, no crystal, one-half (upper or lower) of the slit being covered.
(2) Crystal placed in position; cover removed from slit; exposure to reflected rays.
(3) Plate moved back to PP; further exposure.

(4) Crystal removed; short exposure to other half of the primary beam passing through the slit.

Measurement of the images so obtained gave at once the angle of reflection required. This is, of course, the important measurement. It can be found by determining two points on the direct line of the primary beam and two points on the reflected beam. In the method described this is exactly what

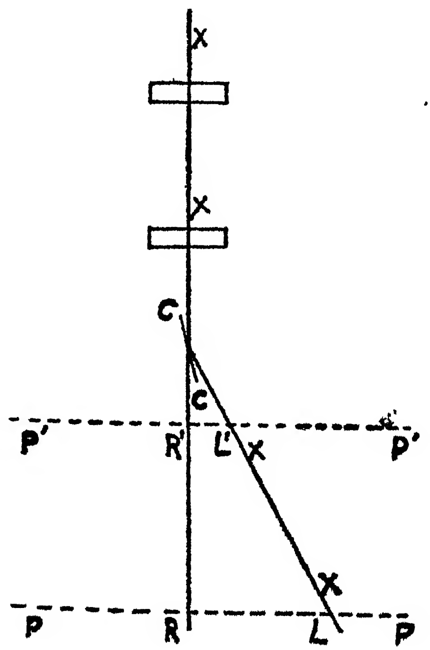


FIG. 2.

The pencil XX is reflected at the crystal C. The photographic plate is first placed at P′P′ to get the points R′ and L′, and afterwards at PP to get the points R and L. The exact distance between the plates being known it is possible to get an accurate measure of the angle between L′L and R′R.

is done. In some cases it has been assumed that one point on each line is in the face of the crystal; but since the X-rays actually penetrate the crystal the position of this point is not sufficiently definite, unless a very thin crystal is used and carefully placed in its right position.

In the ordinary spectrometer the similar case does not arise, because the object glass of the telescope sends all parallel rays to the same focus, so that the position of that focus deter-

mines the direction of the rays, without any error due to uncertainty as to where the rays started from. It does not matter if the position of the prism or grating is altered provided that its orientation is unchanged. In the X-ray spectrometer there is no focussing lens, and the determination of the reflecting angle must be made differently.

In many cases observers have measured, not the angle through which the X-ray is displaced right and left, but the angle at which the crystal must be set to displace it. This method avoids all uncertainties due to the volume nature of the crystal reflection, but it requires the use of the ionisation method and is not adapted to the photographic method. Perhaps it cannot reach the accuracy of Uhler and Cooksey's method, which certainly gives highly concordant results.

The following extract from their results will give an idea of the accuracy attained. The readings of the angular deflections by calcite for four independent sets of measurements of the lines α_2 and α_1 were :

TABLE II

α_2	α_1
12° 47′ 11″	12° 45′ 3″
12° 47′ 13″	12° 45′ 3″
12° 47′ 1″	12° 44′ 59″
12° 47′ 7″	12° 45′ 3″

An error of 5″ in the reading corresponds to an error in the wave-length of about one part in 9,000.

From such examples it is clear that as an accurate measurement the determination of X-ray wave-lengths has made strides. Collected results have been given in tabular form by various writers, *e.g.* by Siegbahn, *Jahrbuch der Radioaktivität und Elektronik*, XIII. 3, September 1916, by Malmer and by Friman in separate publications issued by the University of Lund in Sweden, by De Broglie in the *Comptes Rendus*, by Duane and others in the *Physical Review.*

As a result of this work of the last two or three years the accuracy of our knowledge of X-ray spectra has been increased considerably, certain conclusions which had been drawn tentatively have been put beyond doubt, and fresh ground has been broken.

First of all with regard to the original law enunciated

by Moseley, no break is observable under this closer inspection in the plotted trace which shows the connection between frequency and atomic number. The curve is still quite smooth. The earlier experiments showed a tendency for the ratio between the square root of the frequency and the atomic number to increase somewhat for the higher numbers; and the tendency is maintained. It must be a real effect and not due to some experimental error.

It has been known for some time that an X-ray of any frequency can be excited by an X-ray of a different frequency only if the exciting frequency is the higher of the two. It

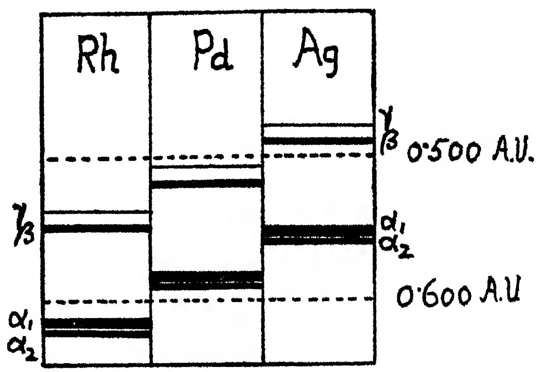


FIG. 3.

The figure shows the K series for rhodium (atomic weight 102·9, atomic number 45), palladium (106·7 and 46), and silver (107·9 and 47). As the atomic number increases the lines move up towards the higher frequencies, but their relative positions and intensities do not change.

had also been shown (W. H. Bragg, *Phil. Mag.*, March 1915) that in a certain case at least the K series of any element could only be excited by X-rays of frequency at least as great as the greatest frequency of the series. For example, consider the K spectra of the elements Ag, Pd, Rh, shown diagrammatically in fig. 3. In the experiments referred to it was found that Pd had no special absorption for any of its own radiations, nor even for the α-rays of Ag. But the β-ray of silver was very highly absorbed by Pd; and it will be observed by reference to the figure that the wave-length of this ray is just less than the least wave-length of the K series of Pd. From other sources it is known that a sudden increase in

absorption like this goes with the excitation of the characteristic X-rays of the absorber. Hence the inference mentioned above. Moreover, it appeared that once the frequency of the exciting ray passed this critical limit, all the lines of the K series were excited at once; the relative intensities of the lines have always been the same in all experiments in which they have appeared.

These conclusions have been made very clear, mainly by the work of Duane, Webster, Hull and others. In a special experiment by Webster and Hull, described by Duane and Kang-Fu Hu in the *Physical Review*, June 1918, the wave-lengths of the K series of rhodium were found to be, in three separate determinations, as follows:

TABLE III

K SERIES OF RHODIUM

Wave-lengths in Å.U.

	α_2	α_1	β	γ
(1) . .	0·6164	0·6122	0·5451	0·5342
(2) . .	0·6163	0·6121	0·5453	0·5343
(3) . .	0·6161	0·6120	0·5454	0·5342

The critical wave-length which is just short enough to be highly absorbed by rhodium was found by arranging for a continuous spectrum in this region to be given by an X-ray bulb having a tungsten anticathode, and placing a screen of a rhodium salt in the path of the rays. It was then found that absorption set in at 0·5330, which is just shorter than the wave-length of the γ line.

The critical frequency is extremely definite; and as Dr. Duane has pointed out, it is probably more representative of the element to which it belongs than any of the radiations of the K series, since it is unique, whereas the K series has four different members at least. Blake and Duane have therefore made a number of determinations of the critical frequencies for various elements, some of which are given in the *Physical Review*, December 1917 and June 1918. With these are given some previous results of similar character due to De Broglie and Wagner. See the accompanying table (IV.)

TABLE IV

Element.	Atomic number.	Wave-length in A.U.	Square root of frequency $\times 10^{9}$.
Tungsten	74	1·78	4·13
Cerium	58	0 3073	3 125
Lanthanum	57	0·3188	3·068
Barium	56	0·3307	3·011
Cæsium	55	0·3444	2·951
Xenon	54	—	—
Iodine	53	0·3727	2·837
Tellurium	52	0·3896	2·775
Antimony	51	0·4065	2·716
Tin	50	0·4242	2·659
Indium	49	0·4434	2·601
Cadmium	48	0·4632	2·545
Silver	47	0·4850	2·487
Palladium	46	0·5075	2·431
Rhodium	45	0·5324	2·373
Ruthenium	44	0·5584	2·320
—	43	—	—
Molybdenum	42	0·6180	2·203
Niobium	41	0·6503	2·148
Zirconium	40	0·6872	2·089
Yttrium	39	0·7255	2·033
Strontium	38	0·7696	1·974
Rubidium	37	0·8143	1·919
Krypton	36	—	—
Bromine	35	0·9179	1·807

This table contains Blake and Duane's own results, together with a result for tungsten, which is estimated from Dershem's result as given in Table III. above. The figures in the last column are calculated from the values of the wave-lengths. Note their even increase with atomic number.

Such frequencies are to be looked on as among the most fundamental characteristics of the atoms.

In a second direction, greater experimental accuracy has made it possible to determine in a new way the radiation constant h.

From many converging lines of evidence it has for some time been supposed that there are discontinuities in either the emission or absorption of radiation or in both these processes. According to certain theories energy of radiation of a given frequency is transferred in multiples of a unit which is equal to the frequency multiplied by the constant h; and not in any fractions of that unit. The idea is a funda-

mental one ; and one which is not provided for in the older theories of electromagnetic radiation which have been hitherto able to account for observed facts. Its application to the present case would lead to the anticipation that the frequency of the X-rays emitted as the direct consequence of the impact of an electron would be equal to the product $h\nu$, where ν is the frequency and h the constant already referred to. The value of this constant as determined from a comparison of the original theories with experimental facts of radiation which they were intended to explain was known to be nearly $6{\cdot}5 \times 10^{-27}$.

There is at Harvard University a set of no less than 20,000 small storage cells. With this battery a very steady and measurable voltage can be put on to an X-ray bulb. As the voltage increases, so does the energy of the electron which excites the X-ray, according to the rule

$$Ve = \tfrac{1}{2}mv^2$$

where e and m are the charge and mass of the electron and v its velocity. It has been found (Webster and Duane) that no X-rays of frequency ν can be generated in the tube until the applied potential has been raised to the value V where

$$Ve = h\nu,$$

that is to say, until the energy of the electron is equal to the anticipated amount (see fig. 4). Whatever the anticathode in the bulb there is always a *general* emission of X-rays of all frequencies ranging from the critical value downwards to smaller values. The critical value is very sharply defined. In fact, if it is assumed, as it is natural to do, that this interpretation of the results is correct, we have here a very excellent method of determining h. The following table shows the result obtained (see Blake and Duane, *Physical Review*, December 1917, p. 637). Some previous results are also given.

By Means of X-rays

Duane and Hunt, *Phys. Review*, August 1915	$6{\cdot}51 \times 10^{-27}$
Hull, *Phys Review*, January 1916	$6{\cdot}59 \times 10^{-27}$
Webster, *Phys. Review*, June 1916	$6{\cdot}53 \times 10^{-27}$
Webster and Clark, *Proc. Nat. Ac. Sci.* p. 181, 1917	$6{\cdot}53 \times 10^{-27}$
Blake and Duane, *Phys. Review*, December 1917	$6{\cdot}555 \times 10^{-27}$

In converse experiments, when *e.g.* light rays of a given frequency cause the emission of electrons from certain substances (photoelectric effect), a similar relation is known to hold. The energy of the fastest electron which is emitted from the surface of the substance is related to the frequency

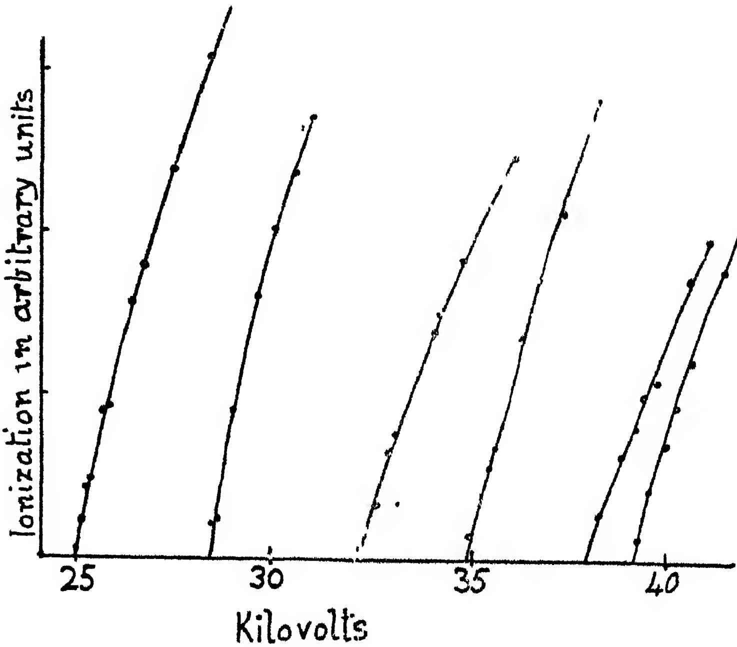


FIG. 4.

The figure is taken from a paper by Duane and Hunt, *Phys. Rev.* August 1915, p. 166. The crystal and the ionisation chamber of the X-ray spectrometer are set to catch rays of a certain narrow range of frequency ν, and the volts on the X-ray tube are gradually increased. Nothing appears until $Ve = h\nu$. This limit being passed the rays come in with a rush, and their amount, as measured in the ionisation chamber of the spectrometer, increases very rapidly as the voltage is increased still further. The anticathode of the X-ray bulb was of tungsten. None of the narrow ranges chosen for examination, and each represented by one of the curves to the figure, contained a characteristic line of the tungsten spectrum.

by the law given above. In this way have been determined the results given below, quoted from the same paper.

DETERMINATIONS OF h BY MEANS OF THE PHOTOELECTRIC EFFECT

Millikan, *Proc. Nat. Ac. Sci.* April 1917 . . .	$6{\cdot}57 \times 10^{-27}$
Kadesch and Hennings, *Phys. Rev.* September 1916 .	$6{\cdot}43 \times 10^{-27}$
Sabine, *Phys. Rev.* March 1917	$(6{\cdot}58 - 6{\cdot}71) \times 10^{-27}$

Webster, *Phys. Rev.* June 1916, has measured the electromotive force that must be applied to the X-ray tube to produce the *characteristic* radiations of the target as distinguished

from the *general* radiation spoken of above. The only difference is that all the lines of a K series appear together, always with the same relative intensities; and the energy of the electron must be that which is associated by the same equation with a frequency just a little greater than that of the γ-line (see fig. 5). See above, p. 577.

All these measurements and confirmations combine to give a simple and striking picture of some aspects of the radiation process. We see that moving electrons excite X-rays,

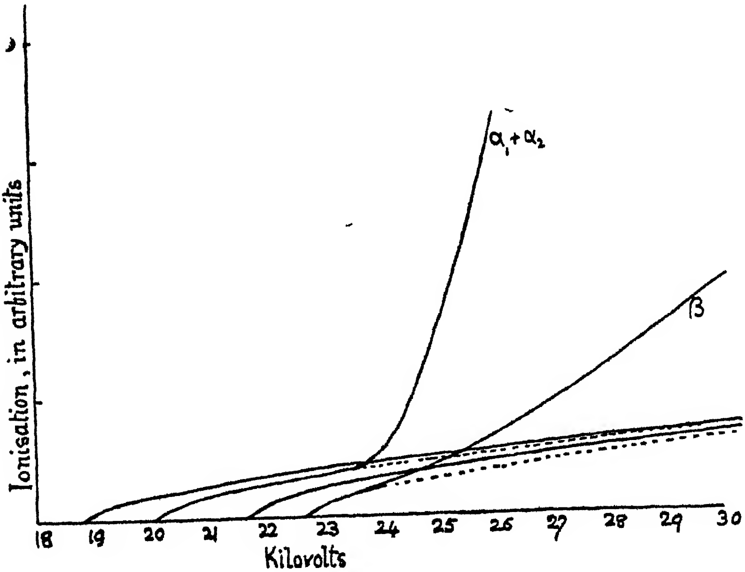


FIG. 5.

In fig. 5 the process recorded is the same as in fig. 4, except that the narrow ranges of frequency represented by the four curves include in one case the α-ray of rhodium and in the other the β-ray. Moreover, the anticathode is of rhodium and not of tungsten. The figure is freely drawn from Webster's results. Note how in the two special cases the curves suddenly break away from the normal form, because when the voltage has reached 23·3 kilovolts the α_1 or the β-ray lines are respectively excited. This voltage corresponds to a frequency just above the γ-line. The real voltage of the α-line—that is, that which is given by $Ve = h\nu$—is about 20 kilovolts, and that of the β-line is a little less than 23 kilovolts.

and that X-rays set electrons in motion; and always, in these occurrences, there is a fundamental condition to be satisfied. The energy of the electron and the frequency of the radiation are always proportional to each other. It is possible to define the motion of an electron by the wave-length which it can just excite. We might talk of a yellow electron, or an ultraviolet electron.

It is true that when a plate is struck by a stream of elec-

trons, all having initially the same velocities, the rule just stated is followed only in the case of the upper limiting frequency of the X-rays emitted; and that much of the X-radiation has frequencies below the frequency corresponding to that of the electron energy. But the electrons lose speed in penetrating the plate and soon possess velocities varying over a wide range from the initial velocity downwards. Consequently it is to be expected that there should be a corresponding diversity in the emitted frequencies, even if an electron, moving with a given speed, can produce X-rays of one frequency only.

So also when a pencil of X-rays of given frequency strikes a substance, a stream of electrons which is made to issue from the substance has velocities ranging downwards from that which corresponds to the given frequency. But that also is to be expected on the simpler hypothesis, since even if the electrons all have the same speed initially, they have to make their way through more or less of the substance before they get out and will by then have acquired a great diversity of velocity.

These two statements may be rounded off by adding that when a stream of electrons having the same velocity is allowed to fall on a plate, the " reflected " or " scattered " electrons are found to be diverse in speed, none, however, moving as fast as the originals. So also it may be confidently anticipated that when a stream of X-rays of one frequency is made to fall on a substance, there will be an emission of X-rays of all frequencies, because electrons will be set in movement and these will cause the varied X-ray emission, as has been seen. In all these cases, of course, characteristic X-rays may or may not be excited, depending on whether the frequency of the incident X-rays or the energy of the incident electrons is above the critical value for the substance which is struck. If they are, they will show up as bright lines in the spectrum of the emitted rays, whenever a spectral analysis is made.

We may sum up the whole situation in the statement that whenever there are X-rays there must be electrons in movement, and *vice versa*, since the one radiation produces the other; and there must be a certain equilibrium between the quantities of each, which will be attained if the radiation once started is left to itself.

It is certain that a given electron energy or its equivalent

frequency can only be produced by at least as great an electron energy or its equivalent. It is very likely that a given electron energy produces a frequency of one kind only, viz. its equivalent, and *vice versa.* In other words a blue electron produces *only* blue waves and *vice versa.* Moreover, but this is a separate point, it may well be that the whole of the energy of the electron is converted into energy of radiation of the corresponding frequency. Exact knowledge on these points will make it possible to calculate the quantities of X-ray and electron energy which are in equilibrium with each other.

It is here that the X-ray work has such an interesting bearing on the general problem of radiation.

But it may be asked whether, when an electron is just able to excite the characteristic rays of say the K series, they must all come out of the atom which is struck. The electron cannot spend any of its energy in exciting one of the rays and then go on to another atom to excite another of the rays, because its energy will have fallen below the critical amount before it gets to the next atom. Either it excites *some* radiation of each ray in the one atom, dividing up its energy in doing so, or it excites only one ray in the atom in one case, and another in another case, there being a certain probability for each ray. It is a question to be solved.

A great difficulty has always been met with in attempting to explain the way in which the heat of a body is distributed between the various modes of vibration of which the body and its atoms and the ether in which it lies are capable. According to mechanical laws, each mode should have an equal share in the energy. But the number of modes of high-frequency vibration is so great compared to the number of low-frequency modes that the energy ought to be stored mostly on the high-frequency side. As Lorentz said in his opening address to the first Solvay Conference :

> " In a system composed of matter and ether the energy must always finish by accumulating in the ether where it will exist in the form of extremely short waves. This is the inevitable consequence of the theory of equipartition if it is applied to two systems of which one has, thanks to its perfect continuity, an infinite number of degrees of freedom, while for the ponderable matter the number is finite because of its molecular structure."

And again:

"If a metal contains particles which can vibrate with a definite frequency determined by their nature, how does it happen that these vibrators remain quite mute until a sufficiently high temperature has been reached? And if, instead of such vibrators, we prefer to think of irregular movements in matter, producing a like state in the ether, which we resolve into harmonic vibrations by an arbitrary and artificial process, how are we to understand why in this analysis the high frequencies disappear completely when the total energy diminishes? It is not possible to suppose that there is no connection between the light waves and the phenomena which go on inside a cold body, because, after all, the body absorbs light although it does not emit it. A mechanism must be invented which will permit the passage of energy under the form of rapid vibrations from the ether to the ponderable matter, but which excludes the passage in the opposite direction."

But when we consider the X-ray phenomena, and that X-rays are only light rays of high frequency, we see that there is here an effect of which older theories took no account. Its consequence is to prevent the radiation energy going into higher frequencies, which is just what is wanted. Whether it is a sufficient explanation is another matter. All that can be said at present is that such an event is anything but improbable. There are excellent reasons for supposing that there are electrons in movement in the interior of any substance, at any rate of any substance that conducts electricity; and that these electrons are the means whereby electricity and heat are able to move across the substance. It is also most probable that the velocities of the electrons are distributed according to Maxwell's law.

From what we have seen above there must be X-rays or high-frequency radiations to match. The rules which govern the rate at which X-ray energy is commuted into electron energy in crossing a given quantity of any material are becoming fairly well known; the rules of the converse operation are not so well known, though some work has been done on the subject. When the rules of exchange are known accurately, it will be possible to calculate the balancing amount of X-ray and electron energies. It looks at present as if all might come out right, and that we may be able to reach an explanation of the old radiation difficulty.

It is, of course, very interesting to inquire whether this critical electron energy which is required to excite the characteristic radiation of an atom is related to any other known characteristics of the atom. It is found that the velocity of the electron is very nearly $2\pi e^2N/h$, where e is the charge of the electron and N the atomic number, so that Ne, according to present ideas, is the charge on the atom nucleus. If this is an exact relation, it is a proper jumping-off place for a theory. It would be impossible, having got so far, not to make a passing allusion to the ingenious theory of atomic structure put forward by Bohr: but no discussion of it will be attempted.

This short account of recent work on the spectra of X-rays will show, it is hoped, what great importance lies in work of this kind. The laws which govern the conversion of X-ray energy into electron energy and of the reverse process must be fundamental so far as radiation is concerned. They are sure to throw light both on the processes of radiation and on the nature of the atoms from which radiation proceeds.

THE ICE-AGE QUESTION SOLVED

By MAJOR R. A. MARRIOTT, D.S.O.

Preface

The solution of this question is now ripe for acceptance, but since of all the sciences astronomy is perhaps the most conservative, it is inconvenient to await the cautious processes by which astronomers have to arrive at the conception of a new idea, when it runs directly counter to certain beliefs as set forth in their text-books. The aim of this paper is to free geologists from their astronomical trammels by convincing them that the dogma which has barred the way to an understanding of the glacial period is now practically removed. This can be shown to be the case both by admissions and statements made by astronomers themselves, and by the fact that Drayson has abounding geological evidence to support him, thus bringing the two sciences into mutual agreement. As a result of the expeditions to the Antarctic, the astronomical explanation of the Ice Age has been proved to be so wrong that it has become an admitted failure. On the other hand, the retreat of the ice at both poles is so much in favour of Drayson that it set me on the track to discover that glacial and other phenomena were in complete accord with him.

The evidence, however strong, since it involves a revolution in astronomical thought and system, must be fully proved before both sciences can be brought to alter the whole course of ideas on this subject. I propose to show cause for immediate inquiry into this question owing to the complete and multiple evidence which can be produced, and I am confident that a searching investigation will be justified by the result.

As the junior science cannot well adopt an essentially astronomical explanation without strong encouragement, I propose to deal with the celestial side of the question first, explaining the discovery and the inherent weakness of the astronomical position, but before exposing the latter I must enumerate briefly the geological facts which agree with

Drayson, with the intention of amplifying in a future article these facts and their significance.

Drayson's Discovery[1]

A circle is known to be traced in the sky by the slow motion of the pole of the heavens (*i.e.* of the axis of the earth pro-

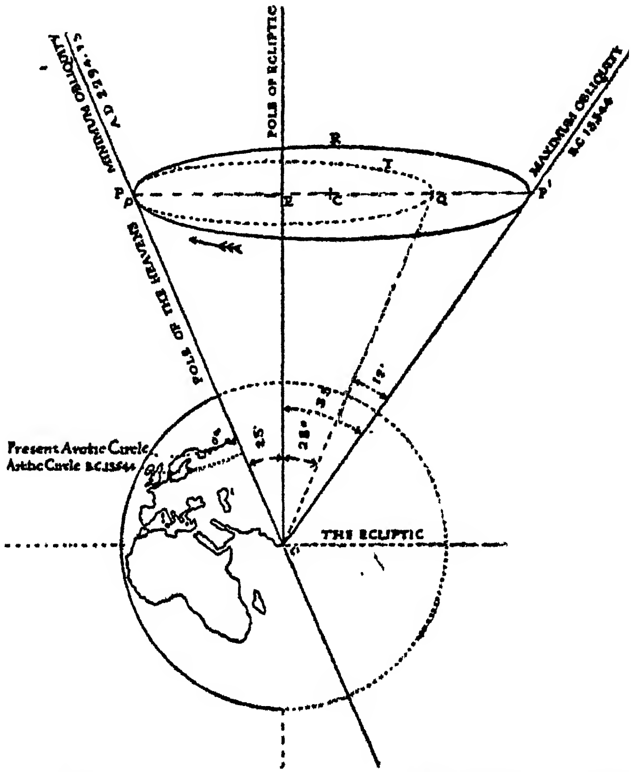


FIG. 1.—Diagram of the slow conical motion of the earth's axis which causes the precession of the equinoxes.

The dotted circle (in perspective) shows the path of the pole of the heavens according to the orthodox theory. The complete revolution is supposed to take 25,868 years.

The outer circle (PRP') shows the true path as found by Drayson and is completed in 31,756 years. Obliquity of the ecliptic at the maximum 35° 25′ 47″. Present obliquity (1919) 23° 26′ 59″·36.

duced) during a long period of thousands of years. The circle PTQ, shown in the diagram as a dotted line, astronomers represent as being described round the point E, the ecliptic pole, as the centre of the circle of movement. It is known,

[1] Fig. 1 illustrates the conditions and results of Major-General Drayson's discovery.

however, that the pole of the heavens now at *p* (near P), instead of keeping at a constant distance from the centre E, has been decreasing its distance for 3,000 years at least, causing the obliquity of the ecliptic, the tilt of the earth to its orbit, to decrease continuously. The present angle is shown as 23°, without the minutes and seconds. Drayson was determined to find out the cause of the decrease on this reasoning:

It might be possible, he thought, that the pole of the heavens always moved round the pole of the ecliptic E as a focus, but since these poles were known to decrease their distance 47″·6 per century, then the course of the pole could not be a *true circle*. Also it might be possible that the course of the pole was a true circle, but in this case the centre of the circle could not be coincident with the pole of the ecliptic.

After years of incessant labour, Drayson proved that the course *was* a circle, of which the centre C was 6° removed from the pole of the ecliptic E. Hence the circle described was larger than had been imagined, and would require, to be completed, about 31,756 years and not 25,868 as has been stated by astronomers.

He was thus able to attribute the decrease to the simple fact that the pole of the ecliptic is not the real centre, but that PRP′ centred on C is the true circle of the movement. These two circles are here shown in perspective. This discovery enabled him to state that the points P, E and C would be on the same great circle of the sphere in the year A.D. 2295. Tracing back from this date against the direction of the arrow, he found that when the pole was at P′ the obliquity would be about 35½°, *i.e.* 12° greater than at present, the obliquity being always measured by the distance between the pole of the heavens and the pole of the ecliptic E. This would cause a 50 per cent. increase in the tilt of the earth to its orbit, which of course would increase the arctic circles to the same amount, and consequently the extent of the ice-caps to a corresponding degree, causing a glaciation of the temperate regions in both hemispheres, bringing the arctic circle in this country as far south as Durham. Its present position is just north of Iceland.

The curve of the Drayson cycle is represented in fig. 2, taken from *Draysonia* (Longmans), by Admiral Sir Algernon de Horsey, a book dealing with the astronomical aspect of

Drayson's discovery in a most convincing way, and published in the same month as my first pamphlet,[1] showing some of its geological results. Incidentally I may mention that his fifteen years of labour on this subject began with endeavours to disprove Drayson.

One can see from the diagram that the end of what would have been a previous "genial period" is arrived at about 22,000 B.C. Here the obliquity begins to increase rapidly after being small for some 8,000 to 10,000 years, marking the autumn of the coming winter of glacial conditions, which culminated between 15,000 and 16,000 years ago. Analogously in the following spring of the cycle the obliquity decreased in equal measure, driving back the ice-sheets, and in this way furnishing a clear explanation of the phenomena which have induced geologists in other countries to hold views in absolute accord with Drayson's teaching.

I will now deal shortly with the various phenomena all the world over which are in agreement with the curve. It is impossible within the limits of this paper to deal fully with each aspect of the question, but they clearly point to the fact that we are on the track of the true solution of a long-standing enigma.

Phases of Drayson's Glaciation Cycle

(1) *A Maximum of Glacial Conditions at Date* 13,544 *B.C.*—It is the conviction of many geologists that the last glacial period was not prolonged much more than 25,000 years, and that it passed away some 7,000 years ago—not 80,000 as stated by Croll. This view is held by a large number of American geologists. The recent date of it is supported also by Swedish researches on phenomena entirely distinct from those presented in America. There is similar evidence in the Antipodes. Prof. E. C. Andrews endorses the opinion, formed many years ago by Haast, Lindenfield and Hochstaetter, that south-west New Zealand was under glacial conditions recently, *i.e.* to be reckoned in several thousands of years (not tens of thousands). His view is stated to have received much support from the observations of Prof. T. W. E. David, in southern

[1] Embodied in *The Change of Climate and its Cause* (Marlborough).

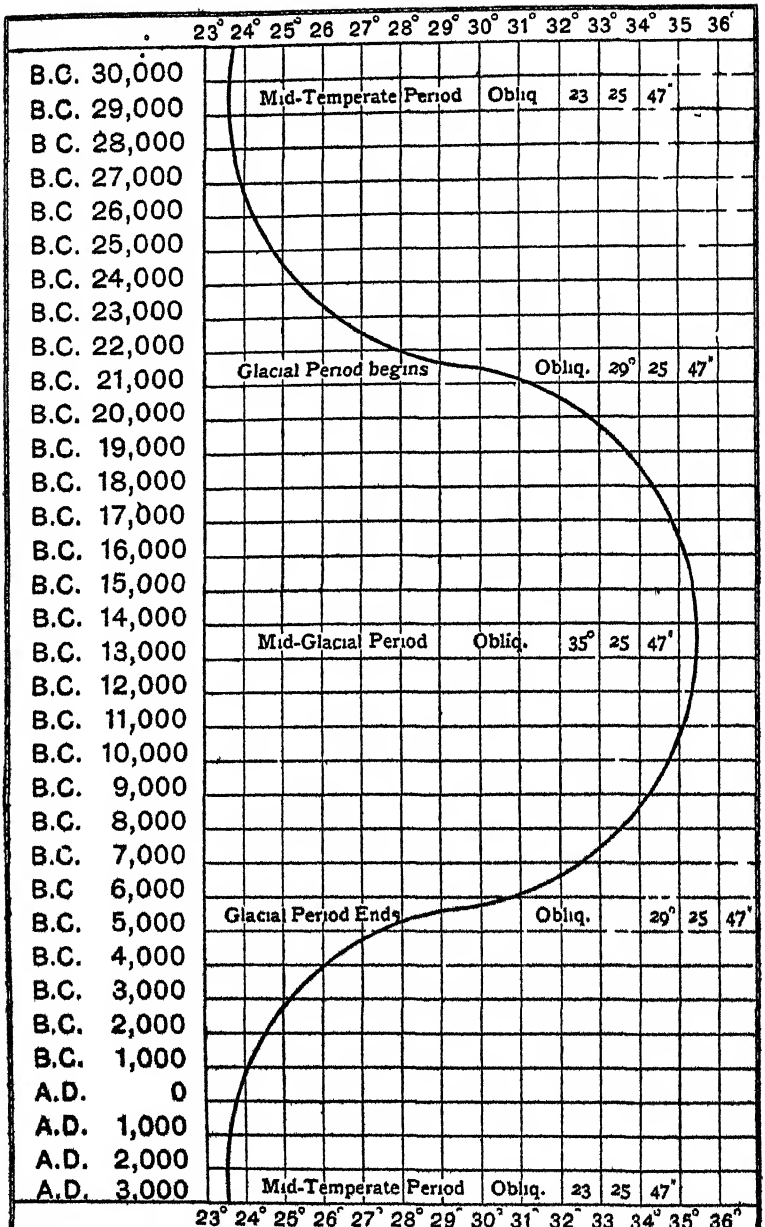


FIG 2.—The obliquity curve during a cycle of 31,756 years.

Showing the commencement, duration, and termination of last Glacial Period.

Reproduced from *Draysonia* by kind permission of Admiral Sir Algernon F. R. de Horsey

New South Wales, where the ice-marks and moraines have a very fresh appearance.[1]

This antipodean evidence is important under (4).

(2) *A Beginning of Glaciation about* 22,000 *B.C.*—This fits in reasonably with the general opinion of geologists of the last generation, who, though they had no idea of a recent glacial period, recognised that glacial periods were alternated by genial periods "every 10,000 or 12,000 years."

There is the further evidence supplied by the existence of pre-glacial forests in lat. 75° N. (St. Patrick's Island),[2] the remains of which testify by their present woody condition to the fact that they cannot be extremely old geologically, though preserved for many thousands of years from decay by the sterilising effect of dry cold on mycelium of fungus and bacterial organisms.

Prof. G. F. Wright, in his *Ice Age in North America,* describes remains of forests of cedars recently released from their ice covering standing upright in the soil with humus about their roots amid glacier surroundings in Alaska, which induced him to believe that they were flourishing "before even the glacial period itself."

Since the publication of the conclusions of Swedish geologists that the glaciation of Sweden was recent, and that Stockholm was under ice some 15,000 years ago, Dr. V. Holst has made further independent research. He showed me his calculations made before I had acquainted him with Drayson's discovery. These gave the date for the oncoming of the glaciation as 23,000 years ago, and 6,900 years ago for its passing away!

As flint implements of a type that is not paleolithic have been found under glacial deposits at West Holstein, and, I believe, in the Channel Islands, and neolithic axes in Minnesota under similar deposits,[3] the date of 22,000 B.C., startling as it may be to certain schools of opinion, furnishes a better solution of the difficulty than can be obtained by any other theory as to the minimum date to be assigned to them.

(3) *A Rapid Melting of the Ice between* 6000 *and* 5000 *B.C.*—

[1] *Australian Association for Advancement of Science,* vol. x. 1904, pp. 200–204.

[2] This is an interesting indication of the extent of polar regions to be opened up before the autumn of the next glaciation. We are at present at mid-June of the cycle.

[3] The Minnesota Historical Society, 1906–11. Report by N. H. Winchell on the Aborigines of Minnesota.

By the curve of obliquity furnished by the diagram one can postulate from its rapid fall from 6000 to 5000 B.C. that the temperate zones must have been under conditions of inundations, avalanches, and removal of landmarks on a stupendous scale. The rapid melting of the ice originally drawn from the ocean as water, and roughly represented by the bulge in the curve, must have had an appreciable effect upon its level when restored to it, and the sea would rise slowly along the coasts. The submerged forests are an eloquent testimony to this process, and the evidence that the course of events was in accord with this assumption is exceedingly circumstantial.[1]

Again, 7,000 years ago is the date generally assigned to the Lake Dwellings. Imagine the conditions of spring time in the mountainous regions of extreme latitudes during this rapid change: avalanches, which even now are dreaded, must have rendered life in the valleys impossible. Could any supposition fit in better with what is known of these villages? And what is more likely than that the world-wide traditions of the Deluge had their foundation in the cataclysms of this period? The engineering skill shown in pile driving must have been equal to making an efficient stockade were wild beasts only to be feared and not the forces of nature.

(4) *A Former Simultaneous Glaciation at both Poles* (conversely a decrease of ice in the Arctic and Antarctic at the present time).—This is borne out by indications in North and South America and elsewhere, which have convinced American geoolgists that the Ice Age was synchronous at both poles.

Mr. Ponting, in his lectures on the Antarctic Expedition, states that the ice has receded thirty-six miles in forty years. There has been an enormous decrease of ice in Alaska in the same time.[2] There are signs in South Baffin Land of recent ice-retreat, while similar reports come from Siberia.

There is also the retirement of Swiss glaciers; but over and above all this it is found that a process of desiccation is going on in parts of Central Asia, owing to a depletion of glacier and ice-sheet feeders for the rivers flowing from the Himalaya and Hindu Kush, which has given rise to some anxiety for the future. In observations made on this subject by the Surveyor-

[1] See *Submerged Forests*, by Clement Reid. (Cambridge University Press.)

[2] The Muir glacier in Alaska, which retreated some twenty miles in the last century, has since 1886 actually retreated at the rate of one mile every three years,

General of India at a meeting in Simla in 1914 of the Board of Scientific Advice, it was stated, without any regard to existing theories, that in that region geological indications point to the sway of a glacial period *some* 8,000 *years ago*, and that the rivers have been fed in part from the glacial deposits of an ice age, which has not even now ceased altogether to affect their flow.

One of the strongest points in favour of Drayson is that without any knowledge of these present-day conditions he opposed the astronomers' assumption of *alternating* ice ages at the two Poles, which, they went so far as to say, was absolutely essential to their theory. This will be shown later on.

(5) *A Former Greater Obliquity of the Ecliptic by Twelve Degrees.*—The obliquity has been decreasing steadily since the earliest known observation of 3,000 years ago. This is attested by the records.

In spite of this fact astronomers apparently have not grasped the significance of it, as they profess to be able to ascribe the decrease to a movement about the " Invariable Plane." It will be shown hereafter that this idea must now retire from public service.

(6) *A Minimum of Obliquity in the Year* 2295 [1] *A.D.*—This statement makes up for its lack of visual confirmation by forming the basis of numerous calculations. It seems to me to be a great triumph and a remarkable proof of the correctness of Drayson's calculations that I was able to work out accurately, *within decimals of a second,* the present obliquity of the ecliptic, without observation, by using this date. It must be remembered also that the year of minimum obliquity, 2295 A.D., enabled Drayson, relying on that certainty, to fix past events and to prophesy future ones, as well as to achieve other results.

After the rapid change of obliquity 7,000 years ago, the subsequent changes from century to century would be practically imperceptible, but millenniums would show a difference. Unfortunately we have nothing more definite than stray passages in Cæsar and Ovid, and historical touches here and there, pointing to colder conditions than now prevail in parts of Europe and Asia ; but what there is, is entirely in favour of a slowly diminishing winter temperature since these historical times.

There is said to be abundant evidence that between 1100

[1] Admiral Sir A. de Horsey makes the Zero Year 2294·75.

and 1500 A.D. the vine was planted and grew as a field crop in the South of England.[1] This evidence showing that, as we go back in time, warmer summers accompanied colder winters, is exactly what one would expect to find.

The polar conditions now disclosed under (4) are alone sufficient without any further justification to compel attention, even though astronomers have hitherto regarded Drayson as a "paradoxer." The truly paradoxical argument is that of astronomers, who prefer a circle with a movable centre. This is what their reasoning about the "invariable plane" amounts to.

Having amplified the discovery by showing its correspondence with geological facts, I will here summarise the main astronomical points which Drayson's new movement of the pole of the heavens explains.

A Comparative Summary

1. Drayson states, in opposition to astronomers, that the Glacial Periods were contemporaneous at both Poles.	1. The views held by astronomers regarding the Glacial Periods will, on their own showing, have to be abandoned.
2. Drayson maintained that errors in sidereal time existed, which by his data could be corrected. In 1892 the error by Drayson would be 41·28″, according to de Horsey's calculation.	2. Mr. Stone in 1892, then Radcliffe Observer at Oxford, working from a wholly different basis, asserted that sidereal time was incorrect by 41·51 seconds.
3. Drayson can compute the obliquity of the ecliptic for any year, past or future, without observations.	3. "Observations of the sun taken for this purpose are made continuously at Greenwich": Astronomer Royal.
4. Drayson, by his data, can compute the Right Ascension and Declination of any star for any year, in the past or future, from one observation of that star.[2]	4. Astronomers can, apparently, only do this from several observations at wide intervals, to arrive at a mean for application to the star in question. For an approximate forecast they are limited to a few years ahead.

[1] The *Selborne Magazine*, No. 301.

[2] Drayson recognised that many stars have proper motions of their own, but maintained that a great number of the so-called proper motions were "due to the movement of the earth's axis in a manner which has not hitherto been correctly interpreted." If the spectrum shows that the majority have a proper motion, the explanation may be that their enormous distances permit of such motions being practically negligible.

Drayson's one movement—the annual motion of the pole—accounts for	(*a*) The progressive decrease in obliquity and its rate. (*b*) The time and duration of the glaciations. (*c*) The changes in the co-latitude of stars. (*d*) The error in sidereal time.

5. Drayson shows that an explanation of the enigma of the "acceleration of the moon's mean motion" is furnished by the adoption of his polar motion.	5. "Gravitation is incompetent to explain completely the observed motion of the moon." "Astronomy is at a standstill for an explanation."

N.B.—I received the following from an astronomer: "As a bold outline your summary is admissible, but it would be as well to indicate that it is put forward more in that spirit than as an actual 'statement of claim.' The headings under *a*, *b*, *c*, and *d* (taken together) constitute a striking claim for investigation."

It will be seen from the foregoing that Drayson's astronomy indicates what should have occurred, and geological research shows that it did occur and that the evidence is still visible. Geologists may now revert to a very simple cause of glaciation—an increased obliquity—a cause which in the early days of geology was sought for as most likely to produce the observed phenomena. They have for so long been side-tracked from the only approach to an understanding of the glacial periods, that they have been forced to base their conclusions on a chain of physical and meteorological possibilities quite unworthy of figuring in any sane survey of the past history of the globe.

Progress in discovery has always been marked by the triumph of the simple over the complex, as is certainly the case in this instance. Every phenomenon falls into place as simply as summer follows winter and for analogous reasons.

The Astronomical Position

Reluctance to admit the proofs of Drayson's discovery, which obtained such magical results in astronomy, and enabled him to solve so many questions with comparative ease, reminds one forcibly of the philosophers who refused to look through Galileo's telescope, but it is fair to say that Drayson somewhat spoilt the exposition of his theory by his method of introducing it as a "second rotation" of the earth, as if it were a totally new movement; and by his ignorance of the technical interpretation of conical motion, which gave colour to unfavourable

reviews. In reality he was merely engaged in helping astronomers to define the true circle and period of precession by supplying information which they evidently lacked. In doing so he removes a paradox, and his views, instead of being regarded with disfavour, should be welcomed as a revelation. It must be remembered that Drayson had no primary interest in geology to make it his aim to prove great changes in obliquity to produce an Ice Age, but he was an astronomer to whom this aspect of the question came as an after-thought, and was considered by him as a side issue.

The opinion of European astronomers has not been unanimous on this subject. The late Astronomer Royal of Sweden, for instance, held the opinion that there were no limits to possible changes of obliquity; and since Drayson first announced his discovery, a significant change of attitude towards this question of obliquity is observable, though astronomers may not be conscious of its consequences. In fact they have drifted into a position which scarcely leaves any course open to them other than to accept Drayson.

It is instructive to trace the development of the dogma for which Newton is chiefly responsible, and thus to grasp the inherent weakness of the astronomical position. Newton, after discovering the Laws of Motion, found himself called upon to explain the Precession of the Equinoxes, which Copernicus was the first to state to be a motion centred on the pole of the ecliptic. This Newton attributed to the gravitation pull of the moon, sun, and planets on the belt of protuberant matter round the earth's equator. Knowing no other play of forces such as modern gyrodynamics have disclosed, and judging the earth to be symmetrical in shape, this was the best solution he could give, though it was scarcely in accord with one of the corollaries of his Laws of Motion, which demonstrates that one body may be considered to act on another body at its centre of gravity, irrespective of figure. Newton in his *Principia* states regarding the above corollary that with an oblate spheroid this is not "exactly true." Why exactly, one may ask? I only wish, however, to emphasise the fact that Newton was wrong in his assumed centre for the reason given below. From gyrodynamics we know that a similar precession movement can be induced by the friction of the tides on a rotating earth or by a slight alteration in the centre of gravity of a revolving

body, so that we have here another basis of investigation besides the pull of the sun and moon and planets on the equatorial protuberance. The present centre of polar motion may be the resultant, for all we know, of both sets of forces. Newton having found a cause for this time-honoured theory, the prestige of his name made it readily adopted ; hence the curve traced by the earth's axis in the heavens was accepted as a circle, and the centre of the circle must, if Newton was right, be the pole of the ecliptic.

The important point, however, is that if the earth's centre of gravity is not the centre of its figure, the centre of the precession curve cannot well be the pole of the ecliptic ; and that the centre of gravity is eccentric is supported by the fact that pendulum experiments disclose a want of symmetry in the shape of the earth. If to this is added the disparity in density and distribution of continent and ocean it will be recognised how improbable it is that the pole of the ecliptic and the centre of the precession movement should exactly coincide.[1]

In Herschel's *Outlines of Astronomy* we can now understand the theory on which Art. 316 is based, where he says : "It is found that in virtue of the uniform part of the motion of the pole it describes a circle in the heavens round the pole of the ecliptic as a centre, keeping constantly at the same distance of 23° 28′ from it." Later on he tells us that it does vary its distance at the rate of about 48″ per century. It is now 23° 26′ 59″·36.

So far the only explanation offered to meet this paradox of a circle round a point with a varying radius has been by ascribing it to a movement of the earth's orbital plane with respect to the invariable plane, investigated by Laplace with a view to establishing the stability of the planetary system.

Drayson has dealt with this matter fully and has shown that this movement, if it occurs, can by no means affect the obliquity of the ecliptic. As a matter of fact the distance between the pole of the heavens and the pole of the ecliptic,

[1] Robert Grant in his *History of Physical Astronomy* (Bohn), writing of Newton's solution of the problem relating to the attraction of spheroids, said that Newton "found that the polar axis was to the equatorial as 229 to 230. The ellipticity of the earth is considerably greater, whence it may be inferred that the density is not homogeneous." Recent seismological research has led to the opinion that below a distance of $\frac{1}{10}$ of the earth's radius the earth's substance is fluid (*English Mechanic*, February 21st, 1919).

which is the measure of the obliquity, can no more be affected by the changing inclination of the earth's orbit to the invariable plane, than looping the loop can affect the distance separating the pilot and observer. Even if Laplace's calculations regarding the invariable plane had no such imperfections as have been acknowledged by astronomers to exist, this fallacy would remain. No critics so far have touched on this crucial fact, but have confined themselves to disputing about comparative irrelevancies only. The decrease in obliquity is therefore an outstanding fact *per se*, and can only be accounted for now by admission that the pole of the ecliptic is *not the centre* of the precession movement.

During the last century astronomers felt it incumbent on them to find some reason for the Glacial Epoch and procured a spokesman in James Croll, who, basing his theory on the varying eccentricity of the earth's orbit as enunciated by Leverrier, produced a plausible explanation of the occurrence of glacial epochs as being due to the increased distance of the earth from the sun at different periods in the past. This made the last glaciation begin 240,000 years ago and end 80,000 years ago. This took the fancy of the geological world for the time and became the accepted astronomical view, though progress in geological knowledge has made the majority of geologists grow increasingly sceptical as to its truth, so that it is no longer a working hypothesis.

When Sir R. Ball took up the subject in *The Cause of an Ice Age*, he made the following commentary on the question of whether glaciation affected both poles at the same period or not:

"They (geological evidences) assure us no doubt that ice ages have occurred in both hemispheres, but they leave us uninformed as to whether the ice ages were consecutive or were concurrent. Now this is not a mere matter of ordinary significance,' as it involves an absolutely vital point in the astronomical theory of the ice ages. So much so is this the case, that if it could be shown that ice ages in the two hemispheres were concurrent the astronomical doctrine would have to be *forthwith abandoned* (italics in the original).

"Of course there is no chance of such a contingency arising, and I merely enunciate it to show the significance of the doctrine that ice ages in the opposite hemispheres were not concurrent, but were consecutive; in fact we may feel con-

vinced on astronomical grounds that this must have been the actual state of things."

Now nothing is more certain than that the glaciation was simultaneous at both poles, and not alternate, as established by researches in America previous to the South Polar Expedition. The discovery made subsequently in the Antarctic of the retreat of the ice there—and we know it is retreating at the North Pole—settles the matter once and for all, that glaciation and its converse in the two hemispheres proceeded simultaneously, so that the foregoing astronomical theory therefore must perforce be abandoned.

How far these failures have influenced astronomers in general to take stock of the position can only be surmised, though it is evident that their attention has been turned to the question of the invariable plane, because in the *Ency. Brit.* of 1906 we have the following:

"Attempts have been made by Laplace and his successors to fix certain limits within which the obliquity of the ecliptic shall always be confined. The results thus derived are, however, based on imperfect formulæ. When the problem is considered in a more rigorous form, it is found that no absolute limits can be set. It can, however, be shown that the obliquity cannot vary more than two or three degrees within a million years of our epoch."

As no cycle of precession under the circumstances could be more than tens of thousands of years, it will be seen that this last statement regarding a million years being required is not founded on any known fact, and may be regarded as merely a step in the process of climbing down from an insecure position, and as a concession to pious belief.

Thus one of the main supports for the fiction of a practically fixed obliquity, which barred the way to geologists, is now removed, and the next investigation in a more "*rigorous*" form will cause the collapse of the whole structure.

In the efforts of astronomers working with a wrong centre to meet the difficulties arising therefrom, and to standardise them, so to speak, they have unconsciously produced a most significant corroboration of Drayson's discovery.

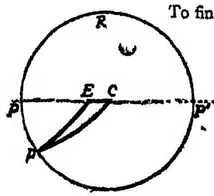
An empirical formula for finding the obliquity of the Ecliptic

in any year, formed by Newcomb from recorded observations supplemented by theory, is as follows:

$$\text{(Newcomb)}\ 23^\circ\ 27'\ 8''\!\cdot\!26 - 46''\!\cdot\!845T - \cdot0059T^2 + \cdot00181T^3$$

Compare this formula with Drayson's and it will be at once seen that Drayson's, being founded on a principle, is far less complicated:

(Drayson) [2294·75 (Zero year) − date required] × 40″ ·81 = angle ECp, whereby Ep the obliquity is obtained.



For any date (2294·75 − date required) × 40″·81 = angle ECp

Now in Newcomb's formula T represents the time factor, and if its value for the whole time is extracted, the minimum of the values for T is found to be 32,502 years, which is 6,640 years in excess of 25,868, but differs only 746 years from the period of Drayson's cycle. One or two critics have attempted to belittle this evidential value of T in Newcomb's formula, saying that its correctness cannot be depended upon, because if Newcomb experimented by extrapolation with a portion of an unknown curve, it is only correct in the neighbourhood of that portion, and the correspondence with Drayson is only a coincidence. That is perhaps a plausible argument for an unknown curve, but unfortunately for the critics it is the essence of the astronomical position that the precession curve must be a *circle* (on Newton's theory), of which any arc is therefore similar in all its parts to any other part of the circle, and again the same astronomical period of 25,868 years is founded on a circle, so that the value of T has a *decisive* significance.

The following is an extract from a letter by an astronomer:

"The maximum variation of the obliquity shown by Newcomb's formula is only $1\frac{1}{2}^\circ$, so that it is not identical with Drayson's: but that its period of variation should be so nearly

accordant with it is a fact worthy of consideration, since the formula is based purely upon a mathematical development of the collected results of observations and is not founded upon a single simple idea such as Drayson's 'true centre of polar motion,' and it should therefore be admitted as inferential evidence of value."

Finally in 1906 an astronomer, G. E. Sutcliffe, wrote a pamphlet which showed that Laplace had made an error between a + sign and a − sign, which has vitiated the whole of his calculations, putting him out of court in the matter of the invariable plane.[1] These calculations are all the armoury that astronomers had left to them wherewith to explain the present decrease in the obliquity; though it would appear that quite recently another cause has been brought forward to account for the decrease. This is the attraction of the planets for the equatorial protuberance. To quote from *Pioneers of Science* (Sir Oliver Lodge):

"Another and a smaller motion of a similar kind (*i.e.* to the precession caused by the sun and moon) has been worked out since. It is due to the unsymmetrical attraction of the other planets for the same equatorial protuberance.

"It shows itself as a periodic change in the obliquity of the ecliptic."

To this is added a footnote:

"The two motions may be roughly compounded into a single motion, which for a few centuries may without error be regarded as a conical revolution about a different axis with a different period, and Lieut.-Colonel Drayson writes books emphasising this simple fact under the impression it is a discovery."

In this attempt to belittle Drayson it is interesting to note the admission, for the first time, that the motion of the pole may be regarded as round a *different axis with a different period*, the sum and substance of Drayson's contention. Until this century there never has been any countenance given to such a statement. Precession was then always attributed to the action of the planets, as well as of the sun and moon. This,

[1] Sutcliffe further stated that the very theory on which the calculation was founded showed, when tested by spherical trigonometry, that the obliquity should —agreeably to the theory—be *increasing*, instead of diminishing, as it actually is. The pamphlet entitled *A Gigantic Hoax* can be seen at the British Museum.

however, was stated to be negligible.[1] These planets are now introduced in order to confound Drayson. This is not cricket, nor is it mathematics. One cannot dissociate the action of one component of a set of forces from the resultant in this way. Two phrases call for remark—" roughly compounded " and " for a few centuries." Mathematics are an exact science and do not permit of " rough " treatment. Now Drayson's exactness extends demonstrably over several hundred years, and Dr. Holst's geology unwittingly agrees with Drayson back to 21,000 B.C.

Are the " few centuries " behind us or still to come? When will the periodic change again change to make Drayson wrong and the astronomers right? Are not the planets always with us, exercising their attractions compounded necessarily with that of the sun and moon? And if Drayson is right in his different axis and different period " for a few centuries," who or what is going to prove him wrong for his longer period? I regard this criticism as a direct confirmation of Drayson.

The Position Reviewed

In tracing the history of this question it will be seen how from an assumption of Newton's, based on insufficient data, and formed before there was any knowledge of geology or of the want of symmetry of the earth, of present gyrodynamics, or of the progressive decrease of the obliquity, astronomers have been enabled by the help of Laplace, now partly discredited even by them, to repel and discountenance for a century such attempts as were made by the early geologists to find a cause for a glacial epoch in a change of obliquity; and great changes of climate depend wholly and solely on the only factor which is able to cause a universal change of climate all over the temperate regions of the globe—namely, a pronounced change in the angle between the earth's axis and its orbit.

The tracing of the circle of the precession movement in accordance with the conclusions of Newton is shown under

[1] On this question Herschel in *Outlines of Astronomy*, Art. 642, says: "The immense distance of the planets, however, compared with the size of the earth and the smallness of their masses compared with that of the sun, puts their action out of the question in the inquiry of its cause.'

all these aspects to be at variance with facts ; and consequently an adherence to former assumption meets trouble in every direction. How different the character of the curve becomes when traced by the hand of Drayson ! The position of the pole of the heavens in its movement round the newly found permanent,[1] and therefore real, centre at once explains the decrease in the obliquity. Ambiguity is no longer necessary. No need for the paradox of a movable centre nor for the changes in the orbital plane. One by one the enigmas of astronomy and geology are cleared up, and in the matter of glacial periods the two sciences have mutual support, and are brought into harmony with each other.

The extraordinary corroboration of Drayson supplied by Newcomb's formula, coupled with Dr. Holst's independent geological computation, are the most dramatic evidences as to the correctness of his discovery. The strongest evidence is perhaps that Drayson, with his knowledge of the true movement of the pole, can account for the "acceleration of the moon's mean motion," a most baffling enigma to astronomers—I am told that only three persons in the United Kingdom can attempt to work it out. This should be sufficient to stamp his interpretation of the curve followed by the procession movement as the correct one, apart from the simplifications which can be thereby introduced in the framing of nautical tables by the easy determination of stellar positions. There are also various matters now requiring elaborate computations which would fall into place without any difficulty at all.

The strength of Drayson's position hitherto has been that his opponents could only assert categorically that he was wrong, without attempting to meet his challenge by demonstration of their ability to make equally concise calculations.[2] They could only take refuge in stating that acceptance of his views was tantamount to opposing the Laws of Gravity. Sir R. Ball said once in conversation that it would upset the whole framework of astronomy.

The tables have now been turned, with the result that it

[1] The permanence is not claimed by Drayson to be indefinite, but only relative.

[2] Sir G. Airy acknowledged to Drayson that his formula was correct, but could not conceive how he had obtained it. I believe Drayson at the time kept this a secret.

is incumbent on astronomers either to publish the breakdown of the Laplace dogma, or take the next step of adopting another centre for the precession movement, with final recognition that Drayson is right.

Perhaps the real simplicity of the question will come as the greatest surprise to geologists, who in studying glaciations have grown reconciled to weird theories of volcanic dust, carbonic acid set free, dearth of supply of comets to the sun, and so forth, though none have a single fact to support them. The power with which a knowledge of Drayson's discovery invests any one in obtaining astronomical results without observation may be doubted by those who will not attempt to investigate his formulæ, but geologists who accept his data will find ample justification for every statement he has made regarding this last period of earth-history. The *universality* of the glaciations at both poles *at the same period* was the first indication that the astronomers were wrong, and that Drayson was right. To this have been added other facts which are each in their way telling proofs of the accuracy of his deductions. Resistance to Drayson must break down when the perplexing question of the "submerged forests" stands revealed as the direct outcome in time and place of the condition specified by Admiral de Horsey's curve. Mr. Clement Reid, without any thought of glaciation or its effects, gives a date for the rise of the sea (which he deems as a submergence of the land) and the probable duration of the process, which supplies a convincing and unintentional verification of the nature and duration of the last phase of glaciation as denoted by Drayson.

I for my part challenge any geologist to bring forward a single geological fact which can be found fundamentally in disagreement with the results of Drayson's discovery.

My chief difficulty until I found such a complete corroboration in the *Submerged Forests*, has been to obtain evidence in these islands which supported researches in North America, Sweden, in the Antipodes, and even in India, in which countries it has been shown that the last glaciation was recent, and only passed away less than 10,000 years ago.[1]

[1] See *Journal of the Torquay Nat. Hist. Soc.* 1917, "The Submerged Forests and the Last Glaciation."

GROWTH AND DIVISION OF CELLS AS AFFECTED BY RADIATION

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THE radiation under consideration here is that from 5 to ·005 A.U. thus comprising X-rays, the types usually spoken of as "soft," "medium," and "hard," together with the gamma rays of radium.

Such rays are now generally known to have profound effects upon animal and vegetable cells. Like other agents, their effects are essentially selective. A measured dose of radiation of certain wave-length which is sufficient for the destruction of one type of cell may be to all outward appearances without effect upon cells of a different variety.

The effects produced by a certain quantity of radiation, however, are largely dependent upon the way in which the rays are administered, *i.e.* the dosage employed. For example, the effect of a single large dose of X-rays upon the human skin is greater than that produced by repeated small doses, the total quantity of radiation being the same in the two cases.

There is at present no satisfactory explanation of the action which X-rays and the rays from radium have upon living cells. A good many facts have been ascertained, and some quantitative observations have shown that a wide range of sensitiveness to the rays exists in different types of cells. Were it not for the fact that in many cases malignant cells are more affected by the X-rays than are the normal contiguous cells, there would obviously be little place for radiotherapy in disease. When we try to push investigation further in an endeavour to find out what processes in the activity of the cell are particularly influenced, it must be admitted that the information so far afforded is scanty. Theories have from time to time appeared endeavouring to locate some specially vulnerable constituent of the cell, such as for instance lecithin or the chromatin of the nucleus, or again the enzymes secreted

by the cell. The evidence for these theories has hardly stood the test of criticism or further experiments, and it may be noted in passing that even though any one of these substances might be shown to suffer decomposition under irradiation it would tell us singularly little of the processes which, once established, lead to the destruction of the cell.

If little is known of the processes entailed in such gross changes as finally lead to the destruction of the cell, still less is the information yet obtained in the slighter and more subtle changes which occur in the living cell when it is exposed to quantities of radiation less than that required for a lethal effect. The dosage may be graded so that no actual destruction of the cell occurs, yet changes in its activity are induced; this may be shown by a reduced rate of growth which persists for many generations, without any recognisable histological changes in the cell or in the structure of which it may form a part.

With still smaller quantities of radiation the boundary line of what we may term "deterrent action" is crossed, and the phenomenon of stimulation makes its appearance.

It appears therefore that the rays whose action we are considering have the power to influence the cell in various ways, ranging from an actual increase in its general functions to complete inhibition of such. The analogy to the action of certain drugs is somewhat striking.

In endeavours to formulate a theory of the action of these radiations upon cell life, it seems to us that one fact established by experiment should be taken into consideration. We refer to the fact that certain cells have been found to be much more vulnerable to radiation when they are in the dividing stage than when they are in the growing stage of their life cycle. This has been observed in ova of *Ascaris megalocephala* and of *Planorbis*, two cases where it has been possible to study the whole cycle of development in isolated cells. The question arises as to whether the same fact holds in groups of cells forming a composite structure, as for instance in a malignant tumour. Here we are unable to separate the cells which are in a state of division from those which are in a state of growth, yet this is a question the answer to which bears intimately upon the procedure to employ in the irradiation of a malignant mass.

If we suppose for the purpose of illustrating the point that

in such cases the dividing cells are more vulnerable to the rays than are the growing cells, then it will be a matter of first importance to arrange that the majority of the cells forming the tumour shall for some period during the exposure be in the act of division. It will be seen later that in order to secure this, some information must be gained of the average rate of growth of the tumour and of the intensity of radiation which is necessary to produce some retarding effect upon the growth of the constituent cells.

On the simplest assumptions of cell growth and proliferation, mathematical expressions may be obtained (*vide* Appendix) which represent the numbers (N and n) of growing and dividing cells, at any time subsequent to inoculation. If λ_1 and λ_2 are two constants which characterise the rapidity with which the whole cycle of division, growth to maturity, and division again occurs, then the numerical values of these two constants for any strain of tumour may be obtained, provided (1) that frequent measurements of the rate of growth are made, and (2) that an estimate is made by microscopical observations of the relative proportions of the growing and dividing cells. These two constants tell us what fraction of the growing cells are reaching the dividing stage per second, and what fraction of the dividing cells are passing over to the growing stage per second.

Now the rate at which a tumour is progressing is proportional to λ_1 and λ_2; the larger these are the quicker the growth.

Confining our attention to one strain of tumour, if it be observed, say, that the tumour decreases in its rate of growth, then the ratio of N to n will remain exactly the same if λ_1 and λ_2 are altered in the same proportion as one another. If, however, when the rate of growth is diminished it be due to an unequal decrease in λ_1 and λ_2, this will show itself when the ratio of N to n is found experimentally.

In other words if we examine how $\frac{N}{n}$ varies for a tumour strain growing at different rates, we may be able to say whether the decreased rate is due to a retardation of the mytotic process or of the actual growth of the cells. This has been put to the test in the case of the Jensen rat sarcoma, which has been cultivated in the Cancer Research Laboratories of the Middlesex Hospital for the last seven years.

Six tumours were removed from rats after their general character of growth had been charted; counts of N and n were then made by an independent observer with the following result:

Character of growth.	Ratio $\frac{N}{n}$.
Very slow	300
Slow	240
Rather slow	204
Rather slow at start	176
Rapid	138
Very rapid	82

It will be seen that when the tumour grows more rapidly, the ratio of $\frac{N}{n}$ decreases, *i.e.* the proportion of dividing cells to growing cells is larger than when the tumour is growing slowly.

Unless the gradual diminution of the ratio $\frac{N}{n}$ with increased rapidity of growth is accidental, we may see to what conclusions it leads.

The normal strain of the tumour has a rate of growth such that it doubles its volume in about four days, and the value of the ratio $\frac{N}{n}$ is approximately 100. It appears from the mathematical expressions obtained that the simple relation $\frac{\lambda_2}{\lambda_1}=\frac{N}{n}$ holds, which being interpreted expresses the fact that, since the growth of the tumour is progressive, a very much bigger percentage of the dividing cells are passing over to the growing stage per unit of time than obtains in the complementary stage. The whole life cycle takes on the average a time T which may be observed (*vide* above, four days), and it can be shown, moreover, that the time t_1 spent by a cell in the growing stage is very much larger than the time t_2 spent in the dividing stage—again a simple relation holds, viz. $\frac{t_1}{t_2}=\frac{N}{n}$ where t_1 and t_2 are the respective times, and $t_1+t_2=T$. From this it appears that in a tumour which doubles its volume in four days the average time spent in the process of division is about one hour.

We return now to the fact that a slowing up of the rate of growth of the tumour leads to an increase in the ratio of $\frac{N}{n}$; although the tumour has increased in size it cannot at once be said that both N and n have increased. If, for instance, the cells suffered an undue enlargement in size, this for a time would simulate growth; such a possibility can be ruled out in the case in question, for no cell enlargement was observed. Hence although the rate of growth was diminished, N and n both increased (since subsequent growth was continuous), but N increased proportionally more than n.

We are therefore led to the conclusion that a diminution in the rate of cell growth subsequent to irradiation means that some change has been brought about in the processes that govern the growth of the young cell to the mature form. Further, although the dividing cell may be more vulnerable to radiation than the growing cell, this does not show itself in an undue lengthening of the time required for division; it might even show a curtailment of this period. The time for division is so short in comparison with the time for growth that observations of the kind recorded would not establish with certainty that any such change had occurred.

The statement that is warranted, however, is that if the rate of growth of the tumour is delayed, it is due to the processes of growth (as apart from division) having been hindered.

This fact is not without significance from a bio-chemical standpoint; with cell destruction and its disintegration products we are not only concerned. Information is much needed as to the changes which are set up in the metabolism of cells after irradiation; the considerations just set forth show us that, with quantities of radiation which produce effects less than lethal, we are essentially concerned with some alteration in the processes which govern the growth (which will include the normal intake and output) of the cell.

We now return to the consideration put forward on page 607 that in the irradiation of a tumour it is desirable to arrange that the majority of the cells forming the tumour shall, for some period during the exposure, be in the act of division. This may be illustrated from the data already given. Suppose that a tumour which doubles its volume in four days is exposed to the gamma rays from 100 milligrams of radium

for two hours. The probable effect of this would be a slowing up in the rate of growth of the tumour, and it is quite clear that only a very small percentage of the cells would be irradiated while they are in the specially vulnerable dividing stage. If we take it that the dividing cell is eight times as vulnerable as the growing cell, then $\frac{100}{8}$ milligrams will have as large an effect upon the dividing cell as 100 milligrams acting for the same time upon the growing cell.

Further, 100 milligrams acting for two hours is equivalent (as far as the product quantity and time is concerned) to $\frac{100}{8}$ acting for sixteen hours. The essential difference is that in the latter case eight times as many cells in the dividing stage are being irradiated compared with what occurred in the first irradiation.

It is clear that they are not receiving so much radiation, but the inference is that in the first case the few dividing cells receiving radiation had far more than was necessary to ensure subsequent hindrance of growth.

It is obvious that some limit is imposed upon the replacement of a large quantity of radiation acting for a short time by a small quantity acting for a long time. Experiment has shown that as regards the human skin comparable effects are obtained by reducing the quantity (gamma rays from 100 milligrams of radium bromide) to one-tenth and increasing the length of exposure by ten times, but if the quantity be reduced to $\frac{1}{100}$ with appropriate extension of exposure the reaction is not of a similar nature; it appears that the processes of repair can more easily cope with feeble radiation acting for a long time than with intense radiation acting for a short time. Too little is known of what may be called "flash-light" radiotherapy to say at what stage the "equivalence of effects" again breaks down when the intensity of radiation is increased.

Another limitation arises from the fact that the radiation must not be reduced in intensity beyond what is warranted by the factor of vulnerability (*vide* above—factor taken as one-eighth), otherwise the dividing cells will receive an intensity of radiation too small to prevent their subsequent division.

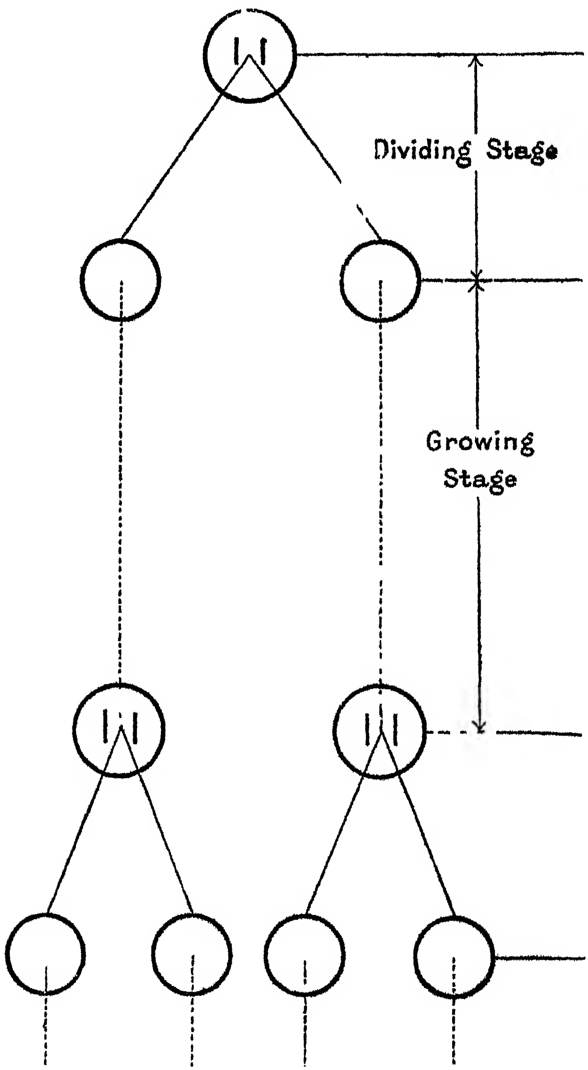
Such an adjustment of the quantity of radiation and the length of exposure can only be made with certainty when both the "vulnerability factor" and the intensity of radiation

necessary to produce a definite change in the rate of growth of the tumour cells are known.

It is a pleasure to record my thanks to Prof. A. W. Porter, F.R.S., for his unfailing kindness in criticism and help in the mathematical treatment appended herewith.

APPENDIX

If $N =$ the number of cells in the growing stage at any time t
$n =$ the number of cells in the dividing stage at any time t



and $\lambda_1 N$ = the number of growing cells reaching the dividing stage per second

$\lambda_2 n$ = the number of dividing cells reaching the growing stage per second,

Then $\dfrac{dN}{dt} = -\lambda_1 N + 2\lambda_2$

$$\frac{dn}{dt} = -\lambda_2 n + \lambda_1 N$$

Hence $N = \dfrac{N_0}{a-\beta}\left[(a+\lambda_1)e^{-\beta t} - (\beta+\lambda_1)e^{a}\right.$

and $n = \dfrac{\lambda_1 N_0}{a-\beta}\left[e^{at} - e^{-\beta t}\right.$

where $a = -\dfrac{\lambda_1+\lambda_2}{2} + \sqrt{\dfrac{(\lambda_1+\lambda_2)^2 + 4\lambda_1\lambda_2}{4}}$

$$\beta = +\frac{\lambda_1+\lambda_2}{2} + \sqrt{\frac{(\lambda_1+\lambda_2)^2 + 4\lambda_1\lambda_2}{4}}$$

These expressions reduce ultimately, after a few generations, to the simpler expressions—

$$N = -\frac{N_0}{a+\beta}(-\beta+\lambda_1)e^{at}$$

$$n = \frac{\lambda_1 N_0}{a+\beta}\cdot e^{at}$$

$$\therefore \frac{N}{n} = \frac{\beta-\lambda_1}{\lambda_1} = \frac{1}{K}$$

From the above $\beta = \left(\dfrac{1+K}{K}\right)\lambda_1$

but $a - \beta = -(\lambda_1+\lambda_2)$

$$\therefore \quad \lambda_2 = \frac{\lambda_1}{K} - a$$

Since a satisfies the auxiliary equation of the differential equation—

$$a^2 + a(\lambda_1+\lambda_2) - \lambda_1\lambda_2 = O$$

Inserting the value of $\lambda_2 = \dfrac{\lambda_1}{K} - a$

we obtain $\lambda_1 = a(2K+1)$

$$\lambda_2 = a\left(\frac{K+1}{K}\right)$$

If we take the experimental fact that the volume of an average tumour of the rat sarcoma of Jensen doubles itself in four days, it is found that $a = 2\times 10^{-6}$ sec^{-1}. The further experimental fact is that for such a tumour $\dfrac{N}{n} = 100$ approx.

Substituting these values in the expressions for λ_1 and λ_2 we obtain—

$$\lambda_1 = 2 \times 10^{-6}(\tfrac{2}{100} + 1) \simeq 2 \times 10^{-6}$$
$$\lambda_2 = 2 \times 10^{-6}(1 + 100) \simeq 2 \times 10^{-4}.$$

In such a tumour therefore it is deduced that one out of every five thousand cells in the dividing stage is going over to the growing stage per second, and one out of every five hundred thousand cells in the growing stage is going over to the dividing stage per second.

SOME SCIENTIFIC ASPECTS OF COLD STORAGE

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AND

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In an article we contributed to Science Progress a year ago [1] we had occasion to suggest that the specialisation of study which came about in the last century might not always have tended towards the development of many subjects, and may indeed have hindered the solution of important problems. We were then dealing with a question which illustrated this point, that of the electroculture of crops, and although the problems with which we deal in these articles are somewhat simpler in nature than those of electroculture, yet they occupy in many respects a rather similar position to the latter, especially in regard to the way in which specialisation of scientific studies has in all probability had a retarding influence on development. As in electroculture so in cold storage, a lack of realisation of the exact terms of the problem and a neglect to apply the principles evolved in pure science with a direct bearing on the problem, probably account for the slow development of the subject in spite of its obviously high economic importance.

The present position of cold storage problems has been expressed as follows by W. D. Richardson [2]: "If the authorities are consulted on the subject of cold storage, we find a variety of opinions expressed which are quite lacking in agreement, as would be expected in a subject which has been so little investigated in a scientific manner. In marked con-

[1] "The Electroculture of Crops," Science Progress, 12, 609–21, 1918.

[2] "The Cold Storage of Beef and Poultry," *Proc. First International Cold Congress in Paris*, 1908, vol. ii. p. 264.

trast with the opinions of scientific writers are the statements of men who have had practical experience in cold storage warehousing."

A more recent statement of the position of the cold storage industry in its relations to science, especially as regards this country, has been made by J. Wemyss Anderson in a lecture delivered before the Royal Society of Arts [1] at the end of the year 1917. Prof. Anderson emphasises the failure to apply the principles of pure science to the problems of cold storage, and he says: "While the applied science of the engineer has done much for the advance of cold storage, pure science has in this country done little or nothing for the commercial preservation of foodstuffs."

With this and much more said in Prof. Anderson's paper we are in complete agreement. In the present articles we shall attempt to show that even a brief consideration of the scientific problems involved and an application of scientific principles and methods, modifies profoundly the whole aspect of the subject, widens the scope of industrial application, and makes it possible to place the whole subject on a more definite research basis, and yet this is probably only what could be done in regard to the application of science to many industrial problems.

In order to give an insight into the range of problems with which we are concerned, and to indicate the main lines along which research is progressing, it is useful to attempt some sort of classification of the material for research. In the paper already referred to, J. Wemyss Anderson makes use of the following classification of food-stuffs which are kept in cold storage:

1. Produce whose life history is finished (meat, poultry, rabbits, fish).
2. Produce whose life history is not finished (fruit, eggs).
3. Milk and produce from milk (cream, butter, cheese).

This classification is perhaps not very fortunate, as it is rather immaterial whether the life history of the produce is finished or not, as in general the produce is not intended for use in continuing the life-cycle, neither apples nor eggs, for example, being stored in order to propagate the species.

[1] "Science and the Cold Storage Industry," *Journ. Roy. Soc. Arts*, **66**, 82–8, 1917.

The main distinction between food substances at present in regard to cold storage appears to lie in their methods of preservation. These are two in number and are as follows :

1. The temperature is kept above a certain value (0° C. or a little lower) whereby no change in the physical state of the produce is caused, such as in the case of chilled beef, fruits, eggs, etc. On the other hand, at these storage temperatures certain chemical processes take place which bring about the maturing of the substances or their deterioration.

2. Low temperatures are employed which cause an immediate alteration of the physical structure of the substance as a result of freezing. On the other hand any chemical actions, including enzymatic processes, will proceed at an extremely slow rate, and so the keeping properties of the produce will be enhanced. We are here mainly concerned with the question whether the processes involved in the freezing of the substance can be made reversible on thawing, so that the substance presents its normal appearance and retains its qualities, in regard to nutrition and taste. A much wider scope for variation in method is provided in this class of process, and one of the chief objects of investigation at present must be to transfer as many food substances as possible from the first group to the second.

Any classification is of course artificial, and the two classes are connected by a series of transitional substances. The further research progresses the more the sharpness of the distinction between the two groups will be effaced.

We shall in the following deal in succession with examples from these two classes. As an example of the first class we shall consider the cold storage of fruit, a produce which distinctly belongs to the first group, as up to the present its preservation by freezing has only been employed in exceptional cases. On the one hand not much variation in method is possible, but on the other hand a close study of metabolic processes is necessary in order to determine at what stage of development it is best to store the fruit and to discover for how long the fruit can be kept in store. It must also be borne in mind that with fruit it may be a case of preserving normal appearance, as when the fruit is sold as such to the consumer, or on the other hand storage may be a measure for dealing with gluts of fruit which is subsequently used in the manufacture

of preserves (*e.g.* jam), and here the questions of appearance and consistency are unimportant, and it is rather properties such as aroma and power of gelatinisation to which attention must be principally directed.

It must be strongly emphasised that the value of each species or even variety of fruit depends upon something very specific in the matter of aroma and flavour. On this account each species will have to form the subject of a separate investigation, largely biochemical, in order to determine the best conditions of storage.

We shall first consider the chemical changes undergone by fruits in general during ripening. These changes relate to two classes of substances: carbohydrates and organic acids. As regards carbohydrates the unripe fruit contains starch, disaccharides (chiefly cane sugar), and monosaccharides (glucose, fructose), as well as the cellulose of the cell walls, and degradation products of starch such as dextrin. In the first stage of the ripening process the starch decreases in quantity, and ultimately disappears, being hydrolysed to sugars,[1] which thus correspondingly increase. This decrease in starch content runs parallel with the softening of the fruit, but is not the only cause of it. The second stage of ripening is marked by the inversion of cane sugar into glucose and fructose, and this diminution of cane sugar and increase in hexoses continues until the fruit commences to rot. Throughout the whole of the ripening period respiration is proceeding by which the sugars are transformed to carbon dioxide. While the transformation of starch to sugar is characteristic of one phase of ripening, and the inversion of cane sugar of the second stage, respiration takes place in the earliest stages of ripening and continues over the whole period. It involves, of course, a loss of valuable material.

It was earlier assumed that the acid content of fruit did not undergo any significant alteration during ripening, and it was supposed that improvement in taste was due to increase in sugar content. It is now realised, however, that the acid content decreases continually during ripening, and that this causes the fruit to taste sweeter. The acid content must not sink too low as otherwise the taste deteriorates, the best flavour

[1] Bigelow, Gore, and Howard, "Studies on Apples," *U.S. Department of Agriculture, Bureau of Chemistry*, Bull. 94, 1905.

obtaining with a medium acid content. Many conjectures have been made as to the fate of the acid which disappears, but no definite facts are known. It is not unlikely that it also forms the material for respiration.

The disintegration of carbohydrates and acids involves the production of carbon dioxide, and it is therefore interesting to measure the output of carbon dioxide and correlate it with the state of ripening. It has indeed been suggested [1] that the amount of carbon dioxide evolved by unit weight in unit time may be taken as a measure of ripening velocity. We give below in table form a few examples from Gore of this value, which we propose to call the "respiration number." [2]

Gore's experiments were carried out at various temperatures and he found that the relation between respiration and temperature can be characterised by the equation

$$\log y = \log y_0 + at$$

where y is the respiration at a temperature $t°$ C.,
y_0 „ „ „ „ 0° C.,
and a is a constant, which does not vary much for different fruits, its average value being 0·0376. In the table the respiration numbers are all reduced to 0° C.

Species and variety.	Respiration number (in mg. per hour per kilo.).
Strawberry, Gandy	17·8
Raspberry, Cuthbert	20·4
Blackberry, El Dorado	30·9
Red currant, Fay	5·0
Apple, Missouri Pippin	4·6
Grape, James	3·8
Plum, Wragge	6·5
Orange, Valencia	1·8

It will be seen from the table that the values vary a good deal, the highest value being found for soft fruits with the exception of red currants and grapes, average values for stone fruit, smaller values for apples and pears, and the smallest for citrous fruits. Summer apples respire more rapidly than winter

[1] Gore, "Studies on Fruit Respiration," *U.S. Department of Agriculture, Bureau of Chemistry*, Bull. 142, 1911.

[2] This is analogous to the term "assimilation number" introduced by Willstätter and Stoll to represent the number of grams of carbon dioxide assimilated in one hour by 1 mg. of chlorophyll. See Willstätter and Stoll, *Untersuchungen über die Assimilation der Kohlensäure*, Berlin, 1918. Of course the analogy can only be approximate until one can determine the exact weight of respiratory material.

apples. The higher the respiration number the quicker the ripening process and the shorter the time the fruit can be kept.

From the equation found by Gore one can of course calculate the temperature coefficient. He gives a value for Q_{10} of 2·377, which is in agreement with the Van't Hoff rule. The maturation velocity at 8° C. is double that at 0° C. and at 16° C. four times that at 0° C., a result agreeing with that found by the experiments of others.[1]

It is obvious therefore that cold storage offers a good means for preservation of fruit. However, the same chemical processes occur as at higher temperatures.[2] The cooling causes only a slowing down and not a disturbance of the normal processes, but whether this can be accepted as a general rule is doubtful. For example, Molisch[3] points out that the different processes in plant organs are inter-related, and if the velocities with which these processes occur are altered in different degrees by temperature, the equilibrium is disturbed. In this way he explains the sweetening of potatoes at temperatures just above 0° C.; the potatoes form sugar from starch, and the sugar is used in respiration. If the temperature is lowered, sugar formation and respiration do not decrease to the same extent, sugar formation being reduced much less than respiration, the consequence of which is the sweetening of the tuber.

Thus it will be seen that the respiration number affords a convenient index for ascertaining the keeping possibilities. Another index which perhaps is still simpler is the temperature difference between the fruit and the atmosphere in the store. If fruit at say 15° C. is introduced into a cold store at, for example, 1° C., the temperature of the fruit will fall at a rate depending on the dimensions, spacing, and thermal conductivity of the fruit, and the condition of the air in the cooling chamber, until an equilibrium is reached after two or three days. This equilibrium temperature lies, however, one or two degrees higher than that of the chamber. This temperature difference is a characteristic of the different fruits, so that soft fruit which matures most quickly shows the greatest differ-

[1] *E.g.* Buison, "De l'emploi de froid dans la conservation des fruits," *L'industrie frigorifique*, 1912, p 165.

[2] Gore, *Proc Second International Cold Congress in Vienna*, 1910, vol ii p 348.

[3] *Untersuchungen uber das Erfrieren von Pflanzen*, Jena, 1897; *Pflanzenphysiologie als Theorie der Gartnerei*, 2te Auf, Jena, 1918.

ence. As respiration must be the cause of this constant heat production in the fruit, there must be a definite relation between the respiration number and the temperature difference. The temperature difference increases after a certain time of storage, for instance, in apricots it is 0·7° C. at the commencement of storage, while after four to five weeks it is 1° C. Thus a very simple and convenient means might be found for indicating when the time has arrived to remove the fruit from the store, as experiment would show at what temperature difference it becomes inadvisable to keep fruit longer.

It is easy to understand that it would be possible to keep the temperature in the cold store for fruit below zero, partly on account of this temperature difference, and partly because the freezing point of the fruit lies a little below 0° C., namely between $-1°$ and $-2°$ C. It has indeed been the tendency in the history of the cold storage of fruit to use lower and lower temperatures. The first temperatures used were 8° to 10° C., later it was found that fruits kept longer if kept at from 4° to 5° C., while now temperatures about zero are very generally used, but it is scarcely generally realised that it is possible to go still lower without any danger of freezing the fruit.

Thus there is not much scope for varying the conditions in the cooling chamber as regards temperature. The other factors which allow of variation are humidity, light, and movement of air, but in respect to these, conditions have to be worked out independently for each species, or even variety. Generally speaking, for soft fruits, which are easily destroyed by fungi, it is preferable to have a moderately dry atmosphere which is kept in motion, while for stone fruit and apples which shrink owing to loss of water, it is preferable to have moist and still air. In regard to illumination a diversity of opinion exists, some writers urging complete darkness and others a high light intensity.

As the value of some fruits depends on specific aroma due to the presence of volatile acids or alcohols, cold storage may be of no commercial value in these cases unless special precautions are taken to prevent evaporation such as by wrapping the fruit in paper or dipping it in a liquid which easily solidifies and covers it with a protective glaze.

(*To be continued*)

CORRESPONDENCE

To the Editor of "Science Progress"

A NATIONAL SCIENCE LIBRARY

From R. E. Slade, D.Sc., F.I.C., M.C.

Dear Sir,—I have recently had occasion to make a complete search of the scientific literature of certain physical and chemical subjects, and in the course of this work the defects of our present system of libraries in London has been continually brought to my notice.

I am aware that these defects are generally recognised, and that steps are already being taken to remedy them by the formation of a large central scientific library in London. The formation of such a library will no doubt be of great value to scientific workers who are in London and near to the library, but I think a very much more comprehensive system should be set on foot to meet the demands of scientific workers in the future. The rate of progress of scientific research will depend more and more on the facility with which we can obtain the results of previous investigators.

It is impossible, with our present library system, for a scientific worker away from London to write a complete monograph on any scientific subject.

I propose that a Central Library for the English-speaking races should be formed in collaboration with all English-speaking scientific societies on the following lines:

Each society would carry on its own meetings and select its papers for publication without any interference from the Central Library.

Each society would circulate its own reviews and proceedings as at present, but instead of circulating a journal containing original communications it would deposit some definite number of copies of every original communication with the Central Library.

The library would circulate to the members of a society abstracts of all papers on that subject published in all languages.

A member of a society would, after seeing the abstracts, decide that he would like to read certain of the papers. These he could obtain on loan from the Central Library, and any which he wished to keep he would be able to purchase for a small sum. In this way every member of a society would be able to read the literature of his own subject in his own home; he would have a complete set of abstracts for searching the literature and could always obtain any paper by return of post. If all societies printed their papers in a standard size, it would be possible for a member of a society to collect and bind in volumes all papers on particular subjects.

The function of the library is fairly well explained above, but in order that papers should be available to people in all English-speaking countries, it would be necessary for a certain number of copies to be kept in a branch library in each English-speaking country.

If all copies of a paper were used up owing to a large demand for this paper, another edition would be printed, and this would generally be a business proposition, and entail no financial loss to the library.

The library would translate, print, and store copies of scientific papers published in all other languages. This will be found more necessary as time goes on, for though it is now possible to obtain access to the large majority of scientific papers by a knowledge of English, French, and German, it is probable that there will be scientific as well as social revolutions in some of the countries of Eastern Europe, and the Slavs, Poles, and Czechs will probably wish to publish the results of their investigations in their own language instead of in German as at present. Even now the publications in Russian and Italian, which are of no mean order, can only be obtained in the form of imperfect abstracts by the majority of British scientific workers.

This scheme may appear large and expensive, but I do not think that the organisation would be too difficult for the British and American nations, who have shown their capacity for forming huge organisations during the war. As to the cost, money expended on scientific research could not be better invested.

A scheme on these lines would cause English, already the language of two great nations, to become the international language of science.

Yours faithfully,
R. E. Slade,
Director of Research,
British Photographic Research Association.

SCIENCE AND THE INTERNATIONAL LANGUAGE

From H. Whichello

Dear Sir,—I should like to suggest that you should publish an article in each number of Science Progress of general scientific interest in the simple Latin Messrs. Prideaux suggest. A common pronunciation would have to be agreed upon. Each country now has its own way of pronouncing the consonants. That of the Roman Church would be the most convenient, as it can be heard in all countries where there is a large Roman Catholic church or cathedral.

Yours faithfully,
H. Whichello.

NOTES

Rights against Duties

Now that the war appears to be over, the men who rule us and who caused it are busy explaining that it was really due, not to themselves, but to poor Humanity in general. The Politicians blame the Militarists; but when charged with helping to precipitate the disaster by refusing to prepare for it, reply that the people would never have made the necessary sacrifices—though they never asked the people to do so. On the other hand, the Militarists declare that in any case war was inevitable owing to the pugnacity of the people, and that they seized the offensive because it is the best defensive—though they omit to mention that they never attempted to placate their enemies, but for fifty years went about clanking the sabre and shaking the mailed fist. Lastly the Anarchists blame both—but do not explain how it is that when they have the power they commit what are perhaps the foulest crimes recorded in history. Meanwhile poor Humanity is left to-day among her tombstones, counting her losses and weeping.

This war, like previous ones, was due to the single fact that many people—perhaps the majority of mankind—prefer their rights to their duties. Let us analyse this word *right.* It is unfortunate that in the English language the same word denotes different if not opposite things. As an adjective *right* is the equivalent of *proper* and *true,* and is a good word. But what do we mean by the unqualified substantive, *a right*? It connotes a property which belongs to us by some supernal sanction. A right is not the same as a *legal* right—which is conferred upon us by the law of our country. Nor is it the same as a *just* right—which we may think is due to us by the balancing of justice between man and man. But when we say that we have a right to live, or to breathe, or to eat, we imply that we possess this right quite independently of our fellow men; that we were endowed with it when we were

born; and that not all our fellows, or our enemies, or our rulers, or any one else, can deprive us of it. But what proof have we that we ever really received such an endowment? . . . And then we shall proceed to claim a right to work, say, or to marry, or to acquire wealth, or to live in the sun. But who gave us these rights, pray? Show the title-deeds. You cannot: then your claim is a pretence made in order to extract something from your fellows! No, there is no such thing as a right; we have no rights; we possess only such privileges as nature and our fellow men choose to allow us.

This word has been the curse of mankind from time immemorial; for if we once admit a right to anything, we must admit a right to everything; and if for one, then for all—and trouble follows. If there is a right to possession, the right must be equal for all. A man who has a right to possess a house has the right to possess as good a house as you have, or as the king has: and then he is aggrieved if he does not possess a palace, and sets to work to seize the king's house by force. Such "rights" indeed constitute the whole text of almost all mob-orators, demagogues, radicals, anarchists, and the like; and the logical sequences of their teaching are the Russian Bolshevik and the horrors which he has inflicted on his country. But these people are by no means the only prophets of the false god. We have heard of the rights, even of the divine rights, of kings; and these are generally associated with the rights of numerous deputy-kings; but never have we seen any proof, or indeed any evidence, of the reality of such things. Lastly there are the rights of nations; or of classes, such as the "working classes"; or even of persons interspersed among a population but who hold different creeds or believe themselves to belong to a different race to the rest of the population—such as the Irishry in Ireland. All these rights are very fine things; and at present, during the Peace Conference, we are being deluged with them, and with the "aspirations" of numerous petty tribes which have generally been failures in the world, but wish to obtain territories belonging to more able nations. In fact observation suggests that, with nations as with individuals, the possession of many rights is almost always associated with the absence of the qualities which give prosperity and also with the strong desire to become more prosperous in the future without undue

fatigue. And herein we begin to discern the true cause of the "rights" delusion. Nature has implanted in many people a dislike of labour; and, also, experience proves that a stalwart profession of rights often succeeds in acquiring much more value than will ever be obtained by a stalwart exhibition of hard work—as in the case of the Irishry during the war. Further, it will be observed that all politicians of every shade have one character in common, they are invariably preachers of the great superstition of rights: the demagogues, of the rights of the people; the "reactionaries," of the rights of their special autocrat; the diplomats, of the rights of their own nation or tribe. And as unfortunately all these cannot obtain their rights simultaneously, their clamour, reacting upon the immense stupidity of the masses of mankind, is apt to produce little disturbances such as those from which we have been suffering during the last four years.

Now let us examine *duties*. Can it be claimed of these also that they are imposed upon us by supernal command? There are many who think so. In that great Book, which is obeyed so scrupulously by the Christians who caused the war, there is no mention whatever of rights—but of duties, innumerable. There is no Decalogue of rights, but only one of duties; and, in the Prayer, God is besought and not commanded to give us our daily bread. So also in those other bibles, the great Poems, duties are ever opposed to rights; and the whole teaching of the Fathers of Humanity has been based upon this text. But apart from those injunctions (which some may scoff at) there is this supernal fiat regarding duties, that without them we perish. Civilisation can exist without rights, but not without duties. That is a fact—and facts are supernal.

Who is free from duties? Only he who is free from everything—that is, no one. On this earth there is no such thing as freedom, and liberty does not exist; and these are the lies of the rights-mongers. We are all slaves—slaves of our fellow men; and from our birth we are bound in their chains. You may try—openly by violence, or secretly by cunning—to break those chains; but then they will be bound more tightly about you. You may pretend to liberty by vaunting supernal rights or a supernal conscience; but you will find that the rights and consciences of your fellows are still more supernal.

You may pretend with Rousseau that you have a "social contract" with your fellows, binding you to obedience only on condition that they behave agreeably to yourself; but you will find that the contract is like one between a mouse and a lion. "Show us your agreement," the world will say, "and the seals and signatures thereof"; and you may shriek deprecations to it, but the lion will cover you with a paw and reply gruffly, "You are a little liar! I make no agreements with a thing so small as you." A single molecule of a mass of lead, say, might as well protest against the pressure of its millions of fellow molecules. That pressure is a law of Nature. Our duties, also, are the laws of attraction and cohesion combining us with other individuals. But our rights are only the fancies, or the lies, of politicians.

Admit then that we have no rights, you may say; but why should we not all possess the same privileges? Because it is impossible. Some one must do the gross work of the world—must dig the coal and cook the dinners. Not all the cells of the body may be the brain-cells, watching the beautiful earth through the windows of the eyes and communing with heaven. This, too, is a law of Nature. Nature is not omnipotent; she does the best she can; she may wish to make us all kings and gods—but she cannot. Nor can human science and all the parliaments and revolutions in the world do more. And here enters another and perhaps a divine law—that the tiny ant bearing his burden along the track is probably as happy as the lion in his forest; and the factory-hand, deftly managing his humming engines, probably more happy than the king, vexed by the clamours of a hundred rights-mongers all pulling different ways.

Be this as it may, but the following law is always true. Try to break the chains which our fellows have bound about us, and they become chains of iron; but try to bind them closer, and they become chains of flowers. Only a little while ago, a party of politicians were commending liberty and thundering against every kind of compulsion, even that which forced them to fight for their country. But a philosopher among them said, "Why so? Freedom is not my god; I do not desire liberty; I have given myself into the hands of my country; I serve a master, and glory in the service; and others serve me, and glory in theirs." "Ah,"

cried the politicians, "but what of those who are at the bottom and have none to serve them?" "Let them serve well," said the philosopher, "and they shall soon be at the top. Serve your master well, and he shall serve you well, and you shall both be crowned. Nothing that a man can do may be higher than this—to serve well; and nothing that a man can do may be lower than this—to serve no one. Not by your rights but by your duties shall you become gods."

The General Meeting of the Association of Public School Science Masters (C. L. Bryant, M.A., B.Sc.)

The nineteenth Annual General Meeting of the above Association was held at the London Day Training College on December 31 and January 1. An important change was made in the constitution. The Association owes its birth in 1900 to four Eton masters, who have not yet laid down the burden of their work at that school—Dr. T. C. Porter, and Messrs. W. D. Eggar, M. D. Hill, and H. de Havilland. The membership was restricted, almost exclusively, to schools of the Headmasters' Conference; for it was thought that there were special problems connected with the public schools which waited for solution. Many of these have now been solved. Against few only of the best of such schools can the charge still be made that the opportunities for good work in science are not sufficient. Those, too, who have read the series of resolutions passed in January last by the members of the Headmasters' Conference and the Association of Headmasters in Joint Session must have been struck by the recognition given to the value of science in education by those who are in the best position to advance the teaching of the subject. Two of these resolutions read as follows:

> That suitable instruction in natural science should be included in the curriculum of preparatory schools, of the upper standards of elementary schools, and of all boys in public and other secondary schools up to the age of about sixteen.
>
> That mathematics and natural science should be necessary subjects in the entrance scholarship examinations of public schools, in the first school examination, and in the examinations for entrance into the Navy and the Army; provided that good work in other subjects should compensate for comparative weakness in mathematics and natural science, and *vice versâ*.

This is a striking expression of opinion about a subject which, until recently, was often regarded with suspicion, if not with active disfavour.

Perhaps it was a feeling that their position had been won in the public schools which inclined the Association to broaden its membership so as to include science masters in all types of secondary schools, and to change the name accordingly to "The Science Masters' Association." Mr. W. D. Eggar, of Eton College, one of the original founders, is fortunately to take a leading part, as General Secretary, in directing the activities of the bigger Association. One of the first tasks it has undertaken is the starting of a quarterly magazine entitled *The School Science Review*, which will be issued first in May from, it is hoped, the same press as SCIENCE PROGRESS. Mr. G. H. J. Adlam (City of London School) will act as Editor.

In connection with the meeting there were exhibits of apparatus by members and dealers. A description of most of these would be of interest only to teachers; but one, by Mr. R. G. Durrant, of Marlborough College, is of more general scientific importance. This was the use of crystal violet in indicating the hydrion content of aqueous solutions of acids. When a strong acid is added to a solution of the dye, an orange tint is produced: on dilution, or on the careful addition of an alkali, there is a gradual change through the colours of the spectrum, until violet is reached. By adding measured proportions of the dye to different concentrations of various acids, the hydrion contents are revealed by the tints obtained. Mr. Durrant's figures agree well with data obtained by conductivity experiments. He says that the dye also lends itself in a remarkable manner to the illustration of such phenomena as reversible action, equilibrium, and the progress of chemical change; and he suggests that it may be useful as an indicator in certain titrations, since it shows the approach to neutrality before this is actually reached.

Colonel Sir Ronald Ross presided during the meetings. As his address to the members was reported in *Nature* of January 9, it is not commented on here, except to quote the expression, "With our teaching of most things, we potter about in the porch and never look into the temple at all." This proved to be the key-note of many of the subsequent discussions. For years past the members of the Association have been criticising their own methods of teaching science, with a feeling, vague at first and then more clearly defined, that something was wrong somewhere. At the time when the Association was formed, science teaching had little weight in the public schools: for the most part the subject was taught only to those boys who had special need of it, the future specialists in science. That being so, it was natural—though perhaps wrong, even for these—for the subject to be presented in a systematic and formal manner. All the science masters in the public schools had been through the classical routine; they had approached the literature of a language through its grammar, and they applied the traditional method in the teaching of their own subject. The result was that the boys who failed to achieve their object of specialising in science had "pottered about in the porch and had failed to look into the temple at all." Now, however, circumstances are changing: far more boys are studying science than mean to make special use of the subject later; and it is coming to be realised that even for the future specialists a more liberal treatment of the subject is to be desired.

The discussions, therefore, were carried on along two main lines of thought: reform in the methods of teaching, and the effects of external influences. With regard to the former, early specialisation, it is agreed, is wrong: sixteen years, or thereabouts, is the earliest age at which a boy should be allowed to follow his bent at the sacrifice, to a certain extent, of other important subjects of the school curriculum. This is the age at which he is faced with the first school examination, designed to test his general education rather than his special abilities. For boys younger than this the courses in science training should be self-contained, not designed to lead up to something which, in the majority of cases, does not follow. The human interest should be kept in the foreground all the time. No school course in general science is adequate if, by the omission of the biological sciences, the phenomena of life are disregarded entirely. As Mr. G. H. V. Civil (Wellington College) pointed out, in such teaching there is difficulty in ensuring accuracy and diligence on the part of the pupils; but that is a problem for schoolmasters to solve without falling once more into the error of going into too much detail at the outset. Mr. W D. Eggar, in opening a discussion on this subject, described an experiment at Eton College of having some of the classical masters responsible for

part of the science teaching in the lower parts of the school. Elementary ideas of astronomy, heat, and light are given to the boys by their form masters. The experiment seems to have been a success as far as the boys are concerned, and it has the added value of arousing interest in things scientific on the part of these masters, who have so much influence over the lives of their pupils.

The change from the formal and sectional teaching of science to the general treatment of the subject is being impeded by examinations. These must, by their nature, hinder any progress. Examining bodies say that it is their duty to examine candidates in what they are being taught, not to influence the teaching. The schedules they draw up tend to make the teaching stereotyped, and it is only in the most independent of the schools, where examination results may be more or less disregarded, that any substantial change can be initiated There happens to be a striking example of the examination evil at the present moment. Nearly three years ago the Association drew up a scheme of work called "Science for All," which was intended to show the lines on which the teaching of general science might run for boys below sixteen years of age. At the meeting much dissatisfaction was expressed with the new regulations for the School Certificate Examination, in which the Oxford and Cambridge Joint Examining Board appear to have ignored the wishes of the Association in this matter. The papers in science are still, it seems, to be restricted to physics, chemistry, and biology, in the old water-tight compartments. A resolution was passed that the schedule should be broadened by the inclusion of an alternative paper on general science.

Far too long have schools suffered unheard under the imposition of examinations by external examining bodies on which they are not represented. The universities of Oxford and Cambridge especially are at fault in this matter. On the Joint Matriculation Board for the northern universities, to give an example on the other side, there is substantial representation of schoolmasters. This system should be imitated for all examinations which affect schools. One looks with hope towards the work of the Secondary Schools Examination Council, which has enlisted the help of many schoolmasters in their investigations.

Having passed the sixteen-year-old examination, a boy who remains at school has the opportunity of following his own bent to a certain extent. The kind of science teaching, for those who choose that subject, should now be more formal and systematic than in the earlier period. Here again examinations have a big influence. The normal test for competence in this work should be the second school examination. But the competition for university scholarships distorts the normal course. As long as these scholarships are awarded for merit in one subject alone—at some colleges they are granted for proficiency in a single branch of science—there is a direct incentive to undue specialisation. Mr. F. S. Young, Headmaster of Bishop's Stortford College, spoke of the necessity of restricting specialisation in these examinations and of giving weight to general education. The standards of the papers, he said, should be lowered; candidates should be required to offer one or more subjects subsidiary to their main ones, and should produce evidence of a satisfactory general education before being allowed to compete for the scholarships. At present the standards of many of the papers in science set for scholarships at the colleges at Oxford and Cambridge are far too high. The Association has repeatedly, but without avail, protested against this unrestricted tendency to raise the standards in these examinations, and it is urgently necessary to set up some machinery which will have a restraining influence on the examiners.

The foregoing is an old problem. The Rev. S. A. McDowall tackled a more

recent one, that of the kind of science teaching which is suited to the needs of boys who are specialising in other subjects; for now, in many of the public schools, science has invaded even the classical sixth forms. Mr. McDowall described a course which has been designed with this object at Winchester College. As the subject is both novel and interesting, it seems best to quote the speaker's own words.

Courses in General Science for Classical Sixth Forms

First, the ideal at which we aim. There seem to be three possible ones.

(*a*) We may seek to give the older boys some useful knowledge of the machines, and industries, and processes which they are likely to come across in everyday life. There is much to be said for such a course; but against it may be urged the fact that these are things which any boy can acquire for himself, by reading and the exercise of a little tact when he manages to get an introduction to some one at the nearest works, and that there is something more important still which cannot be acquired in this way.

(*b*) We can seek to "interest the boys in science." There are no arguments in favour of this ideal, and many against it. Of these the chief is that it is sure to fail, resulting in boredom, tempered with mild amusement. The reasons for this need not detain us. The chief one is the boys' instinctive realisation that they are playing on the surface, and not exploring the depths.

(*c*) The third ideal is this: that the boy shall leave school with a certain power of detached judgment and criticism; that his attitude to the experiences of his ordinary life shall be a scientific one; and that he shall feel that physical and chemical facts underlie human activities and human problems.

If we adopt this last ideal, we must face the difficulties and recognise that the laying of a firm foundation of facts in the earlier years is vital to success, for you cannot safely generalise without facts. And here there are some psychological factors with which we must reckon. In early years a boy can assimilate facts, but they remain in water-tight compartments. Simple trains of reasoning can be followed, and even to a certain extent reproduced. But that is all. This is the time for laying foundations; for it is generally impossible, whatever method you use, to break down the walls of partition that separate the facts.

Normally, at the age of about seventeen, the power of correlating and generalising begins to develop. This is the time for our final course to start, if we are to have any chance of achieving our aim. The water-tight compartments can be broken down now; they could not before. The facts long since learned can be recalled and fitted into place, and they will not be forgotten again quickly, for they are seen to be of use. Memory only brings up from the depths those facts which are of practical use; she will not bring up a single one that has been left in a water-tight compartment; and, as a rule, very few of the facts we teach our non-specialists in science escape to the wider air. The compartments must be broken down now, or probably never.

Before proceeding to a summary of the last year's work, let us note very briefly what is done in the years of foundation-laying. The course is a five-year one. Two lectures a week are given, generally experimental. But for the war an additional period of a week would have been given for practical work during most terms. It is hoped that this will be realised before long. The work starts two-thirds of the way down the school. No doubt field-botany and astronomy might advantageously receive a mention lower down.

First Year.—Physical geography.

Second Year.—Hydrostatics and heat.

Third Year.—Chemistry (based upon air, water, chalk, and salt).

At this stage the specialists branch off, and we may leave them on one side.

Fourth Year.—Physics, directed partly towards principles, partly towards instruments, very little towards formulæ.

The first term is devoted to optics and sound, a good deal of time being spent on the wave-theory; the second, to electricity and magnetism, including electric

waves; the third, to modern molecular physics, starting with the kinetic theory of gases, and passing, *via* surface tension and osmosis, ionisation, and spectroscopy, to electrons and positive-ray analysis. This third term is of vital importance as preparation for the last year's work.

In the *Fifth Year's* work, which will mainly, but not entirely, concern boys in the sixths, the first term is devoted to what may be called the philosophy of science, the second to mechanistic biology, the third to wider biological problems.

In the first term, the boys will learn the scope and limitations of natural law, the value of theory and hypothesis, the abstract nature of science, and the limited conception of reality which it gives. They will learn something of the transformation and degradation of energy on thermo-dynamic principles very simply explained, and they may even learn something of the problems of the evolution of consciousness, assuming for the moment the fact of evolution. In the second term they will learn something of the anatomy, physiology, reproduction, and evolution of certain typical animals and plants. At this period, the work should be almost entirely based on the idea of the creature as a mechanism. Their knowledge of osmosis, surface-tension, ionisation, and the like will come in as a basis, and the different branches of science will begin to fall into place. The breaking down of the water-tight compartments may be very effectually secured in this way. The recapitulation theory and a discussion of the origin of seed-plants will pave the way for the next term's work on evolution.

In the third term, the evidence for, and mechanism of, evolution are dealt with briefly, but carefully. Theories of heredity are discussed, and their practical application to agricultural and stock-raising problems. Then come sociological applications, and some of the wider issues, political and social. Insanity, feeble-mindedness, the rise and fall of families, alcoholism, tuberculosis, genius, at once suggest themselves; and legislative attempts to secure temperance, improvement of the race by education, and such-like afford matter of real interest and importance. So, also, do eugenic questions and the effects of war on a race. Experience, comparatively brief as yet it is true, has shown that such a course as we have outlined does arouse a great deal of enthusiasm, and we believe that a great work in educating future employers, teachers, and, above all, administrators, is possible along these lines.

It is evident that if such a course is properly carried through with due attention to the literary and human matters with which it can make contact—with constant cross-references to humanistic studies—the gain for the mathematician and the historian will be as great as for the classic. While far, no doubt, from ideal, it at least has this to commend it: it has a definite aim, and to some extent that aim appears, in practice, to be achieved.

Health Conscription

Sir Ronald Ross's Presidential Address to the Association of Public School Science Masters, referred to on page 629, and published in *Nature* of January 9, contained a suggested scheme which he called Health Conscription. This scheme was further detailed in *The Evening News* of January 7th, and the editor kindly permits us to copy the statement:

In my Presidential Address to the Association of Public School Science Masters, after mentioning how I have always found as a military medical officer that a period of open-air military training under discipline, combined with good food, greatly improves the physique, health, mind, manners, and moral of recruits, I went on to say that:

For this reason I should be in favour of universal military training everywhere; but, on the other hand, I admit the force of the argument that such *military* training may be an incentive to puerile wars—though I am not sure of it. On the whole, therefore, I would at least suggest an alternative scheme—that is, a scheme

of what I call *Health Conscription*, consisting of at least a fortnight's compulsory *physical* training, under discipline in the open air, for both sexes every year for five years between the ages, say, of fifteen and twenty.

Of course, if the general principle of such a scheme were to be accepted, the details would require careful consideration and, probably, amendment by experts. Personally, I should prefer a month's to a fortnight's course, or, possibly, periods graduated according to age; and instead of the ages between fifteen and twenty being selected, it might be better to take those between ten and fifteen for girls and twelve and seventeen for boys. Three instead of five years may suffice.

The training should be given in the open country in some of the admirable hutted camps built for soldiers, most of which will be available immediately after the declaration of peace. Probably the summer months will be selected, and the young people could be pushed through the courses in batches, so that the same instructors and camp-staffs could be used over and over again.

I anticipate, of course, that food, cooking, bedding, lavatories, etc., will be provided, just as they are for soldiers, with parade grounds, drilling and messing sheds, reading rooms, and other details. The sites must, of course, be selected with care and with regard to accessibility, sanitation, salubrity, and even beauty of scenery, and the existing camps will afford a wide choice. Free railway tickets to and from the camps must also be provided. Until details are decided upon it is not possible to quote figures regarding the total accommodation that will be required and the cost of the scheme.

For the boys' camp the actual training should, I think, be similar to the preliminary training given to recruits, that is, various kinds of drill, exercises, and route marches, increasing in amount towards the end of each course, when the boys will have become hardened.

Whether training in the use of weapons is to be given in the later courses will depend on political decisions; but in any case the preliminary stage of drill will have been passed through; and if training in weapons is not to be allowed, other kinds of training or field instruction can be substituted. The evenings will be utilised for out-of-door games, followed perhaps by singing, lectures, and other forms of instruction.

For the girls' camps the training must, of course, be suitably modified. In both cases much of the teaching now administered in the smoky atmosphere of schools in towns may under the proposed scheme be better imparted in the camps. Class certificates should, I suppose, be given at the end of each course to the young people who deserve them.

Obviously the scheme is merely, or partly, an extension of the admirable Boy Scout and Girl Scout movement due to the genius of Sir Robert Baden-Powell. It can be linked up later with Territorial or with universal military training, as may be decided upon. In any case it will *give to the whole population the health-giving open-air exercise* now enjoyed by boys in our public and certain other schools.

Personally I should be in favour of the scheme being ultimately made compulsory, like other branches of education, for all classes, because I should deny that faddy parents have the right to refuse to their children anything that makes for their health and happiness. But we are an air-loving people, and I fancy that all classes will welcome the principle.

Of course the cost will be large; but even if it amounts to several millions annually it will be small compared with the cost of the physical and mental degeneracy now too often inflicted upon the young who spend all their lives in the bad air and the slums of our cities.

To begin with, however, I think it would be better, as Sir Henry Cowan, M.P., has suggested to me, to make the scheme optional. The first step would apparently be for the new Parliament to allot sufficient funds to the Board of Education to enable it to take over some of the military camps, as these become available, for an experimental trial of the system.

I do not propose that this camp training should replace the daily physical raining given in schools—as Lord Gainford seems to have thought in his address to the Association of Assistant Masters. The camp training should be *in addition to* the physical school training. Few people will, I think, admit that twenty to forty minutes' physical training in school yards, even though given daily, will produce anything like the effect on the young which will be produced by the system outlined above.

For the young people of our great cities, at least, mere removal into fresh air once every year during the summer will be an incalculable benefit. Lord Leverhulme, who from his own work and experience in this line is entitled to be considered our leading authority, tells me that he is entirely of my opinion. But I am only making a suggestion, and the details must be worked out by others. I am glad, however, that the idea appears to have been received with interest in many quarters.

The Teaching of the History of Science

The Scientific Monthly for September 1918 contains an excellent article on the Teaching of the History of Science, by George Sarton of the Carnegie Institution. In criticising the attitude of professors of Philosophy, who seem to think that the history of science is taught when they speak of Thales, Pythagoras, and Democrates, he says: "The chief requisite for the making of a good chicken pie is chicken; nay, no amount of culinary legerdemain can make up for the lack of chicken. In the same way, the chief requisite for the history of science is intimate scientific knowledge; no amount of philosophic legerdemain can make up for its absence." This is quite right; but we must also remember that the history of science contains many great dramas which are, or should be, of poignant interest to all people of intelligence, because each drama ends in a climax which implies a definite advance of the whole human race. The great discoveries such as those of the Calculus and of Evolution, were really the most important events which have ever happened to humanity, excepting only one or two great moral teachings such as Christianity.

The Discovery of the Calculus

There has probably never been a more important find in the domain of the History of Science than what may be called the recent discovery of the discovery of the Calculus. Every one has heard of the long and sometimes bitter controversy between Newton and Liebnitz and their followers as to which of these great men actually gave us the great Calculus; but we are apparently indebted to Mr. J. M. Child for having ascertained and demonstrated that the honour really belongs to Isaac Barrow. Three years ago, Mr. Child translated and edited Barrow's Geometrical Lectures (*Lectiones Opticæ et Geometricæ*, 1669 and 1670), and while doing so concluded that "Isaac Barrow was the first inventor of the Infinitesimal Calculus; Newton got the main idea of it from Barrow by personal communication; and Leibnitz also was in some measure indebted to Barrow's work . . . from the copy of Barrow's book that he purchased in 1673." The translation, with numerous notes and a description of how the editor made

his discovery, was published in 1916 by the Open Court Publishing Company under the name of *The Geometrical Lectures of Isaac Barrow.*

Barrow was born in 1630; became Professor of Greek at Cambridge thirty years later, and, three years afterwards, Lucasian Professor of Mathematics there, in which capacity he seems to have *taught Newton*, who was only twelve years younger. After five years, in 1669, he resigned this chair to Newton, devoted himself to divinity, and died in 1677 and was buried in Westminster Abbey. He was evidently a man of a singular and wide genius. Mr. Child thinks that he discovered the Calculus before he became Lucasian Professor, and that he came into close contact with Newton in 1664. Certainly he secured Newton in the succession to the Chair, and also left to him and to Collins the publication of his *Lectiones Opticæ* when he himself retired upon divinity.

So far as I can see Mr. Child has proved his point, and it was Barrow who really discovered the fundamental theorems both of the Differential and of the Integral Calculus. But he set them forth geometrically, and it was Newton who first set them forth analytically and who invented the special dotted-letter notation for derived functions. Still later, Leibnitz improved the notation by using dy and dx. I have seen it claimed for him that he also developed some fundamental rules of the Calculus, such as those regarding the differentiation of sums, products, and quotients of functions; but Mr. Child easily shows that Barrow did this, besides differentiating and integrating many "standard forms."

I think that there was a fourth great advance made in the Calculus—in the discovery, apparently by Boole, of the law that the symbols of differentiation and integration can be treated like those of numbers. This can be made into a perfectly rigid "operative algebra" by the use of the symbol of substitution (SCIENCE PROGRESS, October 1915 to April 1916, and October 1918).

Mr. Child's book is so important and interesting that subsequent editions deserve to be written in a less rambling and more carefully considered style. A recent little book, *A First Course in the Calculus*, Part I., by W. P. Milne and G. J. B. Westcott (G. Bell & Sons, 1918) gives the history according to the new light, and includes Barrow's prayer beginning, "How great a Geometrician art Thou, O Lord!"

R. R.

The Exhibition of the British Science Guild

On January 15 the Organising Committee of the British Scientific Products Exhibition of the British Science Guild gave a dinner at the Princes' Restaurant to a number of men of science and leaders of scientific industries. The Marquis of Crewe presided and said that our secondary education still remained rather humane than scientific, though this tendency was in process of correction. It would always be a pleasure to him to recollect that he was the Minister who brought into being the Department of Scientific and Industrial Research. He trusted that the exhibitions of the Guild would become a permanent feature of the life of the country. Lord Sydenham declared that in the past the leaders of science in this country had been pioneers far in advance of the scientists of other countries, but their great work was not followed up in the way it ought to have been, and he cited the cases of helium and of aniline dyes as examples. The Guild hoped to hold another and a larger exhibition this year; and he trusted that science and scientific methods of direction in government, commerce, and industry would help this country to re-create national prosperity in the future. Mr. F. G. Kellaway, Deputy Minister of Munitions, gave some interesting facts

regarding the manner in which our men of science had helped us to repel German air attacks during the war—men whose names were almost unknown to the general public. The British magneto and plug were now the best in the world; and the output of the former in this country had risen from 1,140 in 1914 to 128,637 last year; and of the latter from 5,000 to 182,148,000.

Alan Milne: Some Reminiscences of an Important Movement (Sir R. Ross)

The recent death of Mr. Alan Hay Milne, B.A., C.M.G., formerly Secretary of the Liverpool School of Tropical Medicine (and of the Liverpool Chamber of Commerce), imposes upon me the duty of recording some appreciations of a friend who really took a leading though little recognised part in a movement of considerable Imperial importance—that of the prevention of malaria and the teaching of tropical medicine. I cannot do so better than by giving a very brief narrative of the origin and earlier development of the movement, which owed so much to him.

In 1897–8 I had found in India that the parasites of malaria develop in certain species of mosquitoes which carry them from infected persons and inoculate them into healthy people. This was an event of some little consequence, because malaria is the most ubiquitous disease in the tropics, rendering many of the most fertile areas difficult of development, and killing, it is thought, over a million persons annually in India alone. The logical result ought to have been that all the large Departments of State connected with the tropics should have followed up the investigations with vigour, and have applied them to the saving of human life and health on the large scale. But at that time we were sunk in a state of absolute intellectual sloth, and a discovery of this kind created much less interest in the public mind than the result of a horse-race or cricket-match In 1898-9, however, Mr. Joseph Chamberlain, then at the head of the Colonial Office, did move a little in the matter, at the instance of Dr. (now Sir Patrick) Manson; but instead of compelling the Colonies to maintain a proper organisation for the work, he suggested only that private persons should be patriotic enough to start schools of tropical medicine in various parts of England at their own expense! The result was that two such schools were inaugurated, with the help of small subsidies, in London and Liverpool. Alan Milne was the secretary of the latter, and it was largely owing to his tact and energy that enough money was obtained for the purpose from public subscriptions (see SCIENCE PROGRESS, January 1914, for details) to pay mean salaries to the workers—who have really been the sufferers.

The moving spirit in Liverpool was the late Rubert Boyce, Professor of Pathology at the University College, who persuaded his friend the late Alfred Jones, a shipowner, to provide a nucleus by subscribing £300 a year to the Liverpool School for a few years. Early in 1899, on completing my work in India, I reported to the Indian Government my method of banishing malaria by "mosquito-reduction"—the method subsequently adopted with such great success by the French at Ismailia and by the Americans in Panama, and now shown to be by far the most practicable method for large populations. But no notice was taken of my report, and I could obtain no assurance from the authorities that I should be allowed to continue the malaria work; and I therefore wished to leave the Indian Medical Service, and reached England in March on leave. Boyce now asked me to join his proposed institution, which I did.

Immediately on going to Liverpool I suggested a series of expeditions to West Africa in order to study and prevent the malaria which gave that region the name of the "white man's grave." Boyce and Milne collected the small but necessary

funds, and our first expedition, in 1899, resulted in the detection of the chief malaria-bearing mosquitoes at Sierra Leone, the study of their habits, and the development of mosquito-reduction as a practical sanitary measure. Other expeditions continued similar studies further down the coast. In 1901 Mr. Coats, of Glasgow, gave us £2,000 to provide an object lesson of how to apply mosquito-reduction—which I gave at Sierra Leone, with very little effect on the local intelligence. In 1902 I visited that enlightened colony for the third time, and then showed the Suez Canal Company how to reduce malaria at Ismailia In 1904 I visited Panama at the request of the American Government; in 1906, Greece; and in 1907–8, Mauritius; and we also sent many other expeditions to Africa and America. But I mention all this merely in order to indicate the wide activities of the School, which were rendered possible only or largely by Milne's capacity as secretary. The shipowners and merchants of Liverpool found the money. The colonies themselves, except Mauritius, subscribed little or nothing, looked upon our visits almost as intrusions, and (excepting Lagos under Sir William MacGregor) seldom took our advice. From that day to this I for one have never received a word of thanks from most of them for the time we spent and the money we lost by trying to help them So much for the administrative capacity of the modern Briton—which, as I have always said openly, I have little belief in. We talk grandly of the manner in which we bear "the white man's burden," but when science gives us such a magnificent opportunity for bettering tropical conditions we do not take it—and even omit to pay our doctors' fees! The neglect has probably cost the Empire millions of lives and money.

Jones, being a shipowner, and Boyce, being a professor, were speedily knighted for all these discoveries. Milne received a C.M.G in 1911; but when Jones left a large sum of money to the cause, neither Milne nor, I believe, any of the other workers were given a penny of it; and when he retired some little time ago, the institution which he had so largely helped to create did not even spare him a pension. Of course I am no longer connected with it in any way.

Milne was born in Jamaica in 1869—the son of the Very Reverend A. J. Milne of Fyvie, Moderator of the General Assembly of the Church of Scotland, and his mother was *née* Miss A. L. Hodgson. He was educated at Fettes College in Edinburgh, and at Pembroke College, Cambridge, and was never married. He was a typical secretary—good-looking, genial, diplomatic, suave and smiling, a remorseless extractor of subscriptions, and a brilliant organiser of meetings and dinners. He was the author of some very amusing illustrated rhymes called *Ulysses, or de Rougemont of Troy*; and, of course, an editor of certain business journals and secretary of various bodies in Liverpool. Tropical medicine has often suffered much from the class of person connected with it; but Milne was a well-educated man and a gentleman. He saw, with some humour, through the people with whom he had to deal, but remained loyal to them; and I do not think that he had an enemy in the world. Latterly he had suffered much from ill-health, on account of which he retired in 1917. He died at Paignton on January 21, 1919, and was buried in Fyvie churchyard.

Dead Darwin

We have to present our congratulations to Dr. Charles H. O'Donoghue, recently of University College, London, on being appointed Professor of Zoology in the University of Manitoba. His Inaugural Lecture contains many points of interest. Recently it has been the fashion to decry Darwin as much as possible, and we have even heard professors of ultra-science trying to maintain that Darwin

has been entirely defeated by modern zoological discovery. It is therefore comforting to read that Prof. O'Donoghue is not of this opinion, for he says:

> The starting point of all modern zoological work goes back to the publication of Charles Darwin's famous book on *The Origin of Species* in 1859. It is probably no exaggeration to say that this book had a more profound influence on all branches of thought, and not simply Biology alone, than any other that has appeared since or for some hundreds of years before, and we are proud to think that it was written by a zoologist. Its publication was followed by a storm of discussion, and a great deal of that intellectual friction that tends to generate heat rather than light. This gradually died away, as the main principles it advocated were accepted by all biologists, and then followed a period of activity in research and revolution in ideas such as had not been witnessed since the great revival of learning, at the end of the 15th and beginning of the 16th centuries Whether or not all his interpretations of the causes at work were right does not concern us here so much as the inestimable service he rendered in bringing home the great fact of Evolution, and thereby providing a solid foundation for future endeavour. Zoological and botanical work, previous to this, had resulted in the accumulation of an enormous number of facts and large collections of material, but, as Theseus would have been lost in the Cretan labyrinth without the thread supplied by Ariadne, so would the biologist have been lost in the maze of individual and unrelated observations but for the clue provided by Darwin.

The Mathematical Amœba

In *Science* of December 13 last, Prof. S. O. Mast gives us an admirable critical article on "Problems, Methods, and Results in Behavior," in which he examines and summarises mechanistic and vitalistic opinions on the behaviour of animals. Consider the following case: In 1915 I was examining a number of small amœbæ taken in a pool of fresh water close to our hotel in Alexandria. These beautiful little creatures each lived like a caddis-worm in a house consisting of minute stony particles, and were moving about with their houses catching small diatoms. Presently one protruded a pseudopodium, caught a diatom by the middle, and tried to ingest it. Now, as every one knows, a diatom is a double cone in shape, that is, is shuttle-shaped; and the length of the diatoms in question was almost as great as the diameter of the amœbæ. Hence my poor friend having caught his diatom by the middle could no more swallow it in that position than I could swallow a marrow-bone sideways. What did the amœba do? After thinking out the mathematical problem for a time, it proceeded to turn the diatom until the end of the latter entered its mouth-aperture and was sucked in. But it was still unfortunate, for when nearly half the diatom was drawn in the amœba could not stretch its "mouth" sufficiently to take in the whole width of the diatom in its middle section. It remained thinking for some time with the diatom half in and half out of its mouth, and then wisely gave up the problem, suddenly ejected its intended prey, and went off in an opposite direction to look for another and a smaller victim. First, did the amœba show any evidence of a mathematical knowledge of the shape of a double cone; and, secondly, if amœbæ are attracted only by chemiotaxis, why did not the same amœba return over and over again to the same diatom? In both cases it looks as if amœbæ possess experience, that is, memory of the shapes of diatoms, as well as judgment, that is, the power of refusing to persist in futile endeavour! Will mere chemiotaxis explain what I saw? (*Vide* my hypothesis of the Ego Cell in Science Progress, July 1916.)

Prof. Mast laughs at the "anecdotage" of those who tell stories about the intelligence of animals, but repeats Menault's tale of the drake which dragged a

young lady by the skirt of her dress in order to rescue a duck the head of which had been caught in a sluice-gate. I can tell a similar tale. About 1891 a cat was good enough to keep three small kittens in a bathroom on the ground floor of my house in Bangalore in India One morning when my wife and I were sitting in the veranda, the cat came out to us crying terribly. When we jumped up to see what was the matter, it led us into the bathroom, and there we saw that the kittens in their basket were writhing under a swarm of enormous black ants which literally covered them. I rescued them with difficulty, being obliged to pull off the ants with my surgical forceps—and one of the kittens died shortly afterwards. The behaviour of the cat here was precisely what the behaviour of a human being would have been under the circumstances.

It flatters our vanity to think that we are the only creatures who possess mind; but probably the elements of mind—that is, memory, imagination, and judgment—exist all through animal life even down to unicellular organisms—though I will not pretend that the mind of an infusorian equals that of a politician or of a headmaster. The sooner we disabuse ourselves of the notion that we are gods by nature (which is not true), and try to become gods by science and art (which may be possible, though not by politics and grammar), the better for humanity.

R. R.

How to Pay One's Debts

We were glad to see that India has given some recognition to Sir Leonard Rogers, F.R.S., for his life-long work, chiefly in connection with the treatment of various wide-spread and formerly fatal tropical diseases such as kala-azar and dysentery, by presenting a bust of him to the School of Tropical Medicine in Calcutta last November. But we suppose that even India feels, somewhat uncomfortably, that this reward may not possibly be quite sufficient; and that excellent Calcutta paper, *Capital*, wonders why a Nobel Prize "has not long since been awarded" to him, and suggests that somebody in authority, such as "the Viceroy and the Governor of Bengal," should bring his claims before the Nobel Committee. The name of Sir Leonard Rogers has been before the said Committee for some years; but we fear that the representations of the Viceroy and the Governor of Bengal would not carry as much weight with the Nobel Committee as *Capital* thinks. The Committee might conceivably even hold that a rich Empire like India should pay its own professional benefactors rather than attempt to saddle a small and comparatively poor nation like Sweden with the charge If a patient should give a fee to a doctor for his prescription, so should a country pay him specially for discoveries which apply, not to an individual, but to the whole community. Every year the Government of India spends large sums on pensions and high salaries for politicians, civilians, and judges whose services are small compared with those of Sir Leonard Rogers. If, instead of troubling Sweden, the Viceroy and the Governor of Bengal were really to try to think out the matter for themselves, they might do some good. Why is it that work such as that of Rogers is not done much more frequently? For the simple reason that it is the form of medical work which, though of the highest value to the world, is the most unprofitable or even ruinous to the man who undertakes it. Pathologists and sanitarians are the most important, but the worst paid, of all members of the medical profession; and the more capable young men now fully recognise the fact and "are not such fools" as to touch these subjects. We have urged this matter over and over again; and the public will do well, in its own interests, to look into it more carefully.

The Society of British Science Students

We have received a leaflet from this Society setting forth its objects and aims, which are principally to strengthen the relations between the younger students of science in this country by means of meetings, lectures, and publications, and to help those students who are in any way unfortunately placed in life, and those members in the Forces whose studies have been retarded by the war. It is an essential part of the scheme that all sciences should be represented in the Society. Those interested in the furthering of this idea should apply for details to the Hon. Gen. Secretary, Mr. P. E. Owens, 25, Jesse Terrace, Castle Hill, Reading.

Notes and News (D. O. W.)

The following candidates have been selected by the Council of the Royal Society to be recommended for election into the Society: Dr. F. A. Bainbridge (Professor of Physiology, University of Durham College of Medicine); Dr. G. Barger (Research Chemist under the Medical Research Committee); Dr. S. Chapman (Chief Assistant at the Royal Observatory, Greenwich); Sir C. F. Close (Director-General of the Ordnance Survey); Dr. J. W. Evans (Lecturer in Petrology at the Imperial College of Science and Technology); Sir Maurice Fitzmaurice (Civil Engineer); Dr. G. S. Graham-Smith (University Lecturer in Hygiene, Cambridge); Mr. E. Heron-Allen (Protozoologist); Dr. W. D. Matthew (Curator of Vertebrate Palæontology, American Museum of Natural History, New York); Dr. C. G. Seligman (Professor of Ethnology in the University of London); Dr. B. D. Steele (Professor of Chemistry, University of Queensland); Major G. I. Taylor (Staff Officer in charge of Meteorological Research, R.A.F.); Dr. G. N. Watson (Professor of Mathematics, University of Birmingham); Dr. J. C. Willis (late Director of Botanical Gardens, Rio); Prof. T. B. Wood (Professor of Agriculture in the University of Cambridge).

The Honours List published on New Year's day contained the following names of interest here:

K.C.V.O.: Sir G. A. Critchett, Surgeon Oculist to the King; Dr. H. H. Hayden. Director of the Geological Survey of India.

C.B.: Mr. C. E. Ashford, Headmaster, Royal Naval College, Dartmouth.

The appointments to the Order of the British Empire included the following:

K.B.E.: Prof. W. J. Pope, F.R.S. (Member of the Chemical Warfare Dept. M. of M.; Professor of Chemistry, Cambridge University); Aubrey Strahan, F.R.S. (Director of the Geological Survey of Great Britain).

C.B.E.: Prof. J. W. Cobb (Livesey Professor of Coal, Gas, and Fuel Industries, Leeds University); H. H. Dale, M.D., F.R.S.; J. C. M. Garnett (Principal, Municipal College of Technology, Manchester); Capt. P. Chalmers Mitchell, F.R.S.; T. A. Ferris (Mathematical Instrument Office, Calcutta).

O.B.E.: A. W. Borthwick, D.Sc. (Forestry Advisory Officer, Board of Agriculture for Scotland); G. T. Chivers (Headmaster, Dockyard School, Portsmouth); R. Corless (Superintendent of Instruments, Meteorological Office); F. Holt (Chemist to the Castner-Kellner Alkali Co); W. Foord-Kelcey (Professor of Mathematics and Mechanics, Royal Military Academy); A. O. Rankine, D.Sc. (Chief Research Assistant, Harwich); J. E. K. Studd (President, Regent Street Polytechnic); W. E. S. Turner, D Sc. (Head of Dept. of Glass Technology, Sheffield); Capt. E. Maclagan-Wedderburn, D.Sc. (Experimental Dept., M. of M.); Mrs. M. L Davys (Laboratory Assistant, Kasauli, Punjab).

M.B.E.: Lt. B. V. Gander (Demonstrator, Sheffield University Discharged

Officers' Training School); E. L. Pickles (Chief Examiner to the Air Inventions Committee); Major F. E. Pollard (Sub-Section Director, Technical Dept. Aircraft Production); A. Rule, D Sc. (late Superintendent, H.M. Wood Distillation Factory); J Savage (Chemist to the Castner-Kellner Alkali Co.); A. E. Shorter (Senior Assistant Inspector, Munitions Areas, Sheffield); A. Stevenson (Chief Chemical and Technical Assistant in the Optical Munitions Dept.); W. H. Weddingham (Chief Designer, Elswick Ordnance Works); A. G. Williams (Principal Observer, Optics Division, N.P.L).

Knight: Prof. G. D. Thane, Principal Inspector under the Cruelty to Animals Act, and Professor of Anatomy at University College, London; Col. Sir Almroth E. Wright, A.M S.

Marshal Foch, Sir Douglas Haig, and Admiral Jellicoe have been elected to the roll of distinguished honorary members of the Institution of Civil Engineers.

Sir Napier Shaw has been elected a foreign member of the Reale Accademia dei Lincei of Rome.

The L. La Caze prize for Physics of the Paris Academy of Sciences for 1918 was awarded to Aimé Cotton for his researches in magneto-optics; the Clément-Felix prize to Paul Langevin for his work on electrical resonance; the Cuvier prize to Dr. A. Smith Woodward for his work on fossil vertebrates.

The Swiney prize of the Royal Society of Arts for 1919 has been awarded to Dr. Charles Mercier for his work *Crime and Criminals.*

The President's gold medal of the Society of Engineers has been awarded to Mr. T. Roland Wollaston of Manchester for his paper entitled "A Survey of the Power By-Product Problem."

The council of the Geological Society has this year made the following awards: Woollaston medal, Sir Aubrey Strahan (Director of H.M. Geological Survey); Murchison medal, Miss Gertrude L. Elles (Newnham College, Cambridge); Lyell medal, Dr. W. F. Hume (Director of the Geological Survey of Egypt); Bigsby medal, Sir Douglas Mawson; Woollaston fund, Dr. Alexander Logie Du Toit (Geological Survey of South Africa); Murchison fund, Mrs. Eleanor M. Reid; and Lyell fund, Mr. John Pringle (Geological Survey of England and Wales) and Dr. Stanley Smith (University College, Aberystwyth).

Mr. Vilhjalmur Stefansson has been awarded the gold medal of the American Geographical Society.

Sir Everard im Thurn is the newly elected President of the Royal Anthropological Society.

Dr. C. G. Knott has been elected President and Prof. Hudson Beare and Mr. J. Mackay Bernard Vice-Presidents of the Scottish Meteorological Society.

The British Association for the Advancement of Science will resume its annual meetings this year at Bournemouth.

During the last quarter influenza has taken a heavy toll from those engaged in scientific work and especially from the younger generation. Among those who have passed away are the following: W. Allingham (Principal Assistant in the Marine Dept. at the Meteorological Office); Prof. Gustave Bouchardat (of the Paris School of Pharmacy, well known for his work on synthetic rubber); A. C. P. de Candolle (the Genevan botanist); Dr. G. S. Corstorphine (Principal of the South African School of Mines, Johannesburg); Marcel Deprez (the electrician); J. H. Dibb (of the Mathematical Dept. University College, London); Prof. G. Carey Foster (Emeritus Professor of Physics, University College, London); Dr. Leonard G. Guthrie (secretary and vice-president of the section on Medical History at the Royal Society of Medicine); Lt.-Col. Longstaff, F.R.G.S. (whose generosity contributed

so largely to the success of the first Scott Antarctic Expedition); M. Luizet (Assistant at the Lyons Observatory); Gaston Milhaud (Professor of Mathematics at the Sorbonne); Prof. E. C. Pickering (Director of the Astronomical University, Harvard); W. L. Preece (son of Sir W. Preece); Col. Theodore Roosevelt; J. McGarvie Smith (of N.S.W. who, among his many other activities, produced the anthrax vaccine which has been used successfully on twenty-five million sheep in Australia); Dr. W. G. Smith (Combe Lecturer in general and experimental psychology in the University of Edinburgh); Dr. Charles R. Van Hise (of the U.S. Geological Survey); Dr. W. Marshall Watts (of Index of Spectra fame).

We have been requested to announce that owing to the retirement of Mr. Wallace Goold Levison, Dr. Edgar T. Wherry, Bureau of Chemistry, Washington, D.C., will assume the duties of Editor-in-chief of the *American Mineralogist* with the following associate editors: George F. Kunz, President, New York Mineralogical Club; Herbert P. Whitlock, American Museum of Natural History; Alexander H. Phillips, Princeton University; Waldemar T. Schaller, U.S. Geological Survey; Edward H. Kraus, University of Michigan; Austin F. Rogers, Leland Stanford Junior University; Thomas L. Walker, University of Toronto, Canada; and Samuel G. Gordon, Academy of Natural Sciences, Philadelphia.

The Aero Club of America announces that an aeroplane expedition is to start next June to explore the Arctic regions and fly, if possible, over the North Pole. The expedition will be known as the Roosevelt expedition, and any new land that may be discovered will be known as Roosevelt Land. It was through Colonel Roosevelt's intervention that Admiral Peary received the leave of absence which enabled him to discover the North Pole.

Two expeditions are being organised in this country for taking observations of the solar eclipse on May 29th. One party (in charge of Messrs. Commelin and Davidson) will be stationed at Sobral in Ceara, Brazil, and the other (in charge of Prof. Eddington and Mr. Cottingham) on the island of Principe, 110 miles from the West Coast of Africa. This eclipse is noteworthy for the long duration of totality (5 m. 13 s. at each of the observing stations) and the richness of the field of stars round the sun. The observers will concentrate their whole attention on the photography of this star field in the hope that data may be obtained for testing Einstein's relativity theory.

The Smithsonian Institute has sent out an expedition to Calama, a town near the nitrate desert in Chile, for the purpose of obtaining determinations of the solar constant under the best possible conditions over a period of some years. The locality was chosen as being the most cloudless region of the earth readily accessible, though, since its arrival, the expedition has found that there is a considerable haze which will form a serious obstacle to the investigation of solar variations, these requiring an accuracy of at least 1 per cent. The two observers (Messrs. A. F. Moore and L. H. Abbot) sent out have been equipped with very complete outfits for spectro-bolometric, pyrheliometric, and meteorological work. The linear bolometer is enclosed in a vacuum, and its indications, as measured by a sensitive galvanometer, are recorded photographically on a moving plate which travels proportionately to the movement of the spectrum over the bolometer, the time required for a complete curve being eight minutes. The average value of the constant so far obtained is 1·951 calories per sq. cm. per minute.

The Conjoint Board of Scientific Societies has commenced the publication of a Bulletin containing a diary of forthcoming meetings of scientific and technical societies. The issue dated January 27, was divided into three sections: (1) Council meetings during the period January 27—April 16; (2) Society meetings from

January 27—February 8, with titles of papers, lectures, etc., so far as they were known; (3) Society meetings, February 10--April 15, with no details. It must be admitted that, in the absence of abstracts of the papers to be read, the Bulletin hardly justifies its existence. A mere diary can be found in *Nature*, and in the various technical journals. Something far more useful is provided by the Bibliographic Service of the Wistar Institute of Anatomy and Biology, Philadelphia, Pa. This Institute apparently controls eight of the leading periodicals dealing with these subjects in the United States. Each paper accepted for publication in them is accompanied by the author's abstract (not exceeding 250 words) and a list of subject headings under which the various sections of the work are treated. Upon receipt at the Wistar Institute, the abstract is printed immediately on one side of a standard library card. On the reverse side of the card is given the complete bibliographic reference to the paper as it will appear eventually, the various subject headings following the main title. Copies of this abstract bibliographic card are distributed at once to the anatomists, zoologists, university and college libraries, research institutes, public libraries, academies and scientific societies of the world. The service is available to private subscribers for $3 per annum. If the Conjoint Board were to initiate something on similar lines, suitably classified as to subject-matter and at an equally reasonable cost, it would be doing something much more worthy of its opportunities and influence in the scientific world.

An attempt is being made by past and present students of the Imperial College of Science and Technology to induce the Governors of the college to take steps to raise its status to that of a Technological University able to grant its own degrees in science and technology. There does not seem to be any valid reason why the College should not co-operate with the departments of applied science in the other schools of the University of London in creating a faculty of technology in the University. Such a scheme would retain the valuable diplomas already awarded by the Imperial College and at the same time enhance the value of the degree in technology.

A Research Board has heen appointed by the Department of Scientific and Industrial Research and the Medical Research Committee, jointly, to consider and investigate the relations of hours of labour and of other conditions of employment, including methods of work, to the production of fatigue, having regard both to industry, efficiency, and to the preservation of health among the workers. Prof. C. S. Sherrington, F.R.S. (Professor of Physiology at Oxford), has been appointed Chairman of the Board.

A Federal Council for pure and applied chemistry has been established to "advance, safeguard, and voice the interests of chemical science, to secure the co-ordination of future effort, and to consolidate the position chemists have won during the war." It consists of representatives appointed by the following constituent bodies: Chemical Society, Society of Chemical Industry, Association of British Chemical Manufacturers, Institute of Chemistry, Society of Public Analysts, Faraday Society, Biochemical Society, Institute of Brewing, Society of Dyers and Colourists, Society of Glass Technology, and the Ceramic Society. Sir William J. Pope has been elected first Chairman of the Council, and Prof. H. E. Armstrong Honorary Secretary. Among the immediate proposals of the Council is the provision of an adequate building for the use of the several associated societies and the formation of a complete chemical library.

It now appears to be definitely decided that an Institute of Physics will be formed as the result of the combined action of the Physical, Optical, and Faraday

Societies. Regulations for the control of the Institute have been drafted by a joint committee representing these bodies, but, at the time of writing, they had not been approved for publication.

The Senate of Cambridge University has at last abolished compulsory Greek from the syllabus of the Previous Examination. Candidates have now a choice of four languages: French, Greek, Italian, or Spanish. Latin is still compulsory, so that the unfortunate science student, who *must* know French and German, is still severely handicapped.

At a joint meeting of the Headmasters' Conference and the Incorporated Association of Head Masters it was resolved: (1) That suitable instruction in natural science should be included in the curricula of preparatory schools, of the upper standards of elementary schools, and of all boys in public and other secondary schools up to the age of about sixteen. (2) That mathematics and natural science should be necessary subjects in the entrance scholarship examinations of public schools, in the first school examination, and in the examinations for entrance into the Navy and Army, provided that good work in other subjects should compensate for weakness in mathematics and natural science, and *vice versâ*.

Science states that the Medical College in Pekin, which is being built for the Rockefeller Foundation at a cost of $6,000,000, will be opened in 1920. The college includes eighteen university buildings, forty faculty residences, and a hospital with 200 beds. A second medical school is to be established in Shanghai, and subsidiary medical stations are to be maintained in different parts of China. Subsidies will be granted to existing missionary hospitals which will be standardised and will offer internships for the University. The over-all cost is expected to be $10,000,000 and the annual up-keep $250,000 - 500,000.

In his address as retiring President of the American Association for the Advancement of Science Prof. T. W. Richards gave, last December, a most interesting account of the work on radioactive lead which has been carried out in the last few years, more particularly at Harvard University. The results which have been obtained may be summarised thus: (1) The atomic weight of common lead is 207·19, and of the purest uranic lead 206·08, a figure which is in remarkable agreement with that deduced from the atomic weights of Uranium (238·18 − 8 atoms of Helium = 206·18), and of Radium (225·96 − 5 atoms of Helium = 205·96; mean 206·07). (2) The atomic volumes of the two varieties are the same; the densities being proportional to the atomic weights. (3) The spectra are identical—except for a minute shift equal to 0·0001 per cent. of the wave length detected by Prof. Harkins of Chicago in one of the lines. (4) The refractive indices of the salts are the same, which would indicate that atomic volume rather than density is important in determining the refractive index. (5) The solubilities are the same; one thousand fractional crystallisation of Australian lead nitrate, which contains both varieties, having failed to produce any measurable separation. (6) The thermoelectric effects are the same; wires made of ordinary and uranic lead giving no measurable thermoelectric effect. (7) The melting points are the same. Finally, in the hope of determining whether ordinary lead (which has the same atomic weight from whatever part of the earth it originates) is itself a mixture, experiments on fractional diffusion are being conducted at Harvard. This work is not yet sufficiently advanced for any conclusions to be drawn. The address is published in full in *Science*, January 3, 1919.

The *Scientific Australian* (September 1918) contained an abstract of a paper read by Mr. J. H. Maiden, F.R.S., before the Royal Society of New South Wales,

dealing with the early history of scientific societies in the Commonwealth. It appears that the earliest recorded effort to form an improvement society was made in the year 1818, when Judge Advocate Wylde's attempt to form an Agricultural Society failed because Governor Macquarie demanded the admission of emancipists. In December 1821, Governor Brisbane formed a scientific club under the grandiloquent name of the Philosophical Society of Australasia, whose members read papers at each other's houses in turn. Some of the papers were printed by Barron Field, while the bronze plate at Kurnell at once celebrates the foundation of this Society and the jubilee of Captain Cook's visit. This was succeeded by the Agricultural Society in the following year, which became a Horticultural Society in 1826, and it is noteworthy that Sydney has never been without a Horticultural Society from that day to this. Mr. Maiden considers that the Royal Society of N.S.W. is a lineal descendant of the Australian Philosophical Society founded in January 1850 and resuscitated in July 1855 as the Philosophical Society of N.S.W.; the present title being assumed in December 1866. This would make the Royal Society of Tasmania, dating from October 1843, the oldest scientific society in Australasia.

Messrs. Hilger & Co. have issued a short brochure descriptive of the new and compact type of Abbe refractometer designed and constructed by them. With this instrument refractive indices from 1·3 to 1·7 can be rapidly measured to an accuracy of 0·0001, while at the same time the dispersion can also be obtained. The purity of oils and other substances, and the strengths of aqueous, alcoholic and other solutions, can be determined with only the one or two drops of liquid required to form a thin film between the two dense flint glass Abbe prisms. To increase the usefulness of the refractometer for industrial purposes Messrs. Hilger are publishing tables of refractive indices of substances of industrial importance. Vol. I., *Refractive Indices of Essential Oils*, is ready, and Vol II., on *Oils, Fats, and Waxes*, will shortly be in the press. The price of the refractometer, whose parts are all interchangeable, is £47, while the lists of refractive indices are issued at 15*s*. per volume.

The report of the Organising Committee of the Scientific Products Exhibition held last summer at King's College has now been issued by the British Science Guild. It appears that the whole cost of the exhibition, some £4,000, was met by admission fees and by donations from exhibitors and members of the Guild. These were more than sufficient, for when the outstanding liabilities have been settled there should remain a balance of about £1,500, which it is proposed to use as the nucleus of a fund to cover the expenses of a second and still more comprehensive exhibition this year. No financial assistance was received from the State, though two or three individual departments gave such help as they were able. Thus the Air Ministry, through Brig.-General Bagnall-Wild, undertook the complete arrangement of the aircraft section, the Inventions Department of the Ministry of Munitions gave demonstrations of the oxidation of ammonia, and the Board of Trade loaned show cases and gave advice. Mr. J. H. Reynolds, formerly Principal of the Municipal School of Technology, Manchester, was responsible for the first draft of the exhibition scheme; Mr. F. S. Spiers, Secretary of the Faraday Society, acted as Executive Officer; and Mr. F. C. Higham gave his services as Director of Publicity.

At a dinner of the Guild on Thursday, January 9, Lord Crewe, the president of the exhibition committee, laid further stress on the objects the Guild has in view—namely, to emphasise the importance of science as part of our general scheme of education, and to assist in the promotion of research in both pure and

applied science. Lord Sydenham referred to the manner in which our country had permitted itself to become dependent on Germany for the products of our own discoveries and raw materials, but was perhaps hardly fair in his remark that while "helium gas had been discovered in this country it had been left to America to show that it could be produced in quantities which might enable the airship to become a great weapon of war." In the first place, it is understood that this helium is produced from certain of the vast stores of natural gas which exist in America and not in England, and, in the second place, even on a war basis, the cost of a large airship filled with the gas would be almost prohibitive.

The report of the discussion on the Teaching of Physics in Schools, organised by the Physical Society last June, serves once more to focus attention on the necessity for the revision of the methods of teaching science at present in vogue if the science course is to be of real value in our scheme of general education. Present-day courses are, for the most part, based on the syllabus drawn up in 1895 by a Committee of the Incorporated Association of Head Masters. This syllabus was adopted as a basis for the Oxford and Cambridge Local Examinations, and practically all other examinations of a like standard throughout the country. As is well known, it lays overwhelming stress on laboratory work of a quantitative character which requires a very large proportion of the time allotted to physics for its execution. It ties the teacher down in his descriptive lessons to what may best be described as the grammar of the subject, and never allows the unfortunate learner a glimpse of the wonderland beyond. Moreover it sends him forth from school without any knowledge of the common scientific things around him. There is a general consensus of opinion that this state of affairs must be altered, and its alteration becomes the more urgently necessary now that science is being made a compulsory part of the school curriculum. Further, the principles which must govern the new methods are not in dispute. Up to the age of sixteen the science course should be general and descriptive, the laboratory work taking a smaller (but not less essential) proportion of the time. The syllabus should be on the lines indicated in the "Science for All" scheme drawn up by the Association of Public School Science Masters: the practical work for the most part qualitative, more especially in the earlier stages. The First or General Schools Examination would terminate this part of the work and, for the majority, the school career as well. Those who intend carrying their science further must then set to and learn the fundamentals of the subjects they propose to study, be they "drudgery" or not. A year so spent should bring the standard up to that of the Matriculation Examination. It seems to need to be emphasised that this examination is intended as an entrance to a university course; its syllabus is, therefore, properly drawn up by the university authorities, who do "know what future work is required of the student," and rightly demand a sound knowledge of the fundamental principles of science. These are not essential in an examination intended to hallmark the close of a school career. At present the attainment of a certain standard at the General Schools Examination serves as an exemption from matriculation (at least at the University of London). This could no longer be the case, as far as science subjects are concerned, if the plan advocated above was adopted; but partial exemption could be granted if it were considered to be desirable. In any case, we are at a parting of the ways; the general course which alone is suitable for all is quite inadequate for the intending specialist. Finally, it may be pointed out that statistics show that the average age at matriculation is about seventeen years, so that, on the whole, no time would be lost by the separation of the two examinations.

Reference has already been made in these pages to the unsatisfactory position of the members of the non-professorial staffs at the universities and university colleges in this country. This position has been aggravated, not only by the enormous increase in the cost of living during the last three years, but also by the facts that the Thomson report fails to recommend any reasonable amelioration of it, and that university teachers have been deliberately excluded from the benefits of the Teachers' Superannuation Bill. As an outcome of these circumstances an attempt has been made during the past year to form an Association of University Lecturers for the purpose of bringing pressure on the Government and the authorities directly concerned. To those familiar with university life, the need for such a movement has been evident for many years. With the modern development of the university, especially on the science side, the number of lecturers has greatly increased and their chances of obtaining a professorial post have correspondingly diminished. In consequence many of them, after spending the best years of their lives in the university, have been compelled to migrate into other spheres in order to earn a livelihood. That this statement is in no way an exaggeration may be seen from the fact that the average salary of 330 lecturers in fifteen colleges and university colleges is £206, and of these only thirty-seven per cent have salaries over £200. Out of this truly màgnificent sum the unhappy recipient is expected to save for his old age, for only those in receipt of more than £200 are (in the ordinary course) admitted to the very unsatisfactory contributory superannuation scheme. The initial steps towards the formation of the association were taken in the summer of 1917 by the members of the staff of Liverpool University. They invited representatives from other universities to come and discuss matters with them, and at the first meeting practically every college of university rank was represented. As a result of the meeting a conference of university lecturers was formed and the foundations of the new association have since been laid. Its basis has been broadened so as not to exclude any branch of the teaching staff of a university, and it is hoped and confidently expected that before the end of the year the new body will be a living force competent to improve and watch over the interests of the university teacher. Its claims have been strengthened by the scale of salaries granted to the staff at the National Physical Laboratory now that that institution has been taken over by the Government. This scale starts at £175 for the junior assistant fresh from college, and rises by six stages to £1,000, the limit for superintendents; each stage rising by suitable increments to its own maximum. Further, a similar scale, rising to £750, has been approved by the Councils of the Scotch Universities, but this, at present, is held up by the University Courts owing to lack of funds—the Government grants being altogether inadequate. At a time when labour is making such extravagant demands, and when living expenses are in consequence mounting higher and higher, the injustice of the present position is cruel and glaring, and it is to be hoped that alleviation may come without the need of recourse to methods which would be deprecated by none more than those forced to employ them.

ESSAYS

MATHEMATICS IN AN ENCYCLOPÆDIA (Philip E. B. Jourdain, M.A.)

A DISCUSSION of the nature and extent of articles in a proposed mathematica dictionary has lately excited a good deal of interest in America. The Mathematical Association of America has appointed a committee to investigate the subject, and Dr. G. A. Miller (*Amer. Math. Monthly*, 1918, **25**, 383 7; cf. 428) has given an example of a proposed article dealing with the theory of groups. This article has the conventional characteristics of an article for a dictionary. This is not meant to imply that the article is not a good one of its kind: it is, in fact, a very thorough and exhaustive treatment of the things about which it means to give information. But it seems to me that what it and most other dictionary articles discuss is exactly what nobody really wants who is not bent on acquiring merely the sort of knowledge that is required by examinations. Nearly all of Dr. Miller's specimen article is devoted to definitions of "groups" in the general technical meaning of the word, and particular qualities of groups. It is no uncommon thing to read in an examination paper such a question as: "Define the terms *isomorphism* and *transitivity*"; but such information, at least in this form, is not required outside the examination room. Surely, an intelligent being who consults an article on a science in an encyclopædia does so from a wish to know what such-and-such a department of knowledge is about and what has been done in it, whether he does so for cultivating his own mind, or as a preliminary for cultivating the minds of others, or for his own work of discovery. He does not want simply to fill his memory with what certain words mean in the technical language of the present: his aim would be to get a firm grasp of the principles underlying that particular subject, and to find out why certain notions were so important as to be fixed by a name—such as "isomorphism," for instance. It is not the formal *definitions* that we must seek in the first place: it is those more or less vague ideas which have lost part of their vagueness so as to become apparently definable. Of course we can never be quite certain that the definitions we may arrive at in some science at a particular time are quite free from vagueness: a mathematician who is nterested in the principles of his subject can find many instances of "definitions" which were seriously given only a few years ago and which can now be seen not to define at all. The theory that definitions should form the subject-matter of articles in a dictionary or encyclopædia, and a great part of the subject-matter of text-books, is a theory held by those schoolmasters who think that examinations are the goal of knowledge; and we must always remember that professors are only a better class of schoolmasters.

Accordingly, we find other characteristics of text-books in encyclopædia articles written by or under the inspiration of such pedagogues. These articles very often, if they contain anything but an incomplete or complete enumeration of pseudo-definitions, are simply condensed text-books: the "bookwork" being preserved in an abridged form and the "examples" omitted. Further, there is an almost total neglect of the fact that a science is a living and growing organism;

that its conceptions emerge rom a vague state into a state in which their logical character is more and more clearly defined, but in which development there is no last stage. Some logicians think that they have discovered the last stage in certain sciences, while nearly all writers of text-books strive to strike what they consider a happy medium between a logically accurate exposition and the sort of propositions which they imagine—sometimes on good grounds—that students find it easy or beneficial to assimilate. A few writers of text-books have gone so far as to mould their expositions on historical lines; but few have the courage to lead a student along the long path of the historical development of an idea, and reach brevity by substituting their own notions of how such-and-such a stage of development might be reached They are like those would-be gods who acknowledge, perhaps even explicitly in a footnote, that things *were* not so, but simply that if they had the ordering of history things might have fallen out as they describe.

These two qualities of schoolmasters can be found in almost every article on mathematics in the *Encyclopædia Britannica*, for example. One other characteristic of most text-books is the deplorable way in which fundamental questions are treated. One can imagine the sigh of relief that an author of one of these books must give when he can at last give himself freely up to technical work; when he can leave the discussion of such things as integers and irrational numbers and functions in general and write about Fourier's series or Bessel functions or asymptotic solutions. No philosophical or even literary training is then required: it is hardly necessary to use any other language than "we have," and then a line of symbols; "consider," and then a mention of what we are to consider; then more symbols; and so on for a few hundred pages. But this avoidance of questions of principle cannot altogether be urged against the article MATHEMATICS in the eleventh edition of the *Encyclopædia Britannica* (1911, **17**, 878-83). This article will now be analysed.

If any one should want to find out how the mathematical articles in the *Encyclopædia Britannica* are arranged, he would naturally turn to the article MATHEMATICS. There would be nothing arbitrary in this selection: we need not suppose that he, like the three sisters about whom the Dormouse told Alice, is interested primarily only in things which begin with an M "such as mouse-traps, and the moon, and memory, and muchness." The article in question seems to be the central, co-ordinating one of all the mathematical articles in the *Encyclopædia*.

It begins by referring to what is said to be the traditional definition of mathematics as "the science of quantity," and gives reasons for rejecting as inadequate such a definition. The idea embodied in this definition seems, by the way, to have been held by the late Prof. G. Chrystal when he wrote the article MATHEMATICS in the ninth edition of the *Encyclopædia Britannica*. These reasons are afforded by Mr. Bertrand Russell's treatment of questions at the foundations of mathematics, and the result is that "there is now no option but to employ 'mathematics' in the general sense of the 'science concerned with the logical deduction of consequences from the general premisses of all reasoning'" (p. 880). It is not necessary to do more than mention the uncritical attitude of the article towards such things as the definitions of "real" numbers and integers which were professedly defined by Russell by the same principle and yet which were actually defined by him by different principles, or the reference (p. 880) to the article NUMBER as giving the arithmetic of infinite numbers, although it is explicitly not dealt with in that article. These defects are merely small incompetences and incorrectnesses; larger incompetencies than these will also be found to annoy a reader.

In the article MATHEMATICS we find what we would naturally look for, namely a "Synopsis of Existing Developments of Mathematics" (p 882) containing a general account of what articles are to be consulted for information on any particular branch of pure or applied mathematics. But the method adopted in this synopsis is thoroughly unsatisfactory. The classification adopted in the Royal Society's *International Catalogue of Scientific Literature* is, after some disparaging remarks, such as that "it would be unfair to criticise it [the classification] from an exacting philosophical point of view," laid down as fundamental, the headings are copied out in a column and a half of matter, and those articles of the *Encyclopædia* which seemed to the copyist, on a casual inspection, to be related closely to those headings are enumerated. The mention of the articles of the *Encyclopædia* made here can only have been due to a casual inspection, because some of the articles, such as INFINITESIMAL CALCULUS and NUMBER, are not mentioned where they palpably ought to be. Any one who has even a slight acquaintance with what these articles contain would see this. Further, there are omissions and inexact references which point to careless proof-reading. But the most serious omission is the omission to draw the editor's attention to the lack of any article in the *Encyclopædia* which deals with functional equations and operations or the "functional calculus." This subject is one in which a new and great conception was introduced into fairly modern mathematics—namely, that of a calculus of functional *operations* instead of real or complex *numbers*—and it could not be urged that this calculus came into importance only after the eleventh edition of the *Encyclopædia* was planned. The solution of functional equations occupied a fairly large part in British mathematical literature during the first half of the nineteenth century, and was mentioned at the end of the late Prof. Cayley's article FUNCTION in the ninth edition of the *Encyclopædia Britannica*; but afterwards was put into the background by the rapidly growing importance of the theory of functions of a complex variable, and only fairly recently have these researches stepped into the foreground once more. To a mathematician, the mention of the names of Volterra, Fréchet, Hilbert, Pincherle, and Hadamard is enough to indicate the subject.

It is difficult to understand why the author of the article MATHEMATICS could have failed to notice the absence of such an important article, since Frege, whose splendid logical work is mentioned by him, pointed out quite clearly (*Function und Begriff*, Jena, 1891, p. 31), but in rather different words, the place that a calculus of functions must occupy as an important stage in the building of mathematics. It seems to be a neglect of duty on the part of the author of the article MATHEMATICS not to have pointed out to the editor of the *Encyclopædia* this notable and unfortunate omission, and not to have insisted on the gap being filled.

The last part of the article MATHEMATICS is devoted to "The History of Mathematics" (pp. 882-3). If, as the author decided, "mathematics" is to be defined as "the science concerned with logical deduction from the premisses of all reasoning," and "the science . . ." here means, as it probably does—since it does with Russell—"the class of propositions . . .," it is difficult to see how "mathematics" can have a history at all. The number 2 does not have a history: though it is quite possible that there may be a history of our *discovery* of the number 2. Thus, in this article, the author commits the elementary blunder of speaking of two things by the same name: a class of logical propositions, and our process of discovery of these propositions. Even if we suppose that necessary explanations of this somewhat obscure procedure are added, the brief and unsatisfactory sketch of history, which begins with a platitude and a false statement: "the history of mathematics is, in the main, the history of its various branches. A short account

of the history of each branch will be found in connection with the article which deals with it," contains at least one more fearful blunder. "The medieval Arabians," we read (p 882), "invented our system of numeration and developed algebra." The origin of this blunder was probably the statement in Mr. W. W. Rouse Ball's *Short History of Mathematics* (4th edition, London, 1908, p 186) that men of science "had become acquainted with the Arabic system by the middle of the thirteenth century." Two pages before this it is explicitly explained by Mr. Ball that our numerals were used in India in the eighth or earlier centuries and were introduced into Europe through the Arabs: the Arabs certainly did not invent the system: they only gave it their name. The same account could easily have been verified from the article NUMERAL in either the ninth or eleventh edition of the *Encyclopædia Britannica*: the theory of the Greek origin of our numerals could hardly be expected not to have escaped our author; it had been published only about four years before he wrote his article.

The article MATHEMATICS is brought to an end by half a column of "Bibliography" (p. 883). In this short collection of books there are at least three mistakes in spelling, not counting the spelling "Leibnitz," which is, I believe, due to the editorial committee, but counting as mistakes the same spelling when the title of a book is quoted in which there is the spelling "Leibniz." There are also two faulty references. Among the books quoted on the history of mathematics, there is a reference to Moritz Cantor's *Geschichte*. Nobody who knows anything of modern historical research and who has read this book could possibly describe it as "the one modern and complete source of information." This description is made still more extraordinary by the fact that only the first edition of the book, which has long been out of date, is mentioned. If, on the other hand, we are to understand "modern" as referring to the period covered by the work in question, a work which the author carried only up to 1758, and which others continued up to 1799, can hardly be described as "modern."

I will add a few words about other mathematical defects in the *Encyclopædia Britannica*—most of the rest of which is so admirably written and edited. In the ninth and tenth editions of the *Encyclopædia Britannica*, there was no biography of Cauchy—one of the founders of the theory of functions of a complex variable—although there are biographies of men like Poisson and Landen, who were of far less importance mathematically and of no greater importance otherwise. Further, in the tenth edition (1902, **31**, 285), it was actually asserted that the third class of Cantor's transfinite ordinal numbers begins with ω^{ω}, and the third and higher classes are within what Cantor quite clearly defined as the second class. A precisely analogous mistake would be to say that, among the finite integers, there are some that are infinite. Now, it seemed to me that these two errors should not be allowed to contaminate a source of knowledge,—which is what the *Encyclopædia Britannica* may be for many,—and thus I wrote, about 1905, to the then editor, Mr. Hugh Chisholm, pointing them out. Whether or no it was in consequence of this letter—and it is hardly necessary to add that the errors are so obvious to any competent mathematician that I can well believe that my letter was superfluous—in the eleventh edition there was (1910, **5**, 555) a biography of Cauchy, and, though the author of the article NUMBER reproduced much of his article on the same subject in the tenth edition, among other alterations omitted (cf. 1911, **19**, 850–51) the sentence criticised above—but, it may be added, he ludicrously enough retained his "scheme of symbols" in which the fallacious divisions into higher number-classes were marked out.

THE GENERAL THEORY OF RELATIVITY AND EINSTEIN'S THEORY OF GRAVITATION (G. W. de Tunzelman, B.Sc.)

THE amount of the Fitzgerald-Lorentz contraction, dealt with in my preliminary article, is defined by the condition $L_0 = L_v\sqrt{1-\beta^2}$, where L_0 is a length measured in a body at rest in the ether, L_v the contracted length of L_0 when the body moves with a uniform velocity v in the direction in which the length is measured, c is the velocity of light relative to the ether, and β is written for the ratio v/c. For $v=c$, $L_v=0$, so that the body will be spread out into a flat sheet of vanishing thickness in a plane transverse to the direction of motion; therefore no material substance can actually attain a velocity as great as c. This agrees with observations on electrons moving at velocities up to about 96 per cent. of c, the inertia of which increases at a rapidly increasing rate as v approaches c, and with electrical theory, which makes the inertia infinite for $v=c$. Now Maxwell's electromagnetic equations for stationary media, which are in complete agreement with observation, are the basis of accepted electrical theory. Lorentz, therefore, taking these equations, expressed in terms of the rectangular space coordinates x, y, z, relative to axes fixed in the ether, and of the time t, sought for a transformation which should reproduce them, unchanged in form, in terms of new coordinates moving uniformly through the ether. He succeeded with respect to the equations for the free ether, but with a slight deviation in the presence of electric charges. Taking the x-axis as the line of motion, the new coordinates x', y', z', t' were determined by the equations

$$x'=\gamma(x-vt),\ y'=y,\ z'=z,\ t'=\gamma(1-vx/c^2),$$

where $\gamma\equiv 1/\sqrt{1-\beta^2}$. The time t' is here introduced as a mere auxiliary mathematical quantity bearing as yet no physical meaning.

This was presently assigned by Einstein, who pointed out that our observations can determine only coincidences, *i.e.* simultaneities in time and space of material particles with each other and with light rays. On a material system S, such as the earth, the simultaneity of coincidences observed at different stations could be determined if all observing stations were supplied with synchronous clocks. Assuming the principle of constant light velocity, clocks at stations A and B were defined by Einstein as synchronous only if, when a light flash at A at the time t_A by the A clock reached B at the time t_B by the B clock, and was instantly reflected back to A, reaching it at the time t'_A by the A clock, the relation $t'_A - t_B = t_B - t_A$, or $t_B = (t_A + t_A')/2$ holds good. Thus a universal time t, say S-time, can be established for all S observers. Similarly, an S'-time, t', may be supposed established on a system S' in uniform rectilinear motion relatively to S.

Then, always assuming the principle of constant light velocity, c, Einstein enunciates as the General Principle of Relativity: The laws according to which the states of physical systems are changing aré the same, whether these phenomena are referred to the system S or to any other system moving uniformly with respect to it. Since such laws are the results of observations of coincidences only, the condition that laws expressed in terms of S-time and S-space coordinates should be similarly expressed in terms of S'-time and S'-space coordinates, will be that events locally simultaneous for S' observers should also be simultaneous for S observers. Einstein then found that this condition would be fulfilled if x', y', z', t', the S'-coordinates and S'-time, were derived from the S-coordinates and S-time by the Lorentz transformation. Further, the transformation from S to S' differs from that from S' to S only in that v, the velocity of S' relative to S, is

replaced by $-v$, the velocity of S relative to S'. Thus the relation between S and S' is entirely reciprocal, so that neither occupies a special position, the consideration of rest or motion in the ether being eliminated, and replaced by the relative uniform velocity of either system to the other.

Einstein's General Principle of Relativity may be expressed in the form: The mathematical expressions of the laws of physical phenomena are *covariant* with respect to the Lorentz transformation. Einstein has shown that this condition is rigorously fulfilled by Maxwell's equations for stationary media, the small deviation in the presence of electric charges in Lorentz's representation being now eliminated.

We cannot say, in this method of representation, that a longitudinal length L' in S' moving with velocity v relative to S *is* shortened by its relative motion, but that it will appear shortened to L'/γ to observers in S observing its two ends simultaneously. A phenomenon in S', of duration T' to S' observers, will appear to S observers to have its duration increased to $\gamma T'$.

The starting point of Minkowski's representation was his finding that the Lorentz transformation might be expressed in the form $x' = x\cos\omega + i\sin\omega$, $y' = y$, $z' = z$, $l' = l\cos\omega - x\sin\omega$, where $l = \sqrt{-1}ct \equiv ict$, say, $l' = ict'$, $\omega = \tan^{-1}i\beta$. Now this is a pure rotation, in the four dimensional space x, y, z, ict, through an imaginary angle ω in the plane x, ict, and therefore round the plane yz (*i.e.* the whole plane yz remains fixed, just as in three dimensions a rotation is round a fixed line, the axis). There is, therefore, no change in the length $\sqrt{x^2+y^2+z^2-c^2t^2}$, so that its square, $x^2+y^2+z^2-c^2t^2$, will retain its value unchanged, *i.e.* both are invariants of the transformation. But y and z being unchanged, so will y^2+z^2, and hence also $x^2-c^2t^2$. Now, the whole history of a moving particle or light-ray in space and time will be represented in the Minkowski space-time, or *world*, by a continuous line of point instants, which he calls a *world*-line.

Take, after Minkowski, the case of a light-ray, or particle moving with the velocity of light, along the x-axis, so that we have to consider only the plane section of the *world* containing the x-axis and time-axis. We shall also take the *world* defined by x, y, z, ct, in which the Euclidian rotation through the imaginary angle ω in x, y, z, ict will become a real non-Euclidian rotation through the angle $\psi = \tan^{-1}\beta$. Through a point O as origin draw the x-axis SON (south to north) and the ct-axis WOE, ON, OS, OE, and OW being all equal, and representing the units $x = \pm 1$ and $ct = \pm 1$ respectively. Draw the two bisectors $SW\,O\,NE$ and $NW\,O\,SE$ of the right angles NOE and NOW. These will be the asymptotes of the two conjugate hyperbolæ $x^2 - c^2t^2 = \pm 1$. The line $SW\,O\,NE$ will represent the world-line of the ray or particle. To represent either as at rest, *i.e.* to pass from the system S to the system S' moving with the ray or particle, turn ON and OE through the angle $\psi = \tan^{-1}\beta$ and both towards, or both away from, the asymptote ONE, and if N' and E' are the points in which they intersect the conjugate hyperbolæ, the lines ON' and OE' are the new axes and their lengths are the new units in which x' and ct' are measured. Since $x^2 - c^2t^2$ is invariant, the asymptotes ($x^2 - c^2t^2 = 0$) and the hyperbolæ ($x^2 - c^2t^2 = \pm 1$) remain fixed, as would be the case for the whole system of conjugate hyperbolæ $x^2 - c^2t^2 = \pm\epsilon^2$, where ϵ is any real number. Since the velocity v can never exceed c, world-lines through O must clearly be confined to the region included by the angles $NE\,O\,SE$ and $NW\,O\,SW$. Since a time-axis can be drawn from O through any world-point for which $x^2 - c^2t^2 < 0$, and an x-axis through any for which $x^2 - c^2t^2 > 0$, it follows that any point on any world-line can be made simultaneous to O, or any light-ray or moving particle transformed to rest. The case of motion in a plane requires a three-dimensional *world* for its

representation, which can be obtained by imagining the y-axis drawn through O perpendicular to the paper, and rotating the whole two-dimensional figure about the axis WOE. The transformation consists in passing from one to another set of three semidiameters of the conjugate hyperboloids $x^2+y^2-c^2t^2=\pm 1$. The four-dimensional transformation required for motion in three dimensions consists in a process which may be described analogically as passing from one set consisting of one semidiameter of the first of the pair of conjugate four-dimensional hyperboloids $x^2+y^2+z^2-c^2t^2=\pm 1$ and a pencil of semidiameters of the second to another corresponding set.

If the specification of physical phenomena were complete in terms of observed coincidences, they could be expressed by means of intersections of world-lines filling the whole *world*. Any one observer's (correct) results would be expressed in terms of a *field-figure* in the *world*, composed of a net of world-lines of which the point-instants determined by their intersections would alone be significant, and correct observations of all other observers would give rise to field figures apparently differing widely from each other, yet all essentially identical in so far as they record identical phenomena, for any such would be transformable one into the other by mere deformations of the *world* with its world lines. The deformations can be followed up analytically by introducing coordinates by attaching to each point-instant P in a field-figure F four numbers, say x_1, x_2, x_3, x_4, such that along any line in F they change continuously and no two points have the same four numbers. Passage from tetrad to tetrad of numbers by means of the Lorentz transformation will then carry us from point to point of F. The deformations can also be followed geometrically by imagining as constructed around each point-instant an infinitesimal four-dimensional pair of conjugate hyperboloids $x_1^2+x_2^2+x_3^2+x_4^2=\pm\epsilon^2$, where ϵ is a real infinitesimal quantity, and so mapping out the *world* x_1, x_2, x_3, x_4 by these *indicatrices*, very much as an electric field is mapped out by Faraday lines (unit tubes) of electric force.

Gravitation impresses the physicist as being something more general and more fundamental than other natural forces. At a given point in a gravitational field, every material substance receives the same gravitational acceleration whatever its physical or chemical state. Moreover, no satisfactory theory of its nature has yet been found, and every hypothesis that appeared, even temporarily, likely to lead to a solution has involved seeking its source in interaction between the known universe and a greater unknown universe in which ours would be included. Now, in the Minkowski *world*, any point-instant in a gravitational field, representing a material particle moving under the acceleration due to the field, may be transformed to rest, and so will apparently cease to be subject to acceleration. And it is only through the acceleration to which it gives rise that a gravitational field is known to us. Such considerations led Einstein to formulate what is known as "The Equivalence Principle," that *there is no distinction between a gravitational field and a field of acceleration*. Its admission would involve the conclusion that electromagnetic acceleration must be included as dependent on the gravitational field, a statement which I think summarises the theoretical objections that might, *à priori*, be raised against it. If true, gravitation must be determined by the coordinates.

Einstein's procedure in developing the result of this hypothesis is first to define a Minkowski *world* with three imaginary space-axes, ix_1, ix_2, ix_3, and a real time-axis, x_4, so that the invariant element of length joining two adjacent points, PQ, is given by the equation $ds^2=-dx_1'^2-dx_2'^2-dx_3'^2+dx_4'^2$, where dx'_1, etc., are the changes in the coordinates in passing from P to Q. There is then no gravitational field.

The most genera possible transformation to coordinates x_1, x_2, x_3, x_4 will give for ds an expression of the form

$$ds^2 = g_{11}dx_1^2 + g_{22}dx_2^2 + g_{33}x_3^2 + g_{44}x_4^2 + 2g_{12}dx_1dx_2 + 2g_{13}dx_1dx_3 + 2g_{14}dx_1dx_4 + 2g_{23}dx_2dx_3 + 2g_{24}dx_2dx_4 + 2g_{34}dx_3dx_4,$$

in which the g's are functions of the coordinates, and completely specify the transformation. The values of the g's were so determined as to reduce the gravitational field at P to zero, and then, equating the two values of ds^2, a relation was obtained which specified the gravitational field in so far as to show how it might be reduced to zero. Out of all the possible groups of ten equations to determine the g's, Einstein, by means of the Riemann-Christoffel theory of tensors, succeeded in picking out the few covariant groups, and amongst these he found one, and one only, which could be reduced to a form satisfying approximately the equation $\nabla^2 V = 0$, Laplace's equation for the Newtonian gravitation potential at a point in ordinary space where there is no matter, when g_{44} was written for V. In the presence of matter, the equation to be satisfied would be $\nabla^2 V = -4\pi G^2 \rho$, where ρ is the density of the matter and G is the gravitation constant; and the generalised form of this was found by Einstein in a certain expression T which occurred in the group of ten equations for the g's, which could be taken as representing the energy of the gravitational field, and that when this was done, the ten equations satisfied Hamilton's principle. T is not a tensor, but the electromagnetic energy is, and consequently also satisfies Hamilton's principle, from which it follows that in the presence of an electromagnetic field of energy E, the total energy will be $T + E$.

Lorentz has approached the subject directly from the vector geometry of the field-figure, the indicatrices in the case of Einstein's *world* being conjugate hyperboloids with one real and three imaginary axes. He first expresses a system containing material particles and one electromagnetic field in terms of a single function H to which Hamilton's principle can be applied directly. H consists of three parts, relating to the material particles, the electromagnetic field, and the gravitation field respectively. Then, by the introduction of coordinates, Einstein's equations are obtained as a direct consequence of the application of Hamilton's principle to the function H: for the most general form of electromagnetic system: for a gravitation-field consisting of incoherent similar material particles, with or without molecular forces acting between them, in such manner that the system would be regarded as possessing a potential energy depending on the density only: in a more general case, applicable, *e.g.* to systems which are non-isotropic as regards both configuration and molecular action. To arrive at Einstein's complete equations he has to add the term which in Einstein's equations refers to thermodynamic and other effects, which in so far as they are not conservative, cannot legitimately be treated by Hamilton's principle.

The points of Lorentz's field-figure will be identical with the points of Einstein's selected form of the time-space, in which the passage from point to point is effected by pure rotations, if the axes be chosen so that dx_1, dx_2, dx_3 intersect the conjugate indicatrix, of which the semidiameters, determining the units of length, have the length $i\epsilon$, making g_{11}, g_{22}, g_{33} negative, while dx_4 intersects the indicatrix, the semidiameter of which is ϵ, so that g_{44} is positive. It follows that any deformation of the field-figure will leave unchanged the lengths of lines so expressed—in *natural measure*—so that in the geometrical representation, the covariance of the equations, when coordinates are introduced, is assured beforehand.

To determine the ten g's for any point instant other than the selected orig of coordinates for which the gravitation field vanishes, we have ten differential

equations, together with the four which determine the system of coordinates, fourteen in all. But the existence of an invariant such as ds will have the effect, as Hilbert has pointed out, of reducing the number which are independent by four, so that there remain ten equations to determine the ten g's. Moreover, Silberstein has shown (*R.A.S. M.N.* 77, p. 366) that the element of length, and hence the values of the g's at the origin, remain invariant to infinity in the absence of a gravitational field, so that there are no outstanding constants, or arbitrary functions, for determination, and therefore the g's, and consequently the gravitational field specified by them, are completely determined throughout the time-space. Einstein appears, as Silberstein suggests, to have overlooked this, and has attempted to evade the determination of boundary conditions by a modification of his theory in which he assumes that our physical space is of constant positive curvature, and therefore finite, involving some extremely bizarre results, as Silberstein expresses it, besides further complicating his equations.

The introduction of an electromagnetic field gives four more equations, with four parameters to be determined, and the resultant field is completely determined by the g's and these parameters.

We see that Einstein's representation of gravitation does not present it as a force or offer any physical account of its origin, but as a condition modifying the metrical properties even of the time-space forming the ultimate system of reference for all physical phenomena, and consequently the metrical properties of our physical space, which therefore ceases to be Euclidian and varies in curvature from point to point and moment to moment. Gravitation would, therefore, on this theory, influence all physical phenomena. For example, at the surface of the sun, where gravitation has about twenty-seven times its value at the earth's surface, the path of a light ray would be bent to a measurable extent, and the unit of time would be increased by an amount which would cause a measurable displacement of solar spectroscopic lines towards the red.

The first favourable opportunity of observationally testing the former conclusion will occur during the coming total solar eclipse of May 29, when it will be sought for. The latter effect has been sought for in a long-continued series of observations by St. John at the Mount Wilson Observatory with an absolutely negative result, confirmed by later observations by Evershed at the Kodaikanal Observatory.

It would appear then that Einstein's *equivalence principle* must be rejected as untenable.

Now, in 1898, Gerber (see the writer's *Treatise on Electrical Theory*, p. 597), on the assumption that gravitational potential is something propagated from the attracting to the attracted mass with a velocity, v, which is very great compared with the relative velocity of the two masses, obtained an expression for the anomaly in the motion of Mercury's perihelion in terms of v, which, when v is made equal to $c = 300{,}000$ kilometres per sec., becomes identical with that of Einstein, giving 43·1 secs. per century, the observed value of the anomaly being 42·9. Taking as the gravitational potential the velocity potential of the ether in any form of the gravitation theory in which the electrons are regarded as sources or sinks of ether flow (*ibid.* pp. 601–16), the anomaly is, therefore, fully accounted for. Gravitation, in this theory, is assumed to act only on the hydrodynamic mass of the electron nucleus, the inertia of which, if my argument was correct, is negligible relatively to the electromagnetic inertia. Under these conditions an agreement, to the order required to account for the anomaly, of Einstein's formula with Gerber's would be expected.

It should be noted that Einstein's mathematical definition of simultaneity

precludes the possibility of any representation of the ether in the general relativity theory. And the ether is the very foundation of the Faraday-Maxwell electrical theory, and the present completion and extension of this theory is due to the concept of the electron, the only possible meaning of which, as far as the physicist of to-day can see, is as a structure in the ether. The thorough-going relativist who regards the theory of relativity, not merely as a promising mathematical aid in elucidating and reconstructing defective portions of the older theory, but as destined to replace it, constantly finds himself driven back upon such unsatisfactory concepts as, *e.g.* the retardation of a moving electron by the reaction of its own electromagnetic field, adopted by Leigh Page in one of the most powerful of recent contributions to the relativity representation of electromagnetic phenomena.

DRY-ROT IN GOVERNMENT HOUSING SCHEMES (Lord Leverhulme)

SHALL our future housing schemes "lean" on Government and our houses be erected on the quicksands of doles and grants, or shall they be built plumb and true and resting on the solid rock of citizen self-reliance? The man or woman who is an optimist on the possibility of any permanent solution of our housing problem by means of Government doles and grants in aid of approved housing schemes, and who is a pessimist as to the possibility of the full and complete final solution of our housing problem on the lines of individual private enterprise, is going to get a rude shock and awakening within the next quarter-century. Our Government by their housing schemes would treat certain citizens as if they required spoon-feeding or as if they could not earn sufficient money under the British flag and within the United Kingdom by their industry and intelligently directed labour to pay a reasonable self-supporting return on the cost of their homes, but for those working in the United Kingdom under the waving expanse of the Union Jack the Home Government must make grants of sops and doles to provide houses, whilst our fellow citizens in Canada, Australasia, and South Africa can provide their own houses and pay higher rentals than would be the economic rent in the United Kingdom. Britons, we shout, never, never shall be slaves, but there is no slave so fettered as the British citizen who cannot provide out of his wages his own home.

Let our British Government bear in mind that the American Government does not require to give, and does not propose to give, doles and sops to the working men of the United States towards the cost of his home. Are we to undermine the independent British spirit of self-reliance of our British citizens whilst the American Government cultivate to the full a spirit of self-reliance in American citizens? If so, will not the final result be that English-speaking peoples in the United States will become a strong virile race whilst we in the Motherland will become less and less self-reliant and virile until finally the sceptre of Empire falls from our palsied hands? Our newspapers are to-day crowded with columns of articles on schemes for the solution of housing problems, and the stereotyped headline adjective is "Generous." Is it the province of a Government to be "generous"? Is it not rather the province of a Government to be just? It is very easy for a Government to be "generous" with other people's money—the money it takes from the pockets of one set of citizen taxpayers to hand over to another set of citizens—but does a Government achieve anything of permanent good by these methods and cannot public requirements be

more permanently attained on some sounder lines? We have adopted a similar method with regard to the present high price of bread, and a shilling loaf was promised for ninepence. If doles and sops are to be our policy, we may be certain that, like dry rot in a building, it will spread until there is not a sound plank or board left. We are already reaping the natural consequences of the Bolshevik views now being promulgated in our midst. The workman is being encouraged to think that he can get more out of Government by strikes and disturbances than he can out of his own labour by steady industry. He has taken up the Bolshevik shout to Governments, "Do as we want, and mighty quick too, or we'll down and out you." The appetite will grow with what it feeds on until, gorged and repleted, it has killed by disease its pampered victim.

What is the position with regard to housing to-day, and what should be the remedy for the present scarcity of houses? House builders and property owners must be freed from limitations that choke their enterprises. On the one hand, whilst we have rightly imposed many rules and regulations required by health, and whilst these must remain, it is equally certain that we have rather overdone our health aims and ideals, and we must now remove all unnecessary faddist restrictions as to building and building material, the removal of which would not be prejudicial to health. Our building bye-laws would appear to be framed to make cottages costly and to last a century or more. Long before a half-century can have passed our habits will have so altered that different types and plans of houses altogether will be required, and we shall be pulling down and rebuilding these needlessly costly homes. All this wild talk of profiteering when rents are raised must cease and builders must be free, as are all other citizens, including their tenants, to receive increased income to meet increased rates, taxes, maintenance, as well as increased cost of building. Then the joiner, bricklayer, stonemason, slater, and all other artisans must be freed from the tyranny of restriction of output. There must be freedom to the master builder to pay wages, always above the trade union minimum wage, as a premium for increased output. In this way house building will be cheapened enormously, rents be made easy of payment on basis of cost of production out of the higher wages earned by the artisan. Then houses and all property in the form of buildings must be freed from the burden of our present system of rating and all necessary local taxation must be raised by a local income tax. I have here before me a list of rates levied in towns in England during 1918, and I find they vary in total amount from 4*s.* 11*d.* in the £ in Oxford to 14*s.* in West Ham, and that on the average the rating in the £ for 1918 is 20 per cent. to 25 per cent. higher than it was in 1914. In Oxford there are practically no industrial factories, but there is a good average high income—so that Oxford would still be a low-rated town. In West Ham there are abundance of industrial works, and an income-tax on local earnings and incomes of both employers and employees would make the heavy load of rates an easier burden borne by all sections of citizens. Equally in our seaports, such as Liverpool, the earnings of our shipowners would contribute their just share in the form of local income-tax to the expenses of municipal government instead of, as at present, being rated quite inadequately on office rent only, whilst their ships, which are really so many floating factories, escape rating altogether.

On these lines, and only on some such lines, can we meet our difficulties in housing. We must look now and in the future, as in the past, to the private enterprise and self-reliance of our citizens, and not hope to solve our problems by looking to Government for doles and sops and to the building of "leaning houses" dependent on the rotten and insecure prop of Government support.

WAR—A PLEA FOR SCIENTIFIC RESEARCH (Major-General Charles Ross, C.B., D.S.O. (ret.))

PART I

It is evident that war is one of the worst, if not the most terrible, of all the scourges which afflict the human race. For not only are untold horrors inflicted on those communities which are invaded, not only does the victor, even, lose large numbers of his best men, but the conflict is, almost invariably, accompanied and followed by starvation, disease, and revolution, the last being the most savage and ferocious of all forms of war. Who shall compute the total losses and suffering resulting from this last great war?

While a vast and scientific literature has grown up throughout the centuries on the conduct of war, there have been few, if any, serious attempts to study scientifically the causes of war as a necessary preliminary to their elimination. The place of such scientific study has been taken by more or less acrimonious abuse of "militarism"—which is glibly accounted the sole cause of war, whereas it may be but a mere and inevitable consequence of the conditions under which humanity exists.

The causes of war are extremely difficult to ascertain, for the reason that every nation ascribes its participation to the noblest of motives—self-defence, the defence of weaker nations, idealism, and the like—and will never admit that it was actuated by the baser motives of self-interest, ambition, revenge or fear. The motives are also often closely bound up with religious views and convictions and are, accordingly, such dangerous ground that men hesitate to tread it. There is, for instance, a general belief that the Almighty permits these awful horrors to continue, but will, in His own good time, put an end to them by the triumph of good over evil; and we forget that this view is analogous to the discredited custom of the Middle and Dark Ages in which ordeal by battle was called upon to decide the justice or injustice of private feuds and quarrels. Extreme exponents of this view go further and maintain that war is sent as a just punishment for our sins, and that it is, therefore, impious in us to attempt either to avert or prepare for it. These, presumably, would kneel down and permit a ruthless enemy to trample them underfoot. Others, again, are convinced that "Heaven helps those who help themselves"—"to other people's property," adds the free-thinker—that it is incumbent on us to take all necessary precautions, to make essential preparations, to do our utmost up to a certain point to avert war, but in the last resort to throw in our whole power in the fight for justice and freedom.

In contrast to these opinions, there are the strictly materialistic views. The one, that of the so-called "Pacifist" who regards war as a relic of barbarism, a manifestation of criminal lunacy, unworthy of our present stage of civilisation. The exponents of this view maintain that war can, and must, be averted, or that civilisation will be destroyed; that co-operation must replace competition, that nations must take each other on trust; that the world must be democratised: that the lion must have teeth and claws drawn and the lamb encouraged to lie down in confidence by his side. War is to be averted by the reduction or abolition of armaments, except only the necessary police forces; above all, General Staffs, with their plans for wars, secret diplomacy with its treaties founded on the employment of force, must be definitely repressed. In a word, they believe in the balance of impotence for war.

Opposed to this view is that of the militarist. He argues that not only must the teeth and claws of the lion be drawn, but the whole nature of the animal changed

from that of a savage, carnivorous beast to a peaceful creature content to eat grass. Until this is accomplished he can by no means conceive a lamb so foolish as to come within reach of the lion. In his view, war is the inevitable outcome of the struggle to exist, a law of Nature; he believes that competition can never give way to co-operation. He considers that nations fight for their own vital interests, primarily in self-defence, against a growing menace. He doubts whether the abolition of armaments, of plans of war, of secret treaties in a world of democracies will necessarily avert war. He is firmly convinced that no sensible nation will entrust its security to the good will of its neighbours or even to so-called guarantees, especially in view of the late experiences of the Belgians. In a word, he believes in the balance of power or force.

It will be seen that these two last views are as diametrically opposed as those of the religious views; yet there must exist ample evidence in history, as well as in the researches of science, to enable us to arrive at a definite conclusion as to which is correct. It is, surely, necessary to investigate the true causes of war, to go to the root of the evil. To tinker at the surface is to treat medically a sick man without previously diagnosing his complaint. When one considers the enormous strides in the cure of diseases due solely to scientific research, one is astounded at the neglect to apply similar methods to the recurring catastrophe of war. Now that the nations of the world are exhausted by the late great struggle and a powerful League of Nations is being formed to exercise control, there is a prospect of peace for some years to come. This relief, however transitory, gives the opportunity for a scientific and exhaustive study of the whole problem by means of which alone, it is probable, can a permanent cure be effected.

The following conclusions have an important bearing on the subject; they can be easily verified or disproved and may, perhaps, give some foundation for further exploration.

Is War due, as commonly asserted, to Militarism?

German militarism is generally credited with the whole responsibility for the late war.

It is a well-known fact that modern Prussian militarism originated after, and in consequence of, the Battle of Jena (1806), as the only means by which Prussia, conquered and ground down under the heel of Napoleon, could hope to regain her independence. It was, thus, purely defensive in origin.

French militarism—that is, the system of universal and compulsory military service—was introduced after the war of 1870-71, and was the direct result of the fear of further German aggression, though the desire for revenge also existed.

After 1871 all continental nations which had land frontiers and reason to fear their neighbours, introduced militarism, which gradually became the prevailing system throughout the world. Japan also introduced the system with, however, the main idea of suppressing revolution and establishing order within her own territories.

Great Britain did not adopt the system. On the other hand she maintained and increased her great navy as a defensive weapon, a "sure shield." She was content, in the first instance, with a small army for the defence of her colonies. As, however, the German menace arose, so did she organise more thoroughly her land forces to resist possible invasion in case her navy failed her.

The United States, having no fear of foreign aggression, was content with very small armaments.

It is evident that militarism and, indeed, all armaments, are due to fear in the first instance—the instinct of self-preservation.

Militarism cannot, therefore, be held responsible for war, inasmuch as war existed long before modern militarism was evolved.

To what extent were Fear or Ambition responsible for the late Great European Wars?

The war of 1866 was certainly due to the desire of Prussia to establish her supremacy in Germany and to build up a German Empire. The desire originated in the consciousness that the various German States, being separate and distinct communities, were not in a proper condition to resist aggression. Austria, the nominal head of Germany, had displayed such incompetence as to render it patent to all that she was unfitted to champion the cause of the German peoples or to weld them into a nation. The war of 1866 resulted, therefore, from a combination of fear and ambition; but that ambition can hardly be termed other than legitimate

The war of 1870-71 was due partly to the desire of the Germans to establish their empire firmly, and partly to the fear lest the French, jealous of the Prussian military successes, should attack the North German Confederation. The well-known military maxim of attacking the enemy before he was ready was put into practice by the Germans; and, as a means to that end, the most thorough pre-war preparations were made—while the war was precipitated at a suitable moment by most unscrupulous methods. The unready and inefficient condition of the French was certainly well known to the Germans. Thus it appears that the war of 1870-71 was due more to ambition than to fear. Was German ambition legitimate in this case also? It is difficult to say The methods adopted by the Germans can, however. only be justified by the fact that the French would certainly have adopted similar methods had they recognised the expediency and possessed the capacity. For the French were, undoubtedly, jealous of the rise of Prussia, and the whole nation greeted the outbreak of war with enthusiasm, confident in their power to break up the German Confederation. The French Government had, moreover, taken steps to obtain the assistance of Austria; neither had it hesitated, shortly before, to propose a secret treaty to Prussia for the partition of the helpless Belgium. French diplomacy, indeed, was not less unscrupulous than that of the Prussians, but merely less competent. Thus the Germans had good reason to fear the French, and as a defensive measure their ambition can be justified. Incidentally, it is a noteworthy point that, at that time, the modern system of conscription had not been introduced into France. Hence unscrupulous diplomacy cannot be ascribed solely to militarism. Neither can it be ascribed solely to autocratic Government; for Napoleon III. was by no means an autocrat.

It was after 1870 that German militarism changed its nature and, confident in its power, became over-bearing, arrogant, and more utterly unscrupulous than ever. This lack of scruple was due to the fact that the lack of scruple paid—as was evident to the German leaders from their study of history. It had paid in 1870; it paid Napoleon; it paid Frederick the Great; it has always paid; and this late great war is the first notable exception to the rule. It is still doubtful, indeed, whether the Germans will not benefit by it in the end; for, while inflicting the most cruel punishment on their adversaries, they have so far, in comparison, suffered but little themselves.

In 1914 the Germans were again deliberate aggressors. They have shown the world on a hundred occasions that they were fully conscious of their overwhelming strength as against France and Russia. The Russians, however, had commenced

the construction of strategic railways and introduced measures to increase their army and expedite mobilisation. These steps were obviously directed against the Germans or their allies the Austrians. The French also had increased their army in reply to a similar increase on the part of Germany; while the French desire for revenge and the reconquest of Alsace and Lorraine had by no means died out. The Germans were, perhaps, actuated by some slight suspicion of fear; but most men will agree that the idea of fear was put forward as a mere excuse for aggressive and unscrupulous action and as part of the propaganda for gaining the sympathies of neutrals. There is, now, no doubt whatsoever that German ambition was solely responsible for the war. Can this ambition be termed legitimate? A sharp distinction must evidently be drawn between legitimate and illegitimate ambition. The former arises apparently from the instinct for self-preservation; the latter from cupidity. And the German ambition, the Pan-Germanism, which sought to destroy France, to seize Belgium and the Channel ports, to establish an empire in the Near East—the whole as a mere stepping-stone to the destruction of British power and the supremacy of the world, was sheer cupidity.

British intervention in this war has been ascribed to various causes. On the one hand it is said that the motive was idealism, the desire to protect weaker nations and indignation at the aggressive conduct of the Germans. On the other hand, it is said to have been the instinct of self-preservation and the belief that if France were destroyed the turn of the British would come next. Those who desire to test which of these views is correct might ask themselves—would Great Britain have intervened if the Germans had refrained from building a great navy and challenging our supremacy at sea?

However that may be, it is clear that the British were in no wise responsible for the war, except perhaps indirectly in that they neglected to build up a powerful army on the continental system and so to maintain the balance of power. But the point is of interest, as it is often maintained that idealism and religious and altruistic sentiments are causes of war. These are usually aroused—it will probably be found—after the outbreak of war, and are not, nowadays, the prime cause of it. The intervention of the United States, again, though due partly to idealism, was also largely due to self-interest, to sympathy with democracy, and to wrath at the high-handed and unscrupulous actions of the Germans. There also existed in both the British and the American nations the irrepressible desire to knock down a most obnoxious and insufferable cad. This sentiment may, perhaps, be termed altruistic.

The Russo-Japanese War was, as is well known, due to the ambition of both the Russians and the Japanese, legitimate on the part of the latter, and certainly illegitimate on the part of the former. There was a certain nobility in the breadth of design of German ambition in 1914; but in the case of the Russians there was nothing but sheer sordid cupidity.

The Balkan War of 1912–13 was clearly due to the ambition of the Balkan States. In this case, again, a certain justification may be found for the somewhat questionable methods adopted by the allied States in the incompetent, reactionary, and cruel administration exercised by the Turks; but no such justification can be found for the cupidity which led to the second war between the allied States themselves. It was merely a case of brigands quarrelling over their booty.

The Boer War of 1899 was due partly to the ambition of the Boers, partly to fear lest they should lose their country by "peaceful penetration." In this case the ambition appears to have been legitimate, in that the Boers desired to exercise "self-determination," and to exclude foreigners or deny them all participation in

the government of the country. The ambition of the British can also hardly be termed other than legitimate; for they possessed vast interests in the country, and it was but natural that they should wish to introduce a progressive and enlightened system of government.

Thus one can say that while illegitimate ambition is a chief cause of war, legitimate ambition may also be a cause of it.

Fear, due to the instinct of self-preservation, is also a greater cause of war than appears on the surface, in that it arouses the spirit of hostility.

Will a nation watch with equanimity the gradual disappearance of its trade and commerce, the absorption of its most lucrative industries, and the "peaceful penetration" and colonisation of its territories by foreigners, even though such evils are due to its own shortcomings? Will not the cry sooner or later arise, "Ireland for the Irish!" "South Africa for the Africander!" "Mexico for the Mexicans?" Were not such sentiments largely responsible for the Boer War, the invasion of Mexico by the United States, and are they not largely responsible for the disaffection in Ireland? "England for the English!" commenced to be heard in 1913-14 in connection with the peaceful penetration of this country by the Germans.

It is fear of being reduced to the status of helots which gives rise to these battle-cries.

Is not this same sentiment mainly responsible for civil wars? "The land for the worker!" is practically identical as a battle-cry and almost invariably precedes a great revolution. Civil war, indeed, appears to arise from causes very similar to, if not identical with, those which result in international war. And civil war is, in itself, a cause of international war. For a revolution in a country threatens the social stability of its neighbours; it is infectious and must therefore be suppressed. The attempt to suppress the French Revolution resulted in a world war. The great Powers are now, accordingly, endeavouring merely to isolate Russian "Bolshevism."

It is to be noted also that Russian Bolshevists, like the French revolutionaries, seek to impose their views on neighbouring nations.

It thus seems clear that war is due to the two sentiments of fear and ambition, of which illegitimate ambition, the predatory spirit, is the chief cause.

Why should National Ambition grow with Military Power?

It is clear that German ambition grew with German armaments. In the first instance arose legitimate ambition, manifested in 1866 and 1870, which gradually developed into illegitimate ambition as the national armaments increased in power. There are, evidently, grounds for the contention that armaments are responsible for war. How can this fact be accounted for?

It has long been a well-established military maxim that the offensive is the only safe defence and that no army can hope to defeat its adversary unless it attacks. Hence, every effort is invariably made in every army worthy of the name to inculcate the "offensive spirit" to all ranks from the Commander-in-Chief to the private. An army which lacks this spirit, unless it be overwhelmingly superior in numbers and armaments, cannot hope for victory; and this is equally true of warfare on land, sea, or in the air. In this respect an army or navy is in contrast with a police force, in which the defensive spirit only is inculcated. But whereas an army or navy expects to meet forces of equal or perhaps superior strength which it hopes to overcome by sheer fighting capacity, a police force

deals with isolated criminals over whom it enjoys overwhelming power. If, however, a frontier police force has to cope with bands of armed brigands, whom it is necessary to hunt down and attack, an aggressive spirit will quickly make its appearance. Hence, all armaments, however defensive in origin, must tend to become aggressive.

One great secret of victory is to take the enemy by surprise and strike him before he is ready. That is the one real guarantee that war shall, from the very outset, be carried into the hostile territory. It is a perfectly logical step therefore to inculcate this aggressive spirit into a nation as a whole, and especially into its leaders, if there is any reason to apprehend war. By putting this principle into execution the Germans have successfully excluded war from their own country, notwithstanding that they have been defeated.

More than this, as the self-confidence of an armed force increases, the spirit of emulation, the desire to test its strength against a menacing or hereditary enemy, arises. A spirit of contempt is also apt to appear for an hereditary enemy which for one reason or another will not make the sacrifices necessary to attain perfect military efficiency. Weakness is then regarded as decadence, which is always a source of contempt to the strong and vigorous. Thus we see the Germans regarding the French, British, and American nations as decadent. Finally, this contemptuous attitude develops into an arrogance which almost amounts to monomania. But arrogance has by no means been confined to the German nation ; it was a striking feature of the feudal systems and mercenary armies of the Middle Ages.

It is also the case that where the armed forces, acting under the command of an autocratic ruler, control the whole resources of a nation, that nation is more predisposed to arrogance, and therefore to war, than one which lacks these military advantages ; for they add to the sense of power. Such a system does give distinct military advantages and is really vital to the successful conduct of war if the opposing forces are at all equal. It will generally be adopted to a greater or less degree by any nation which fears a powerful neighbour ; and it is to be observed that though neither the French nor the British introduced such a system prior to the war of 1914, yet that they both introduced an approximation to it, the French on the order to mobilise, the British about eighteen months after the outbreak of war.

Thus it is the case, perhaps, that when a nation introduces militarism the aggressive spirit is more likely to permeate all ranks of society than if the nation had been content with a small voluntary army. Nevertheless, though the offensive spirit existed in the French army in 1914, yet there was certainly no aggressive spirit in the nation as a whole. Neither has the British nation contracted this spirit since it organised itself as a nation in arms.

Consequently, it is not the size and efficiency of armaments alone which give rise to illegitimate ambition and arrogance. It is the other way about ; it is, as always, the spirit which impels ; the armaments are merely the outward sign. And these armaments must be combined with education—propaganda—before this spirit of illegitimate ambition, finally of arrogance, can permeate a whole community. We all know, indeed, that this spirit was carefully fostered throughout the German nation.

Let us turn to modern civil or class war. Here we see very clearly ambition and arrogance engendered as the sense of power increases. First we have an upheaval of the lower classes against wealthy and luxurious upper and middle classes. Living, perhaps, in penury and in abominable conditions, the desire is, in the first instance, to exist in some degree of comfort. With a display of weakness by the national leaders, due, perhaps, to the disorganisation of society by

defeat, the lower classes quickly gain the upper hand in virtue of their vast numbers. With the sense of power comes legitimate ambition, the desire further to improve their lot, and to live in greater comfort and prosperity. Successful in this, legitimate ambition develops into illegitimate, the predatory spirit, the will to take by force the wealth of others. And the keener the sense of injustice in the first instance, the more ferocious and passionate the fighting spirit when it is aroused. When finally triumphant it develops into the spirit of arrogance and revenge and is accompanied by unimaginable horrors. The ruthless extinction of the upper classes is also deliberate and is largely the result of fear lest they should regain the upper hand.

It seems evident that the atrocities and wholesale destruction committed by the Central Powers in the territories which they had over-run were due to exactly similar sentiments. The opposition of the Belgians, for instance, and the entry of the British nation, jeopardised the whole success of the German plans and aroused the spirit of revenge. Hence the Belgians and British prisoners were the first to suffer under atrocities inflicted by the enraged Germans. Desiring, ultimately, to establish their own nationals in Belgium and portions of France, the Germans adopted the policy of ruthless extermination whenever sufficient excuse was forthcoming. A similar policy evidently actuated the Central Powers in Serbia, Russia, Armenia, and the Caucasus. The fear of neutral States and, ultimately, the fear of reprisals alone deterred.

There is, evidently, a close analogy between international and civil war, both being manifestations of identical sentiments and running similar courses. It is not sufficient to consider merely how to put a termination to international war ; it is equally necessary to study how to terminate civil war and all social evils which are calculated to give rise to it.

These sentiments which give rise to wars are not cold-blooded and easily controlled. They are ruthless and, perhaps, uncontrollable. War is, indeed, a "passionate drama."

When we consider how deeply rooted are these sentiments in human nature, the motive power, the very springs of action, it is, of course, evident that they can never be eradicated. If such a thing were possible, would not human activity cease? Remove the instinct of self-preservation, the spirit of competition, of emulation, of legitimate ambition, would not all progress cease? It may, however, be possible to control illegitimate ambition and revenge.

But it is only a sense of absolute security as between nations, the eradication of ambition, the existence of comfort and prosperity amongst all sorts and conditions of men, which can give immunity from war. The mere limitation, or even abolition, of armaments can have but little effect; for, in the future, the most appalling and destructive war can be waged by means of mercantile aircraft and poisonous gas bombs, which latter can be constructed with ease, secrecy, and rapidity by any nation which has hostile designs on its neighbour or is moved by the spirit of revenge. Besides, "the moral is to the material as three is to one"; that is, the spirit is three times more powerful than numbers. It was the unquenchable spirit of the old "Contemptibles" which enabled them to stand up to double their numbers in 1914. Thus, you might have two nations with armies identical in numbers, armament, etc., yet one, possessing the fighting spirit, might be double or three times as powerful as the other.

It is the fighting spirit, the "will for war," which must be eradicated or controlled.

(*To be continued*)

ESSAY-REVIEWS

THEORIES OF AUDITION, by Prof. E. H. Barton, D.Sc., F.R.S.: on **An Enquiry into the Analytical Mechanism of the Internal Ear,** by Sir Thomas Wrightson, Bart., Memb. Inst.C.E., with an Appendix on the Anatomy of the part concerned by Arthur Keith, M.D., F.R.S. [Pp. xi + 254.] (London: Macmillan & Co., 1918. Price 12*s*. 6*d*. net.)

This work is distinctly specialised and suggestive. It appears to be the outcome of protracted thought and research, and, in addition to presenting much anatomical detail, puts forward a somewhat novel theory of audition.

The two writers concerned contribute nearly equal parts of the work. The early portion, by Sir Thomas Wrightson, consists of four chapters occupying 155 pages with 56 figures in the text. The appendix, by Dr. Keith, covers 99 pages and has 27 text-figures. There are also 8 folding plates.

Throughout the work the anatomical descriptions are very elaborate, and are accompanied by large and clear diagrams, which will be welcomed by all interested in the intricate complexity of the internal ear. This part of the work rests upon the solid ground of observed facts.

But to some readers the chief interest will centre in the theory of hearing advanced by Sir T. Wrightson and accepted by Dr. Keith.

The presentation of this culminates in the fourth chapter, the earlier chapters being devoted to elementary matters and preliminary ideas. The authors emphasise the point that whereas some have regarded the internal ear as a structure they recognise it to be a mechanism. (The query here arises as to whether they have sufficiently borne in mind the high frequency at which it works.) But the view they hold about the action of this mechanism differs from the Helmholtz theory of sympathetic resonance. Here we enter upon debatable ground. The points at issue are briefly as follows:

(*a*) The resonance theory regards some particular parts of a graduated mechanism in the internal ear as capable of sympathetically responding to the air waves of correspondingly suitable frequency falling upon the external ear. The nerve fibres from these responding parts are then supposed to convey to the brain the mere fact of their stimulus. Thus, the analysis of sounds (or the detection of their frequencies) is accomplished in the internal ear and not in the brain, which only notes which nerve-fibres are stimulated. The sensation of a sound of frequency 100 per second thus corresponds to the stimulus of the nerve fibres which end in those structures which respond best to vibrations of about that frequency.

(*b*) The view of the present authors is that when a pressure wave of the air arrives at the external ear there is a *nerve impulse* corresponding to *each crest* and *trough* of this wave, and also to *each point* where the graph of the wave *crosses* the time axis. They hold that *all* parts of the graduated mechanism of the

ear respond at once, and that *all* the nerve fibres have to convey to the brain the *same* messages, which consist of *four* impulses for *each* wave. Thus, on this view, the ear accomplishes no analysis, in the sense in which the term is understood for the resonance theory. The only so-called analysis which they suppose the ear to accomplish is that of noting the amplitudes and times appropriate to the four points in each wave. Thus the duty of conveying four impulses per wave is put upon the nerves and that of receiving and noting them is put upon the brain.

As to the probability or possibility of these heavy demands upon the nervous organisms being met, the physicist as such has perhaps no right to speak. But there are other aspects of the matter which solicit his attention.

The present authors find difficulty in accepting the resonance theory because liquids intervene between the foot of the stapes and the basilar membrane, Corti arches, and hairlets. They conclude that because liquids are incompressible the liquids present in the internal ear must move in a piece and affect all the graduated mechanisms equally and simultaneously. But, although a liquid is nearly incompressible, it is freely deformable. Hence, if the various parts of this mechanism were tuned to different frequencies, what is there to prevent those and those only responding which are in tune with the pressure wave arriving? Similarly, when their other objections to the resonance theory are subjected to the scrutiny natural to a physicist it would appear that few if any survive.

The supposition of the authors that four nerve impulses are produced by each pressure wave received seems improbable, but, if supported by any adequate reason or experimental evidence, might have to be accepted. At present, however, it appears to lack all such confirmation.

As to the mechanisms which are specially concerned in stimulating the nerve fibres, Helmholtz at first regarded the Corti arches as playing the rôle of resonators, later the basilar membrane was substituted. The present authors regard the basilar membrane as raising one corner of the triangular Corti arches which rock about their second corners, and by their third corners actuate the roots of the hairlets whose tips are in the tectorial membrane, the nerve fibres ending at the roots of the hairlets being accordingly stimulated. This may be quite correct, but forms no disproof of the resonance theory.

On the contrary, the modification in question could be regarded as another variation of detail, leaving all the essential principles of resonance undisturbed. Indeed, there are distinct difficulties in supposing that any sufficient motions could be produced by these rapid alternations of liquid pressure save only in parts of the mechanism tuned to about the same frequency.

Though Sir Thomas Wrightson's view of audition in its present form seems unlikely to appeal to the physicist (apart from experimental support), it is conceivable that some modification of it might prove more successful.

The whole subject is highly controversial, but, even to those who hold no brief for any special theory and are anxious to welcome truth from every quarter, it appears that there is nothing here to overthrow (or sensibly weaken) the strong position at present held by the so-called resonance theory of Helmholtz.

REVIEWS

MATHEMATICS

Differential Equations. By H. BATEMAN, Ph.D., M.A., Lecturer in Applied Mathematics, Johns Hopkins University, Baltimore; formerly Fellow of Trinity College, Cambridge; and Reader in Mathematical Physics in the University of Manchester. [Pp. xii + 306.] (London and New York: Longmans, Green & Co., 1918.) Price 16*s*. net.

AN excellent addition to the excellent "Longmans' Modern Mathematical Series." The technic of solving differential equations began to appear very early in the history of the calculus, because many questions in mechanics and geometry could not immediately be expressed as explicit or implicit relations between the variables alone, but only in the form of relations between these variables and their differentials, which, in many cases, could be solved so as to express some—but not all—of the properties of such relations as are indicated above. This is the point from which the present book starts (p. 1), and the book is principally concerned with methods of actual solution of differential equations, as opposed to more theoretical treatises. In fact, general theorems on existence of solutions are not touched until disintegration by series is considered (pp. 223, 245).

"In writing this book," says the author (p. v), "I have endeavoured to supply some elementary material suitable for the needs of students who are studying the subject for the first time, and also some more advanced work which may be useful to men who are interested more in physical mathematics than in the developments of differential geometry and the theory of functions. The chapters on partial differential equations have consequently been devoted almost entirely to the discussion of linear equations." After a first (preliminary) chapter on differential equations and the nature of their solutions, in which are treated discontinuous solutions and Green's function for one dimension, precedent is slightly departed from in that, instead of beginning with the usual forms of equations which can be solved very easily, the second chapter is on integrating factors, and the third chapter is on transformations of given equations into others which can be integrated directly or which present certain other advantages. The fourth chapter is on geometrical applications, and the fifth is on differential equations with particular solutions of a specified type. The sixth chapter is on partial differential equations, and the treatment of special solutions bears some resemblance to a treatment lately given by Prof. M. J. M. Hill (cf. SCIENCE PROGRESS, 1918, **12**, 548), though Dr. Bateman's treatment is independent of Prof. Hill's paper (p. v). The seventh chapter is on total differential equations, the eighth is on partial differential equations of the second order, and both of these chapters contain a few results which seem new (p. v). The remaining three chapters are on integration in series, the solution of linear differential equations by means of definite integrals, and the mechanical integration of differential equations.

This book is an admirable one; the examples are particularly interesting—

they are often drawn from physics, chemistry, radio-activity, and probability, while there is a problem about finding Sir Ronald Ross's equation in his theory of the infection of a population (p. 296), and problems about the rate at which eggs go bad (pp. 298, 303). The only mistakes noticed are the wrong spelling of the names of Bendixson (p. 252) and Leibniz (*e.g.* on p. 262).

PHILIP E. B. JOURDAIN.

Projective Geometry. By OSWALD VEBLEN, Professor of Mathematics, Princetown University, and JOHN WESLEY YOUNG, Professor of Mathematics, Dartmouth College. [Vol. I. pp. x + 344, 1910; reprinted 1916. Vol. II. pp. xii + 511, 1918.] (Boston, New York, and London: Ginn & Co.)

THE first volume of this important book was reprinted in 1916, and the second volume, which was published in 1918, is due almost exclusively to Professor Veblen. In recent times the science of geometry has been put into a purely deductive form principally by Peano and other Italian mathematicians, and, later on and by rather different methods, by Hilbert and his school. In this transformation of geometry, Prof. Veblen has taken a leading place among American mathematicians by his original work, and now he and Prof. Young have rendered a very great service to the mathematical world by this treatise: little original work and no treatises of great importance on deductive geometry have as yet been produced by British logicians.

In giving a complete foundation for geometry, it is necessary to study linear order and continuity, and this is deferred to the second volume: "the more elementary part of the subject rests on a very simple set of assumptions which characterise what may be called 'general projective geometry'" (Vol. I. p. iii).

After an introduction explaining the logical foundations of the subject and dealing with the "consistency," "categoricalness," and "independence" of a set of postulates, the chapters begin with one on theorems of alignment and the principle of duality. The second to the fifth chapters are on projection, section, perspectivity, and elementary constructions; projectivities of the primitive projective forms of one, two, and three dimensions; harmonic constructions and the fundamental theorem of projective geometry; and conic sections. In the sixth chapter, on an algebra of points and one-dimensional co-ordinate sytems, analytic methods are introduced on a purely projective basis by what are, in essentials, the processes of von Staudt and his modern successors, and the introduction "brings clearly to light the generality of the set of assumptions used in this volume. What we call 'general projective geometry' is, analytically, the geometry associated with a general number field. All the theorems of this volume are valid, not alone in the ordinary real and the ordinary complex projective spaces, but also in the ordinary rational space and in the finite spaces" (Vol. I. p. iv). The other chapters are on coordinate systems in two- and three-dimensional forms; projectivities in one-dimensional forms; geometrical constructions and invariants; projective transformations of two-dimensional forms; and families of lines.

The second volume can be read starting from after the eighth chapter of the first volume, and contains chapters on foundations (including discussions of order and continuity); elementary theorems on order; the affine group in the plane; Euclidean plane geometry; ordinal and metric properties of conics; inversion geometry and related topics; affine and Euclidean geometry of three dimensions; non-Euclidean geometries; and theorems on sense and separation. Both volumes contain indexes.

It is interesting to see on p. 27 of the second volume a form of the assumption made use of by Zermelo in 1904 that a continuum (for example) can be well-ordered.

PHILIP E. B. JOURDAIN.

The Philosophy of Mr. B*rtr*nd R*ss*ll, with an Appendix of Leading Passages from Certain other Works. Edited by PHILIP E. B. JOURDAIN. [Pp. 96.] (London: George Allen & Unwin, 1918. Price 3*s.* 6*d.* net.)

THE motto of this clever and amusing volume is the dictum of the Red Queen that "Even a joke should have some meaning." Mr. Jourdain's little joke has a great deal of meaning, packed into a very small compass. Mr. B*rtr*nd R*ss*ll, whose papers are here collected, was killed by anti-suffragists in 1911; but the manuscript on which this volume is based was fortunately saved from the flames kindled by the eager champions of the Sacredness of Property who burned down his house. The editing has been done by one who believes that such work may be a fine art; and the result fully justifies that belief. References and appendices are constructed with that eye for economically ordered effort and balanced form which one associates with a poet or a sculptor. The relations between Mr. R*ss*ll and other thinkers can therefore be easily and rapidly traced. Thus Mr. Jourdain's notes to the first sentence open up a vast field: "The view that the fundamental principles of logic consist solely of the law of identity was held by Leibniz, Drobisch, Uberweg, and Tweedledee" (p. 11). If any reader should be surprised at the appearance of Tweedledee in this august company let him note the distinction between sign and signification made by the White Knight (p. 22); the nominalism of the Hatter (p. 24); and the likeness of the March Hare and the Gryphon (who changed the subject when Alice asked awkward questions) to those mathematicians whose method of solving the paradoxes, arising out of commonly held logical views, is simply not to notice them (p. 77). In other words this volume has, for the first time, placed the works of Lewis Carroll in their proper place as profound contributions to logic.

The influence of Mr. R*ss*ll's work upon his great contemporary Mr. Bertrand Russell is very apparent. For example, his mode of proving that Humpty-Dumpty was a Hegelian is closely allied to what has sometimes been called, slightingly, the *a priori* method of writing history—a method utilised in Mr. Russell's construction of what Leibniz's views would have been had they formed a consistent whole.

The book is not only full of delightful jokes but propounds an arrangement of them in a hierarchy. This forms a striking application of the theory of types; though it would appear from a footnote that Mr. Russell himself is surprisingly addicted to jokes of the first order (p. 81). The application of logic to everyday commonplaces is in itself humorous, because incongruous; as when Mr. R*ss*ll notes that "people who refer to the Oxford Movement imply that Oxford only moved once" (p. 54). But a more incisive satire is shown when he is exposing the follies of the self-styled anti-metaphysical scientist who imagines that he has no metaphysical basis because what he has is so crudely uncritical. Thus it is observed that "scholastic" is an academic epithet applied by such folk to any order of thought which is more exact than that to which they are accustomed (p. 23; cf. p. 74). As for the "pure experimentalist," let him ponder deeply Mr. R*ss*ll's application of his method (p. 88). "I should make," he says, "a statistical inquiry among school children, before their pristine wisdom has been

biassed by teachers. I should put down their answers as to what 6 times 9 amounts to, I should work out the average of their answers to six places of decimals, and should then decide that, at the present stage of human development, this average is the value of 6 times 9."

There are arrows of wit to pierce all armours. A book for all who can stand a joke, and who love critical acumen better than personal dignity.

A. E. HEATH.

De Wijsbegeerte der Wiskunde van Thëistisch Standpunt. By D. H. TH. VOLLENHOVEN. [Pp. xvi + 447.] (Amsterdam: Wed. G. van Soest, 1918.)

IT is nowadays often recognised by philosophers that their work, if it is not to be mere mythology, must be based on scientific results, and proceed according to scientific method. The present Thesis for the degree of Doctor of Philosophy at the University of Amsterdam has the merit of realising this truth. For the purposes of a notice in a scientific journal, it is only necessary to draw attention to what may be called the substructure of Dr. Vollenhoven's essay. There is some likeness to the old-fashioned philosophies in the fact that the *first* part deals with *à priori* constructions of the possible solutions of the problems of the book; but the other parts will be found to contain much of interest to those who work at the problems of the universe by a scientific method. The second part is historical, and deals with empiricism, formalism, and intuitionism; in the last division there is an account of the work of, among others, Galileo, Descartes, Leibniz, Newton, and Kant, which, however, does not go into detail, and is not free from small inaccuracies of quotation (cf. pp. 106, 108). The third part treats non-Euclidean geometries, "the logic of relations" (in which the modern aspect of this subject is not touched), "the theory of objects" of Meinong and others, Cantor's introduction of the actually infinite, and the arithmetisation of geometry. The fourth part deals with critical attitudes—modern empiricism, modern formalism, and modern intuitionism. Modern formalists are represented by Mannoury and Bertrand Russell, and modern intuitionalists by Poincaré and L. E. J. Brouwer: Brouwer's work should be better known in Britain. There is a long and fairly satisfactory account of Mr. Russell's work and play, though the account is somewhat marred by failure to see jokes: thus, *one* part (only) of a skit on Mr. Russell's philosophy by the present writer is mentioned with apparent seriousness on p. 241; the character of the whole skit may be judged from the review which appears elsewhere in the present number of SCIENCE PROGRESS. The notices of the work of Frege (pp. 195, 224-9) and Peano (pp. 229, 278, 289) are much too slight to be of service.

PHILIP E. B. JOURDAIN.

The Theory of Measurements. By LUCIUS TUTTLE, B.A., M.D., Associate in Physics at Jefferson Medical College, Philadelphia. [Pp. xiv + 303.] (Philadelphia: Jefferson Laboratory of Physics, 1916. Price $1.25.)

"FOR the student of mathematics," says the author (p. v), "this book is intended to furnish an introduction to some of the applications of the exact sciences and their relation to the 'practical' sciences and useful arts, and is primarily intended to give him a knowledge of facts and methods, but without neglecting the accurate exercise of his reasoning powers. For the student of physical science it is intended especially to emphasise general considerations of measurement, theory of errors,

general methods of procedure, quantitative accuracy, adjustment of observations, etc.—topics that are often merely mentioned in the introduction or appendix of a laboratory manual, but that need laboratory work and drill quite as much as the measurements of the individual quantities that the student will take up in his later work." Again, "the object of a course in the theory of measurements is not only to give a certain knowledge of the scientific facts that are studied, but also to develop the thinking and reasoning powers and to furnish the special kind of mental training that results, in the first place, from practice in making various kinds of measurements with particular care for their accuracy, and, in the second place, from the consideration of accuracy in its quantitative aspects,—from realising that accuracy itself can be made a subject of measurement, that there are relative degrees of accuracy, that accuracy is important in one place and means only a waste of effort in another, that absolute accuracy is an impossibility, that a measurement by itself is of much less value than when accompanied by a statement of its precision." This is an admirable book, and it will be found very useful in a mathematical or physical laboratory.

After an introductory chapter, there are chapters on weights and measures, angles and circular functions (in which a "function" is defined on p. 47 to be a quantity which has a definite value for each particular value of the variable, so that apparently the inverse functions are not "functions"), significant figures (we read on p. 60 that "the figures of which a number is composed, except for one or more consecutive ciphers placed at its beginning or end for the purpose of locating the decimal point, are called its significant figures"), logarithms, small magnitudes which may be neglected in approximations, the slide rule, graphical representations by curves, curves and equations, graphic analysis, interpolation and extrapolation, coordinates in three dimensions,—including the construction of a contour map,—accuracy, the principle of coincidence, measurements and errors, statistical methods, deviation and dispersion, the weighting of observations, criteria of rejection, the method of least squares, indirect measurements, and systematic and constant errors.

PHILIP E. B. JOURDAIN.

ASTRONOMY

The Astronomical Observatories of Jai Singh. By G. R. KAYE. [Pp. viii + 153, with 27 plates and one map.] (Archæological Survey of India, New Imperial Series, vol. xi. Calcutta: Superintendent Government Printing, 1918. Price 23*s*.)

THE extremely interesting volume which Mr. G. R. Kaye has compiled on the astronomical observations of Jai Singh is the result of a tour of the observatories of Delhi, Jaipur, Ujjain, and Benares, which was arranged through the kindness of the Director-General of Archæology and the Educational Commissioner of the Government of India. The volume is primarily a tour report for the Archæological Department, and from the point of view of the purely astronomical reader it suffers somewhat from the restrictions necessarily imposed by this fact. The general reader, however, gains on account of the descriptive nature of the book.

Jai Singh, Maharaja of Jaipur, was born in A.D. 1686, the year in which Newton's *Principia* was completed, and eleven years after the founding of Greenwich Observatory. He succeeded to the Amber territory in 1699, and was later appointed Governor of the provinces of Agra and Malwa. He died in 1743 after a troubled reign, in a period when anarchic conditions prevailed. His

claims to remembrance are that "he was the founder of a new capital named after him, Jainagar or Jaipur, which in his time became a centre of learning; he erected caravanserais in many of the provinces; and he built astronomical observatories at five of the principal cities of Hindustan. He conceived and carried out a scheme of scientific research that is still a notable example" Jai Singh early showed a predilection for astronomical work, and studied earnestly the work of his predecessors and contemporaries, Hindu, Muslim, and European. He prepared a set of astronomical tables based on those of Ulugh Beg, of which Mr. Kaye gives an interesting description as well as copious extracts to show the manner in which the tables were drawn up. The preface to these tables, which Mr. Kaye gives in full, is of great interest, because the reasons which led Jai Singh to construct his peculiar masonry instruments are clearly stated.

Jai Singh concluded that the errors in observation which were the cause of the discrepancies between different catalogues were due partly to the smallness of the metal instruments, such as the astrolabe, which were then in vogue and which resulted in division errors and errors of reading, and partly to the shaking and wearing of pivots, displacement of the centres of the circles, etc. It is now recognised that instrumental errors are inevitable, and the modern astronomer does not attempt to eliminate them absolutely, but reduces them to a small magnitude, and then determines them separately with as much accuracy as possible. Jai Singh came to the erroneous conclusion, however, that these defects could be overcome by constructing "instruments of his own invention, such as *Jai Prakas* and *Rām Yantra* and *Samrāt Yantra*, the semi-diameter of which is of eighteen cubits, and one minute on it is a barley-corn and a half—of stone and lime of perfect stability, with attention to the rules of geometry and adjustment to the meridian and to the latitude of the place, and with care in the measuring and fixing of them, so that the inaccuracies from the shaking of the circles and the wearing of the axes and displacement of their centres, and the inequality of the minutes might be corrected."

The fundamental assumptions are inherently incorrect: they presuppose in theory that the only bar to accuracy of observation was the limit imposed by circumstances on the size of the instrument, and require in practice an absolutely accurate knowledge of the latitude and meridian, and no errors in construction. The errors in the observations made with them were not less than those in older observations. The basic ideas of the instruments were not peculiar to Jai Singh, although much ingenuity was shown by him in working them out. Mr. Kaye traces in an interesting manner their probable evolution. For the purpose of expounding the elementary fundamental ideas of astronomy they would serve admirably. Space does not permit of a description of the instruments; for these the reader should consult Mr. Kaye's book.

A detailed account is given of Hindu metal instruments, many of which are of exquisite workmanship. It was upon these that Jai Singh attempted to improve. A general account of the various instruments constructed by him is then given, and this is followed by detailed descriptions of the several observatories, with remarks as to the repairs, etc., necessary for their proper reconstruction and preservation. A general historical perspective follows, with an attempt to evaluate Jai Singh's work. Numerous useful tables are given at the end of the volume.

Mr. Kaye has written a book of great interest from the archæological, historical, and astronomical view-points. It is much to be hoped that the recommendations which he puts forward for putting the observatories into a proper condition, and

for preventing their ruin, will be carried out so that they may long serve as a record of an interesting phase in the history of Hindu astronomy. The numerous reproductions of metal and masonry instruments are excellent and deserve high praise.

H. S. J.

PHYSICS

Les Progrès de la Physique Moléculaire: Conférences faites en 1913-14 par Mme. P. Curie, J. Becquerel, M. de Broglie, A. Cotton, Ch. Fabry, P. Langevin, Ch. Mauguin, H. Mouton. [Pp. 244, with plates and figures.] (Paris: Gauthier-Villars et Cie., 1914.)

THIS volume is the second of the series of physical memoirs published by the Physical Society of France. Publication has been considerably delayed by the war, and although the volume is dated 1914 it has only recently appeared. It contains seven separate memoirs, each of which is in the nature of a review of recent progress in the branch of physics under discussion. The whole series taken together provide a useful summary of the most important advances in physics in the few years preceding the year 1914.

An excellent memoir on "La Physique du Discontinu" is contributed by P. Langevin. Perhaps the most important change in physical ideas during recent years has been the manner in which the fundamental notion of discontinuity has penetrated all its branches. This has necessitated a modification of the mathematical treatment of the subject: the differential and integral calculus inherently involve the conception of continuity, and have had to be replaced by the statistical methods and calculus of probabilities, introduced by Maxwell and developed by Boltzmann. In this article Langevin discusses several problems in probabilities and shows how various physical problems can be reduced to one or other of them. The physical illustrations are taken from many branches of physics, and many important results are derived. The memoir is written with admirable terseness and lucidity, and its publication in book form, so as to procure a much wider circulation, is much to be desired.

A somewhat brief memoir on "Les Progrès de nos Connaissances concernant les Rayons de Röntgen" is written by M. de Broglie. This deals mainly with the properties of secondary Röntgen rays and with the phenomena of diffraction of Röntgen rays. Considerable progress has naturally been made in these directions since the memoir was written.

M. Mauguin has given an account of the curious phenomenon of liquid crystals, and of their properties. He gives strong evidence as to the reality of their crystalline nature, and points out the interesting fact that they are all uniaxial. Hypotheses to account for this are discussed.

Mme. Curie has contributed a memoir on "Les Radio-éléments et leur Classification." Two important rules are advanced which govern the position of the radioactive elements in the periodic system. They may be combined thus: when a radioactive transformation takes place with (without) emission of α-rays, the resulting element whose atomic weight is four units less than (the same as) the parent substance, is displaced with reference to the latter by two (one) columns in the direction of decreasing (increasing) atomic weights. It is also shown that the radio-elements form groups of bodies with the same chemical and electrochemical properties, each group playing the rôle in the periodic system of a single element. The constitution of the various groups is then studied.

H. Mouton deals with the subject of "Biréfringence Magnétique des Liquides

purs. Anisotropie et Orientation des Molécules." The phenomenon and the manner of its variation with temperature, wave-length, etc., are summarised, and the theories proposed by Voigt, Havelock, and Langevin are discussed. The theory advanced by Mouton is that the molecules possess a double anisotropy: a magnetic anisotropy which causes their rotation in a magnetic field, and an optical anisotropy which produces double refraction. This theory accounts satisfactorily for all the phenomena, but the other theories are not yet decisively disproved.

A. Cotton discusses in an illuminating manner the evidence which it is possible to obtain as to the properties of symmetry possessed by molecules, and points out the directions in which there are possibilities of obtaining further evidence. His own contributions to this subject are important and well known.

The final memoir is on "Les Mouvements des Particules lumineuses dans les Gaz," by Ch. Fabry. The uncoordinated movements are first dealt with, and the information which can be deduced from the finite breadth of spectral lines is discussed. The well-known application of the theory to the Orion nebula, by MM. Bourget, Fabry, and Buisson, is referred to in illustration. The phenomena of fluorescence, and resonance of gases, and of diffusion of light by gases, are dealt with. The coordinated movements, such as canal and anode rays, are also discussed briefly.

H. S. J.

BOTANY

The Grasses and Grasslands of South Africa. By Prof. J. W. Bews, M.A., D.Sc. [Pp. iv + 161, with 1 map and 24 figures.] (Pietermaritzburg: P. Davis & Sons, Ltd. Price 7*s*. 6*d*. net, postage 6*d*.)

As the author states in his preface, this work has been written as a preliminary contribution to a big and important subject. From the wide range of aspects dealt with there is necessarily much that will require amplification as the result of further research, but Prof. Bews's presentation forms an excellent *résumé* of existing knowledge to which the author has himself contributed so much. Both the purely scientific and economic aspects receive due recognition, so that, whether the reader be practical farmer or botanist, their mutual dependence cannot fail to be appreciated.

For the assistance of students there is furnished an artificial key to the species and genera of South African grasses, which occupies some forty pages. The value of such a key can, of course, only be gauged by its actual employment in the field, but the almost entire absence of relative characters from this scheme eliminates one of the worst pitfalls for the inexperienced observer. The key is preceded by a glossary, the usefulness of which is, however, somewhat marred by the omission of most of the terms explained in the introduction. The third section is devoted to notes on the principal species of each genus dealing with their ecology, the gross anatomical features of their leaves, economic value, etc. Prof. Bews in the fourth section treats of the developmental phases of grassland and the part played by the Gramineæ in the various plant communities dominated by shrubs and trees. The subject-matter of this portion is subdivided according to regions corresponding to broad climatic and edaphic distinctions.

A particularly interesting feature to agriculturists and ecologists is the description of "substituted" types consequent upon burning or grazing. For example, in the transitional region between the Karroo and the Eastern grassveld the natural grassland is dominated by the Red Grass (*Anthistiria imberbis*), but burning or overstocking often results in the preponderance of far less valuable

odder grasses such as species of *Aristida* and *Eragrostis*. Again, in the Eastern grass-veld, characterised by a summer rainfall, the chief grasses are the Red Grass and Thatch Grass (*Andropogon hirtus*), but here too the same causes may prevent the complete succession, with the result that inferior pioneer species become dominant (*e.g.*, *Aristida junciformis*, *A. congesta*, *Sporobolus indicus*, and *Eragrostis* spp.).

In the concluding chapter, which deals with economic applications, the important effects of grazing, burning, overstocking, etc., and their influence on the composition of grassland, are considered. With regard to burning, the author emphasises the fact that the effect produced is dependent on the time of the year and the particular phase of succession reached when the operation is carried out.

An appendix furnishes a list of the English, Dutch, Zulu, and Sesuto names of the grasses with their corresponding Latin binomials.

To those interested in South African grasslands the work has its greatest value, but it also has a wider claim as emphasising the economic importance of ecological investigations.

E. J. SALISBURY.

The Botany of Crop Plants. By WILFRED W. ROBBINS, PH.D., Professor of Botany, Colorado Agricultural College. [Pp. xx + 681, with 263 figures in the text.] (Philadelphia: P. Blakiston's Son & Co., 1917. Price $2.00 net.)

THIS work is intended to give the botanical student both in agricultural and non-agricultural schools a presentation of the botany of the common crop plants, including those of the garden and orchard. It consists of two parts, the first of 67 pages giving a general outline of the morphology of the flowering plant, and the second, which comprises the greater part of the book, consisting of descriptions of the various species of crop plants.

The object of Part I. is to refresh the student's acquaintance with the fundamental facts of plant structure, or to serve as a preparation for Part II. The author states in his preface that in many institutions Part II. will be preceded by a general course in the principles of botany which will give the student a survey of the plant kingdom, in which case Part I. will be omitted. It is to be hoped that this will very generally be the case, for Part I. only deals with the flowering plant, and it is very desirable that students, as a rule, should have a more general knowledge of the plant kingdom than that provided by Part I. before turning to the specialisation involved in the study of the subject matter of Part II.

Part II. has been written especially to meet the requirements of North American students, and does not by any means deal with all crop plants. Nevertheless, one cannot fail to be impressed by the large number of species with which the author deals. Certainly the great majority, and perhaps all, British crop plants find a place here, and the book will be useful on this side of the Atlantic for this reason, as well as for the information it gives of species which are not cultivated here. As examples of these latter may be mentioned the species of Sorghum, which are favourite objects of research in America, but with which most British readers are unacquainted.

In the description of the various species of crop plants the morphological features are adequately dealt with, although in many cases more might have been made of the physiological and economic aspects of the species.

The book is well illustrated, while the list of references to literature at the end of each chapter should prove useful to those desiring further information on

particular species. The index is good, extending as it does to twenty-nine pages with double columns; even so one looks in vain in it for such a well-known name as Lucerne, although over the description of this plant (*Medicago sativa*) the British name is given as an alternative to the American name Alfalfa.

The growing interest in and importance of the economic aspects of botany, together with the reasonable price of the book, make it very welcome at this time.

W. S.

Fungi and Disease in Plants. An Introduction to the Diseases of Field and Plantation Crops, especially those of India and the East. By E. J. BUTLER, M.B., F.L.S. [Pp. vi + 547, with 5 coloured plates and 205 other figures.] (Calcutta and Simla: Thacker, Spink & Co., 1918. Price Rs. 15 = 20*s*.)

MR. BUTLER as Imperial Mycologist at the Agricultural Research Institute at Pusa has had exceptional opportunities for studying parasitic fungi, especially from the economic standpoint, and it need scarcely be said that the present work, whilst primarily supplying a great need of the Indian student and grower, has more than local value. It is indeed a fact that not a few of the diseases prevalent in the temperate regions are caused by organisms specifically identical with those of Europe.

Rather more than a quarter of the text is devoted to general topics such as the diverse types of parasitic fungi, their habits and life history, and their relation to disease and control. Special mention may be made of those sections dealing with specialisation of parasitism and the relations of host to parasite, both of which are useful summaries of somewhat neglected but economically important aspects. The behaviour of specialised races has for the grower a significance that can scarcely be overestimated. The occurrence of "bridging" species, such as barley for the rust of wheat to oats, may be commoner than is supposed, and the possibility of wild species and especially of natural hybrids playing this rôle must not be overlooked.

The effect of environment on immunity is a subject with which the author also deals. Observations have shown that resistance to disease may vary with the local conditions of soil and climate which probably react upon the anatomical structure or physiological condition of the plant. It is suggested that several features primarily regarded as transpiration checks, by rendering the lodgment of spores difficult or impeding penetration, probably play an important part in decreasing susceptibility to disease As such characters are extremely plastic, this may explain in part the marked differences, as regards immunity, of the same variety when grown in different localities.

Though in many cases the only method of control for these fungal pests is by the selection of immune strains, the effectiveness of the excellent work which has been done in this direction by breeders is largely defeated by our ignorance as to the causes of deterioration and the environmental factors which tend toward increased resistance.

None the less valuable, though less interesting to the general reader, is the second part, where some two hundred diseases are described which attack the field and plantation crops of India. These are arranged in reference to the host plants, which are grouped under such headings as Cereals; Pulse crops; Dye, Drug, and Spice crops, etc.

The diseases of the Sugar Cane, Tea, Rubber, and Coffee receive a chapter

each, both on account of the number of parasites concerned and the economic importance of these plants.

For each species of parasite the writer describes its distribution, the symptoms of the disease, the life history and methods of treatment.

The work concludes with a bibliography of some six hundred references classified under the chapter headings.

The production of a work treating especially of the plant diseases of India is the more welcome in view of the large number of indigenous fungi which produce them, and we shall look forward to a further volume in which the parasites of the forest trees are to be considered with pleasurable anticipation.

It may be added that the printing and illustrations are excellent, whilst the index is far superior to that of most books of this character.

E. J. SALISBURY.

I. **Plant Œcology and its Bearing on Problems of Economic Importance in India.** By R. S. HOLE, F.C.H., F.L.S., F.E.S. [Pp. clvi–clxvii and Plates V.–X. *Jour. and Proc. Asiatic Soc. Bengal*, Vol. XIV., 1918.]

II. **Recent Investigations on Soil Aeration, with Special Reference to Agriculture and Forestry.** By A. HOWARD and R. S. HOLE. [Pp. 415–40, Plates XXIV.–XXIX., and Figs. 1–6. *Agric. Jour. of India*, Vol. XIII., July 1918.]

THE value of œcological methods of investigation as a means of solving economic problems is only just beginning to be realised, but, having regard to the infancy of this branch of botany, the results already achieved are the earnest of its coming importance for practical agriculture and forestry.

Mr. Hole, in his Presidential Address before the Asiatic Society of Bengal, performed a distinct service in calling attention to the part that œcology and œcological methods have already played in solving sylvicultural and other economic problems in India, and it is to be hoped that the authorities are fully alive to the need for both moral and material encouragement for research on these lines which the Indian Forest Service is so eminently fitted to undertake.

An excellent example of such work, carried out by the author, is that concerned with the reproduction of Sal (*Shorea robusta*), dealt with in Part II. of the second-named paper. The natural regeneration of the forests of this valuable timber tree is often either delayed or even almost inhibited by deficient soil-aeration. As a consequence of the latter, root development is retarded, so that with the advent of the dry season from September to June the root systems of the seedlings have not reached a sufficient depth to withstand the drought and a high mortality ensues. The results of œcological experiments have not only established these facts, but have also shown that the ill effects can be remedied by burning the raw humus and by clear-felling in small patches.

The attacks of *Polyporus shoreæ* appear to be particularly associated with the same adverse conditions, and the soil-aeration factor is probably also of first importance in the regeneration of many other timber trees of India. Mr. Howard, in the second paper, after summarising some of the recent work on this subject, suggests that soil aeration may be largely responsible for the differences in quality of barley, tobacco, and cotton.

Mr. Hole (I.) emphasises the importance of the study of natural plant communities as revealing conditions of the habitat, and thus indicating the most suitable methods and species for a given locality; a sylvicultural application of

Syn-œcology, which the reviewer has laid stress on in relation to British afforestation, and which is evident too from a perusal of Dr. Bews' recent work on South African Grasslands. The author also points out that the *intensive* methods of the œcologist are a necessary complement to the *extensive*, and, we may add, often empirical, methods of the sylviculturist.

In the tropics no less than in Europe the great need of the grower is a definite knowledge of habitat factors and especially of their complex interaction, without which empirical skill can make no great advance. This knowledge can, however, only be acquired by an increased number of workers and augmented expenditure of public funds together with the recognition that the bread thus cast upon the waters may not return for many days, though return it surely will in manifold measure.

E. J. SALISBURY.

ZOOLOGY

A Textbook of Biology for Students in General, Medical, and Technical Courses. By PROF. W. M. SMALLWOOD, PH D. [Pp. xiv + 317, with 261 engravings and 10 plates in colour and monochrome.] (New York: Lea & Febiger, 1916. Price $2.75.)

THIS book, as its extended title indicates, is intended to be a general introductory course for students proceeding to more specialised lines of study, and contains a great deal of useful information. The general portion of the book treats of animals from both the physiological and morphological points of view, and also of embryology, cytology, and histology. Further chapters treat of the bacteria, moulds, and fungi, and the classification of plants and animals. Its method of dealing with these subjects is naturally adapted to American university courses and is not well suited to the requirements of British universities. The second half of the volume, however, deals with aspects of biology that are only too often omitted from textbooks, *e.g.* Biological Factors in Disease, Adaptations and Animal Behaviour and its Relation to the Mind, etc. These chapters contain much that is not usually accessible to the student, but which are, nevertheless, of considerable interest and importance in the application of biology to the various aspects of human activities. The book is well illustrated and contains a good bibliography and glossary.

C. H. O'D.

Medical Contributions to the Study of Evolution. By J. G ADAMI, M.D., F.R.S., F.R.C.P. [Pp. xviii + 372, with 7 plates and 18 figures in the text.] (London: Duckworth & Co., 1918. Price 18*s*. net.)

IN this book are collected a number of short studies previously published by the author in various places from 1892 onwards, and re-issued in the one volume because many of them have more or less direct bearings on certain biological problems. It is divided into three parts: Part I. contains the Croonian Lectures on "Adaptation and Disease," delivered to the Royal College of Physicians in 1917; Part II. consists of articles related to "Heredity and Adaptation"; and a series of more strictly medical studies on "Growth and Overgrowth" constitutes Part III. They naturally have not any great unity either of aim or treatment, but will perhaps be accessible to a somewhat wider audience than if they had been left scattered through the back files of different journals.

The first part is in some ways the most interesting, since although, as the author points out, it contains very little new matter, it is to a certain extent a *résumé* of his previous conclusions and a general summary of his present position.

Exactly why such an occasion should have been chosen for a restatement might have been somewhat of a puzzle to the general reader had not the author in his preface and again in the first lecture given a very full explanation. It appears that in the course of a private conversation with a British zoologist certain differences of opinion arose, and hence the Croonian Lectures were chosen as a means of reading a severe admonition to the "Academic Biologist" on his failure to appreciate the work of the medical man. It is to be feared that the reader of this book will have no clearer idea of exactly what is meant by an "Academic Biologist" than the author himself seems to have. This action itself is, in our opinion, in bad taste, and when the preface and appendix proceed to enlarge upon it and give the personal aspect of the quarrel, even to the extent of mentioning names, we feel that it has set an example of controversial methods unworthy of a scientific man, and has detracted considerably from what is, in many ways, an interesting and vigorous volume.

The lectures in themselves show some signs of being hurriedly and loosely put together: for example, we find "katalyst" on one page becomes "catalyst" two pages later, and "toxines" on one line appears as "toxins" on the next. These are small matters in themselves but indicative of a careless style that leads to clumsy sentences, as, for example, at the beginning of the last paragraph on page 93, and a failure to make a sentence at all near the top of page 65. The author, as he himself admits, in his lecture made the mistake of using the word "variability" for "variation," and also allowed it to stand when the lecture appeared in print, apparently not noticing it until it was pointed out, and this too in what the author two lines further on regards as the "basal problem of evolution." When we come to examine his arguments also, certain criticisms are obvious. In spite of his quotation from *The Principles of Biology*, it is still not apparent that he uses the term "direct adaptation" in the same sense that many people consider it was used by Spencer.

In discussing the acquisition of immunity to certain phytotoxins by rabbits and mice, this power is regarded as a "perfectly clear-cut example of direct individual adaptation," and is taken as fulfilling the following essential conditions (*inter alia*): 1. "We deal with the acquirement of a new property; the acquirement cannot possibly be regarded as the calling into activity of a property previously possessed, either by the individual or by the species: this power to neutralise ricin is something absolutely new." 3. "There can be no possibility of ascribing the new property to the persistence of a chance variation: the power to develop antiricin and discharge it into the blood can be produced by any mouse or rabbit with absolute certainty."

Now, surely if such a chemical or physiological response can be obtained "with absolute certainty" from "any mouse or rabbit" we cannot possibly be dealing with "something absolutely new"—the production of a property not "previously possessed." The third condition therefore to a certain extent contradicts the first, and if such a power is exhibited by all members of the species under the same conditions, it is a misuse of words to say that we are dealing with the development of something new—a new character. The only thing that is new is the demonstration that under certain conditions *all* mice and *all* rabbits have the power to exhibit a biochemical reaction. If all members of both species show precisely the same power, then it is obvious that it is a potentiality or character present in the species all the time, but only made manifest under the abnormal conditions of the experiment. In no way can it be regarded as "something positive, something additional," to use the words of condition 2; it is simply the

making obvious of something potentially present in the species but not usually noticed.

The same line of criticism applies to another set of conclusions dealing with the similar modification of "all the microbes subjected to particular orders of alterations of environment" (p. 45), but lack of space forbids entering into a more detailed discussion of all the points raised, and the particular ones chosen are some of the fundamental arguments put forward.

In spite of these criticisms and of the regrettable tone of the preface and Appendix II. there is a great deal to stimulate thought and interest in the volume. It can be read with profit by the biologist, who, in some cases, may perhaps deserve its strictures, and by the practising medical man, whose lack of time renders it hard for him to keep abreast of the theoretical aspects of modern pathology and bacteriology and their inter-relation with biological problems.

C. H. O'D.

The Portal of Evolution; being a Glance through the Open Portal of Evolution at some of the Mysteries of Nature. By a Fellow of the Geological and Zoological Societies. [Pp. xvi + 295, with 4 tables.] (London: Heath Cranton Limited, 1918. Price 16*s.* net.)

THE author's aim in writing the present volume is indeed high, for he tells us in his apologetic introduction that "I wish it to be thoroughly understood that the object of this treatise is not to weaken any one's faith in their own religion, but rather to turn their own, and every other religion, to a better use." He is strongly convinced of the seriousness of his undertaking, which he looks upon in the nature of a revelation. "It is a striking illustration of the way God controls the actions of one and all of us to make them of use to the community as a whole, that at the same time, as at the bottom of the tree of scientific knowledge, my humble self has been elected as the insignificant means to elucidate in the crudest manner possible some of the mighty links that assist us to connect divine and human nature together by the extraordinary revelation my hypothesis contributes to the connection of the Creator and the created in the mystery of His trinity."

The unknown author, an Australian, is read fairly widely in scientific literature, and his sincerity and earnestness are obvious throughout, as is also his effort to deal with the subject in a broad and tolerant manner. In spite of this, however, we must confess that we have been utterly unable to make out what it is all about. This we have to admit after trying to read the book in the same serious spirit in which it is written. The book was read and then laid aside for some weeks and then read again, but in spite of this it simply appears to be a welter of science, metaphysics, philosophy, but mostly religion, faith, and words from which we could extract no coherence. It may be that there are some to whom these "revelations" will bring light, but we must record their failure to do so to ourselves.

C. H. O'D.

A Check List of North American Amphibians and Reptiles. By LEONARD STEJNEGER and THOMAS BARBOUR. [Pp. iv + 125.] (Cambridge, U.S.A.: Harvard University Press, 1917. Price 10*s.* 6*d.*)

IT is difficult to appraise the value of a check list until it has been used a good deal, but there is no doubt that such a book dealing with North American forms has long been needed. The wants of the students of herpetology will be met by this volume, as are those of the ornithologist by the check list of the American

Ornithologists' Union which has, to a certain extent, served as a guide in the preparation of the present volume. The names of the two authors, who are well-known authorities on North American herpetology, are a sufficient guarantee of the recentness and reliability of the matter contained in the book. We should like to point out, however, that from the point of view of the zoologist, who is not familiar with the details of the classification of these two classes, a table of contents and an index similar to those provided in the A.O.U. check list would considerably simplify its use in looking up references. It only remains to be noted that the printing and general "get up" of the book are of that excellent quality that we have learned to associate with the Harvard University Press.

C. H. O'D.

MISCELLANEOUS

Crime and Criminals. Being the Jurisprudence of Crime, Medical, Biological, and Psychological. By CHARLES MERCIER, M.D., F.R.C.P., F.R.C.S. With an Introduction by SIR BRYAN DONKIN, M.A., M.D., F.R.C.P. [Pp. xvii + 291.] (London: University of London Press, 1918. Price 10*s*. 6*d*. net.)

THE appearance of a new work on Crime by the author of *Criminal Responsibility* and *Crime and Insanity* is an event of very wide interest, both to criminologists and to the general public. Dr. Mercier unites a specialist's knowledge of the subject with a breadth of view not often found in specialists, and with an originality of thought and independence of authority not often found anywhere.

The present work is a natural sequel to his work on *Conduct*, and to his writings on Logic. Dr. Mercier classifies crime into the two primary divisions of those aimed against "the very principle of society" (international crimes), and those that injure a single state (national crimes). The former include piracy, filibustering, brigandage, and anarchy. The latter are subdivided into those directed against the state (public crimes) and those directed against individuals (private crimes). These with their further subdivisions are considered in detail.

In dealing with the factors which conduce to criminality, Dr. Mercier repudiates all theories that the criminal has any fundamental difference from the ordinary man. This theory, carried to so extreme a length by Lombroso, has indeed largely gone out of fashion. "Every man," says Dr. Mercier, "is a potential criminal." The perpetration of a crime is consequent upon a temptation which exceeds the resisting-power or "breaking point" of the individual. Every one has his "breaking point," though for a saint the degree of temptation required to attain it will be exceedingly high, and in a habitual criminal low. Hence Dr. Mercier would prevent crime, partly by raising moral tone, partly by removal of temptation. He favours regulations, for instance, to prevent shop-keepers from exposing their wares in such a way as to offer temptations to theft by passers-by. This seems to savour somewhat of moral coddling. If the individual is protected too much from all the temptations in life, is there not a danger of moral softness resulting? *On n'acquiert pas les qualités dont on peut se passer;* moral as well as physical strength comes only to those who have had the opportunity of exercising these faculties.

Sir Bryan Donkin supplies an admirable introduction to this important book. If it is true that Dr. Mercier needs no introduction, it is no less the case that an introduction by Sir Bryan lends great additional weight and authority to any book on the subject of crime.

We are pleased to note that the Swiney Prize was at once awarded to this book. We warmly congratulate Dr. Mercier on this well-deserved honour, which he has now won for the second time.

HUGH ELLIOT.

On the Mechanical Theory of the Vibrations of Bowed Strings and of Musical Instruments of the Violin Family, with Experimental Verification of the Results: Part I. By C. V. RAMAN, M.A. 'Bulletin No. 15, The Indian Association for the Cultivation of Science. [Pp. iii + 158.] (Calcutta: 1918. Price 3*s.* 4*d.* net)

HELMHOLTZ on an experimental basis was able to construct a partial theory of the bowed string. F. Krigar Menzel and A. Raps photographed, upon a revolving drum carrying a film, various points of bowed strings so as to exhibit their displacement-time graphs. E. H. Barton and his pupils took simultaneous photographs of the behaviour of the strings and either bridge, belly, or air of a monochord or violin. But in none of the foregoing cases was a direct mechanical theory of the string, bridge, etc., attempted. This is now done by C. V. Raman.

The equations of motion of the string are written and solved for the case of a periodic force applied transversely by the bow at any given position. The equations of motion of the bridge are next written and dealt with. The *modus operandi* of the bow is afterwards examined and a simplified kinematical theory of the bowed string is based upon it. This leads to a number of types of vibration—two-step, three-step, etc., zig-zag motions appearing in the corresponding graphs.

Another interesting subject here treated is that of the effect of the *mute*, which, by loading the bridge, enfeebles and veils the tone of the instrument. For the purpose of these tests, loads were placed at various positions on the bridge and simultaneous curves obtained photographically of the bridge and of each of the strings in turn. The instructive results so obtained are given in two plates.

Photographic reproductions are also given of simultaneous vibration curves of the belly and G-string of a violoncello when played at and near the "wolf-note" pitch, showing alternate cyclical variations of amplitude.

Other points dealt with are the effects of the variation of pressure and speed of bowing and the relation between them, the width of the bowed place, the yielding of the bridge, and the vibrations obtained from a 'cello by plucking the strings.

This work contains twenty-eight figures in the text and twenty-six excellent full-page photographic reproductions, and well deserves the careful attention of those interested in such a notable contribution to an important subject.

E. H. B.

Life and Letters of Joseph Black, M.D. By Sir WILLIAM RAMSAY, K.C.B., F.R.S., with an Introduction by F. G. DONNAN, F.R.S. [Pp. xix + 148.] (London: Constable & Co., 1918. Price 6*s.* 6*d.* net.)

TWO books in one. A brief memoir of Sir William Ramsay, and a longer account of the life of Joseph Black. The latter is an instance of the truth that only a busy man can find time to do extra work. Sir William Ramsay was a very busy man, but he could find time to write this excellent biography of Joseph Black, which is also a history of an important period in chemical discovery, and could not have

been fruitfully undertaken by any one but a chemist. The book is full of interest for chemists and for all who are interested in the history of science.

C. A. M.

Dr. John Radcliffe: a Sketch of his Life, with an Account of His Fellows and Foundations. By I. B. NIAS, M.D., M.R.C.P., Radcliffe Travelling Fellow. [Pp. 147. Illus.] (Oxford: at the Clarendon Press, 1918. Price 12*s.* 6*d.*)

THIRTY pages are occupied by an account of Dr. Radcliffe's life, sixty by records of the gentlemanly mediocrities who have held the Fellowships he founded, and the remainder of the book by accounts of the other institutions that bear his name and owe their existence to his munificence. John Radcliffe, while not a great man, was a very successful man and a very public-spirited man. He left directions and funds for the foundation of the travelling Fellowships that bear his name, and of the library that is such a conspicuous feature among Oxford institutions, and he wisely left the surplus of his estate, as surplus should accrue, to be administered by his trustees in their discretion for charitable purposes. It is to the wise exercise of this discretion that Oxford owes the Radcliffe Infirmary and the Radcliffe Observatory.

The book does great credit to the industry of Dr. Nias, is printed in a semi-antique type, and is admirably illustrated by reproductions of old prints; altogether an attractive volume.

C. A. M.

The Twin Ideals: an Educated Commonwealth. By Sir JAMES BARRETT, K.B.E., C.M.G., M.D. In Two Volumes. [Pp., Vol. I., xxxii. + 512; Vol. II., xx + 504] (London: H. K. Lewis & Co., 1918.)

SIXTY-TWO newspaper articles that have appeared in various English and Australian newspapers during the last twelve years, grouped under various headings, which indicate the wide variety of subjects of which they treat: Milk and Neglected Children, Town Planning and Playgrounds, Rural Life, National Parks, Music, etc. Sir James Barrett has definite opinions on all these subjects, and expresses them all, but some of his articles are of only local interest, and many are of only ephemeral interest. In short, they are very good newspaper articles.

C. A. M.

National Reconstruction: a Study in Practical Politics and Statesmanship. By J. J. ROBINSON. [Pp. x + 155.] (London: Hurst & Blackett, 1918. Price 2*s.* 6*d.* net.)

THE title is a misnomer. It should be "Misty Generalities: a Study in Saying the Least Possible in the Largest Number of Words."

C. A. M.

The Royal Navy, 1815-1915: The Rede Lecture, 1918. By Admiral the MARQUESS OF MILFORD HAVEN, P.C., G.C.B., LL.D., etc. [Pp. 48.] (Cambridge: at the University Press, 1918. Price 2*s.* 6*d.* net.)

A PLEASANTLY written little summary of naval progress, superficial, but interesting as far as it goes.

C. A. M.

BOOKS RECEIVED

(Publishers are requested to notify prices)

A Treatise on Gyrostatics. By Andrew Gray, F.R.S., Professor of Natural Philosophy in the University of Glasgow. London: Macmillan & Co., St. Martin's Street, 1918. (Pp. xx + 530.) Price 42*s*. net.

An Introduction to the Algebra of Quantics. By Edwin Bailey Elliot, M.A., F.R.S., Waynflete Professor of Purse Mathematics and Fellow of Magdalen College, Oxford. Second Edition. Oxford: at the Clarendon Press, 1913. (Pp. xvi + 416.) Price 15*s*. net.

A Star Atlas and Telescopic Handbook (Epoch 1920). For Students and Amateurs. Covering the whole Star Sphere, and showing over 7,000 Stars, Nebulæ, and Clusters, with Short Descriptive Lists of Objects suitable for Small Telescopes, Notes on Planets, Star Nomenclature, etc. By Arthur P. Norton, B.A. London: Gall & Inglis, 31, Henrietta Street, Strand, W.C., and Edinburgh, 1919. (Pp. 25.)

A Treatise on the Sun's Radiation and other Solar Phenomena. In Continuation of the Meteorological Treatise on Atmospheric Circulation and Radiation, 1915. By Frank H. Bigelow, M.A., L.H.D., Professor of Meteorology in the U.S. Weather Bureau, 1891-1910, and in the Argentine Meteorological Office since 1910. New York: John Wiley & Sons; London: Chapman & Hall, 1918. (Pp. ix + 385.) Price 23*s*. net.

The New Science of the Fundamental Physics. By W. W. Strong, B.S., Ph.D. S.I.E.M. Co., Mechanicsburg, Pa., U.S.A., 1918. (Pp. xi + 107.) Price $1.25.

The Theory of the Relativity of Motion. By Richard C. Tolman, P.D. Berkeley: University of California Press, 1918. (Pp. ix + 225.)

Every Man's Chemistry: The Chemist's Point of View and his Recent Work told for the Layman. By Ellwood Hendrick. London: University of London Press, at St. Paul's House, Warwick Square, E.C., 1918. (Pp. x + 319) Price 8*s*. 6*d*. net.

Technical Handbook of Oils, Fats, and Waxes. By Percival J. Fryer, F.I.C., F.C.S., Chief Chemist and Director, Yalding Manufactory Co., Lecturer in Oils, Fats, and Waxes at The Polytechnic, Regent Street, W., and Frank E. Weston, B.Sc., F.I.C., Head of the Chemistry Department, The Polytechnic, Regent Street, W. Volume II., Practical and Analytical. Cambridge: at the University Press, 1918. (Pp. xvi + 314, with 69 illustrations.)

Recent Advances in Organic Chemistry. By Alfred W. Stewart, D.Sc., Lecturer on Physical Chemistry and Radioactivity in the University of Glasgow, formerly 1851 Exhibition Research Scholar and Carnegie Research Fellow. With an Introduction by J. Norman Collie, LL.D., F.R.S., Professor of

Organic Chemistry and Director of the Chemical Laboratories in University College, London. Third Edition. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp xix + 350.) Price 17*s.* net.

Recent Advances in Physical and Inorganic Chemistry. By Alfied W. Stewart, B.Sc., Lecturer on Physical Chemistry and Radioactivity in the University of Glasgow. With an Introduction by Sir William Ramsay, K.C.B., F.R.S. Third Edition. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1919. (Pp. xv + 284, with 23 illustrations.) Price 12*s.* 6*d.* net.

The Natural Organic Colouring Matters. By Arthur George Perkin, F.R.S., F.R.S.E., F.I C., Professor of Colour Chemistry and Dyeing in the University of Leeds, and Arthur Ernest Everest, D.Sc., Ph.D., F.I.C., of the Wilton Research Laboratories, late Head of the Department of Coal-Tar Colour Chemistry, Technical College, Huddersfield. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. xxii + 655.) Price 28*s.* net.

Surface Tension and Surface Energy and their Influence on Chemical Phenomena. By R. S. Willows, M.A., D.Sc., and E. Hatschek. Second Edition. London: J. & A. Churchill, 7, Great Marlborough Street, 1919. (Pp. viii + 115, with 21 illustrations.) Price 4*s.* 6*d.* net.

A System of Physical Chemistry. By William C. McC. Lewis, M.A., D.Sc., Professor of Physical Chemistry in the University of Liverpool. Second Edition. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1916. (Pp. Vol. I. xi + 523, Vol. II. vii + 552.) Price 9*s.* net each volume.

Catalysis in Industrial Chemistry. By G. G. Henderson, M.A., D.Sc, LL.D., F.R.S., "Freeland" Professor of Chemistry, The Royal Technical College, Glasgow. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1919. (Pp. ix + 202.) Price 9*s.* net.

Catalytic Hydrogenation and Reduction. By Edward B. Maxted, Ph.D., B.Sc., F.C.S. London: J. & A. Churchill, 7, Great Marlborough Street, 1919. (Pp. viii + 104, with 12 illustrations.) Price 4*s.* 6*d.* net.

Tables of Refractive Indices. Vol. I. Essential Oils. Compiled by R. Kanthack, edited by J. N. Goldsmith, Ph.D., M.Sc., F.I.C. London: Adam Hilger, 75A, Camden Road, N.W.1, 1918. (Pp. vi + 148.)

A Manual of Geometrical Crystallography: Treating Solely of those Portions of the Subject useful in the Identification of Minerals. By G. Montague Butler, E.M. Professor of Mineralogy and Petrology, Dean, College of Mines and Engineering, University of Arizona, Tucson, Arizona. New York: John Wiley & Son; London: Chapman & Hall, 1918. (Pp. viii + 155.) Price 7*s.* net.

Genetics Laboratory Manual. By E. B. Babcock, Professor of Genetics, University of California, and J. L. Collins, Instructor of Genetics, University of California. Agricultural Publications: Charles V. Piper, Consulting Editor. New York: McGraw-Hill Book Co.; London: Hill Publishing Co., 1918. (Pp. xi + 56.)

Co-operation in Danish Agriculture. By Harald Faber. An English Adaptation of Andelsbevœgelsen i Danmark, by H. Hertel. With a Foreword by E. J. Russell, D.Sc., F.R.S., Director of the Rothamsted Experimental Station, Harpenden. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp xxii + 176.) Price 8*s*. 6*d*. net.

Agriculture in Oxfordshire. A Survey made on Behalf of the Institute for Research in Agricultural Economics, University of Oxford. By John Orr, with a Chapter on Soils by C. G. T. Morison. Oxford: at the Clarendon Press, 1916. (Pp xii + 239) Price 8*s*. 6*d*. net.

Forced Movements, Tropisms, and Animal Conduct. By Jacques Loeb, M.A., Ph.D., Sc.D., Member of the Rockefeller Institute for Medical Research. Philadelphia and London: J. B. Lippincott Company. (Pp. 209.) Price $2.50 net.

The Evolution of the Earth and its Inhabitants. A Series of Lectures delivered before the Jule Chapter of the Sigma XI. during the Academic Year 1916–17, by Joseph Barrell, Charles Suchert, Lorande Loss Woodruff, Richard Swann Lull, Ellsworth Huntington. New Haven: Yale University Press; London: Oxford University Press, 1918. (Pp. xi + 208, with 38 figures in the text.) Price 10*s*. 6*d*. net.

The Origin and Evolution of Life on the Theory of Action, Reaction, and Interaction of Energy. By Henry Fairfield Osborn, Sc.D. Princeton, Hon. LL.D. Trinity, Princeton, Columbia, Hon. D.Sc. Cambridge, Hon. Ph.D. Christiania. London: G. Bell & Sons, 1918. (Pp. xxxi + 332, with 111 illustrations.) Price 25*s*. net.

Intravenous Injection in Wound Shock. Being the Oliver-Sharpey Lectures delivered before the Royal College of Physicians of London in May 1918. By W. M. Bayliss, M.A., D.Sc., F.R.S., Professor of General Physiology in University College, London. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1918. (Pp. xi + 172.) Price 9*s*. net.

Ellis's Demonstrations of Anatomy. Being a guide to the Knowledge of the Human Body by Dissection. Revised and Edited by Christopher Addison, M.D., B.S., F.R.C.S., Lecturer on Anatomy, Charing Cross Hospital Medical School, formerly Hunterian Professor, Royal College of Surgeons. Twelfth Edition. London: John Murray, Albemarle Street, W., 1905. (Pp. x + 851.)

The Australian Army Medical Corps in Egypt. An Illustrated and Detailed Account of the Early Organisation and Work of the Australian Medical Units in Egypt in 1914-1915. By Lieut.-Col. James W. Barrett, C.M.G., M.D., M.S., F.R.C.S., and Lieut. P. E. Deane, A.A.M.C. London: H. K. Lewis & Co., 136, Gower Street, W.C.1. (Pp. xiv + 259, with 37 illustrations.)

The Question: If a Man Die shall He Live Again? A Brief History and Examination of Modern Spiritualism. By Edward Clodd. With a Postscript by Prof. H. E. Armstrong, F.R.S. London: Grant Richards, St. Martin's Street, 1918. (Pp. 314.) Price 10*s*. 6*d*. net.

War and Civilisation. A Lecture delivered at the John Rylands Library on February 13, 1918. By W. J. Perry, B.A. Reprinted from *The Bulletin of the John Rylands Library*, Vol. IV., Nos. 3-4, February-July 1918. Manchester: at the University Press. London: Longmans, Green & Co. 1917-18. (Pp. 27.) Price 1*s*. 6*d*. net.

The Adventure of Life. By Robert W. Mackenna, M.A., M.D. London: John Murray, Albemarle Street, W., 1919. (Pp. xiii + 306.) Price 6*s*. net.

ANNOUNCEMENT

THE King has graciously consented to act as President of the British Scientific Products Exhibition, 1919, which will be held at the Central Hall, Westminster, during the month of July. The President of the Exhibition is the Marquis of Crewe, K.G., and Prof. R. A. Gregory is Chairman of the Organising Committee.

The British Science Guild has been encouraged to organise this Exhibition by the success which attended that held at King's College last summer and the more recent Exhibition at Manchester. Now that many inventions can be shown which could not be put before the public during the war, there is every prospect that this year's Exhibition will be even more successful than its predecessors.

The Exhibition will include sections dealing with Chemistry, Metallurgy, Physics, Agriculture and Foods, Mechanical and Electrical Engineering, Education, Paper, Illustration and Typography, Medicine and Surgery, Fuels, Aircraft and Textiles. Firms desirous of exhibiting are invited to communicate with the Organising Secretary, Mr. F. S. Spiers, 82, Victoria Street, London, S.W.1.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.